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Incentive Pay in the United States: Its Determinants and Its Effects

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Incentive Pay in the United States: Its Determinants and Its Effects[*]

Daniel Parent[†]

Résumé / Abstract

L'objectif poursuivi dans cet article est de faire un survol des travaux empiriques effectués relativement à l'utilisation des méthodes de rémunération incitative aux États-Unis. Deux questions font l'objet d'une attention particulière : quels sont les facteurs expliquant l'utilisation de ces méthodes de rémunération et quels en sont les effets réellement incitatifs. Les principaux modèles théoriques sous-jacents sont également discutés afin de fournir une toile de fond.

The main objective of this article is to provide a survey of the empirical work done on the use of incentive pay in the United States focusing on two main empirical questions: what are the determinants of their use and what are the effects of pay-per-performance schemes, if any. Theoretical models are also discussed so as to provide a background to the reported evidence.

Mots Clés : Incitations, forme des contrats, États-Unis

Keywords: Incentives, contract form, United States

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1 Introduction

In view of considerations of effort provision by workers and of using explicit incentive contracts such as piece rates to allow individuals to reveal their productivity through their choice of pay method, it would seem that compensation contracts based on objective measures of performance should be the overwhelming choice by firms.

However, put simply, this is just not what we observe in the United States. Although not rare, compensation contracts based on explicit measures of individual performance are the exceptions more than the rule. Over the last twenty years, much of the theoretical literature devoted to the analysis of the employment relationship and to the provision of incentives has tried to rationalize that simple fact. For example, it has been argued that piece rates are not very prominent as an effort incentive device for reasons having to do with the difficulty of measuring individual output or the importance of teamwork where cohesion and cooperation between team members is important.

The main objective of this chapter will be to provide a survey of the empirical work done on the use of incentive pay in the United States focusing on two main empirical questions: what are the determinants of their use and what are the effects of pay-for-performance schemes, if any. To avoid simply enumerating the empirical results found in the literature without having any sense of the factors rationalizing these results, I will also discuss some of the economic models relevant to incentive provision so as to highlight the main issues. However, readers interested in more complete theoretical surveys are referred to, e.g., Gibbons and Waldman (1999) or Prendergast (1999).

Due to space limitations, I will not discuss the large literature devoted to executive compensation, for which readers are referred to the survey by Murphy (1999). I will also not review the growing experimental economics evidence on the use and effects of incentive schemes. This latter literature (e.g. Fehr, Gächter, and Kirchsteiger (1995)) has produced some interesting contributions on the extent to which cooperative behavior among a group of individuals can occur. Whether those controlled experiments are adequate substitutes for what happens within "real" employment relationships may perhaps be debatable. However, some of the very subtle issues involved in the provision of incentives are very difficult to take to "real" data and thus having the ability to run a controlled experiment in a laboratory setting may prove useful.

2 Incidence of Pay-for-Performance Compensation Schemes.

Although a truly voluminous literature aimed essentially at incorporating more "realism" into the theoretical analysis of employment relationships has emerged over the last twenty five years, relatively little empirical work has been done on the topic, especially as it pertains to "rank and file" workers (as opposed to executives) who are the main focus of this survey. While much effort has been devoted to characterizing optimal explicit incentive contracts, it seems to this author that it took some time before people really started wondering why the use explicit incentive schemes was not widespread. Consequently, before analyzing the determinants and the effect of pay-for-performance contracts, it would seem appropriate to show the incidence of various forms of compensation.

Using four different data sets, Table 1 documents the extent to which workers in more or less representative samples of the population operate under some form of incentive pay scheme.[1] The least one can say about the use of explicit incentive pay schemes is that it is rather low, as less than 10% of the workers are paid either through commissions or piece rates. In fact, only in the National Longitudinal Survey of Youth (NLSY) is the percentage close to 10%; in the Quality of Employment Survey (QES), The January 1977 Current Population Survey (CPS), and the Panel Study of Income Dynamics (PSID), the fraction of workers paid either a commission or a piece rate hovers around 4%. A likely reason for such a discrepancy is that the question on how workers are paid differs across surveys. In the NLSY workers are simply asked if part of their earnings is based on either piece rates or commissions. In the other three data sets the question tends to be more restrictive: workers are asked if they are a paid either an hourly rate, a salary, or something else. The mutually exclusive nature of the question is such that workers paid, for example, by the hour and who also earn a certain relatively low percentage through piece rates are likely to report themselves as being paid on an hourly basis. The reverse is true for those who earn most of their labor income through piece rates or commissions. Still, even with the more "liberal" NLSY definition, it can safely be said that explicit pay-for-performance contracts are not ubiquitous. Yet, the fraction of workers being paid either through an explicit contract or receiving a bonus based

[1] See Appendix 1 for a detailed description of the data sets used.

on individual job performance is over 20% in the NLSY, and that excludes workers on profit sharing plans. If we include workers on profit sharing plans, the percentage is in fact close to 50%! Consequently one can see that firms use many tools other than incentive pay based on an objective measure of individual performance.[2]

Table 2 shows the breakdown by occupation. Not surprisingly, most commission workers tend to be in sales related work while most piece rate workers tend to be operatives, although quite a few service workers report being paid according to an explicit measure of performance. In contrast, promotions, bonuses, and profit sharing plans seem to be far more evenly distributed across occupations. From looking at Table 2, we can immediately see that the occupations for which output appears to be easily measured are the ones where explicit incentives are most used. Consequently, we would expect measurability issues to be part of any model that tries to predict the use of such pay systems. In a related vein, another interesting feature of Tables 1 and 2 is that it would appear that firms clearly favor providing incentives through promotions instead of using incentive schemes directly tied to a measure of performance such as piece rates or commissions. Although this is simply a re-statement of Baker, Jensen, and Murphy (1988)'s observation, we should nevertheless be a bit more cautious in that bonuses, which are directly based on a measure of performance, are used about as frequently as promotions.[3]

[2]This is of course assuming firms use profit sharing or bonuses as incentive devices.

[3]Two caveats here: although the question on bonuses in the NLSY makes explicit reference to job performance, respondents are not asked to make any difference between their individual performance and team performance. Secondly, promoted workers may also report that they were paid bonuses. On that last point, about a third of the workers reporting being paid a bonus also reported having received a promotion since the previous interview.

Part I
Incentive Contracts Based on Individual Performance

Outline

In this section I will focus on compensation forms based on an explicit individual measure of performance, such as piece rates or commissions. As we saw in the previous section, their use is not widespread, especially in the case of piece rates. Thus it is important to understand why that might be. Although an obvious answer would be that individual output might simply be too costly to measure, this still leaves unanswered the question of why we observe both straight time rates or salaries and piece rates in a given well-defined industry/occupation cell. After all, if output if easy to measure for firm A and firm A pays a piece rate, why does firm B in the same sector use a salary instead?

After outlining the main models of choice of pay methods, I will then survey what we know so far about the determinants of different contract forms. Then I will look at the effect explicit incentive contracts on wages (and thus, indirectly, on productivity). A major issue is the identification of a true causal effect on productivity of going from an "input-based" compensation scheme, such as an hourly rate, to an "output-based" one.

3 Determinants of Incentive Pay

3.1 Theoretical Considerations

3.1.1 The Lazear-Brown Model

Before trying to determine whether and to what extent incentive pay contracts do indeed "work", it might first be useful to understand part of the process by which some workers in a given industry/occupation cell are paid, say, by a piece rate while others in the same cell are paid an hourly rate.[4]

[4] I use the term "piece rate" here so as to encompass both true piece rate contracts as well as commission contracts. As emphasized by Lazear (1986), any form of pay method

Economic theory offers a straightforward prediction when firms can observe the effort put forth by their workers: it simply does not matter whether workers are paid a piece rate or a salary. Firms can enforce the first-best level of effort under either type of compensation form. To see this, consider the following simple model. A firm hires a single worker and effort e is directly related to output y by the following very simple technology:

$$y(e) = e \tag{1}$$

On the other hand, the worker's utility is assumed to be separable in wages W and in the cost of effort $C(e)$:

$$U(W, e) = W - C(e) \tag{2}$$

where $C(e)$ represents the worker's cost of effort with $C'(e) > 0$ and $C''(e) > 0$. In other words, effort is increasingly costly to the worker. Workers can be paid either a piece rate $P(y)$ or an input-based rate $S(e)$. Assuming a competitive environment that drives profits to zero, workers will be paid their full marginal productivity. In other words, $P(y) = e$ or, under a salary, $S(e) = e$. The socially efficient level of effort $e*$ is such that

$$C'(e*) = 1 \tag{3}$$

The fact that effort is observed and thus contractible makes it possible for firms to offer either a contract in which pay is output-based i.e. it depends on y, which would be a piece rate contract, or an input-based contract in which pay is based on e, and the first-best effort level can be achieved.[5]

A more interesting and realistic case is when effort cannot be contracted upon but output can. So let's assume for the moment that individual productivity can be measured, albeit at a cost, and that effort is not an issue here: firms hire from a pool of workers who are *heterogeneous* in their (time-invariant) productivity y. As in Lazear (1986) or Brown (1990), different methods of pay allow workers to sort themselves among firms. Zero expected profits are assumed throughout and workers are also assumed to know their productive ability but not the firms unless they incur a monitoring cost M.

that links pay to an objective measure of output is, in fact, a piece rate contract.

[5]The socially efficient level of output is such that the total social surplus is maximized. This is given by the choice of e which maximizes y(e)-c(e). Using the fact that $y(e) = e$, we get the first-order condition shown in equation(3).

Thus workers can be in a firm that pays a salary S which is independent of productivity y or in a piece rate firm that pays $W = y - M$. Thus the worker chooses the piece rate firm if and only if:

$$y - M > S \tag{4}$$

and the others choose to work in the salary firm. Provided that $M > 0$, there will be firms offering $S > 0$. Firms paying salaries will know they have attracted workers of lower average quality and they will pay a salary equal to the expected productivity of that subsample of workers:

$$S = E(y|y < y*) \tag{5}$$

where $y* = S + M$. Therefore, in this simplest of cases, compensation is independent of productive skills in jobs paying salaries while compensation moves one-for-one with skills in piece rates. More generally, we may simply allow skills to be rewarded differently in the two types of jobs:[6]

$$lnw_m = a_m + b_m y \tag{6}$$

with m=s,p and

$$a_s > a_p; b_s < b_p;$$

This optimal sorting of workers is illustrated in Figure 1 in the case where $b_s = 0$. This simple model of self-selection illustrate what most people have in mind when they say that salaries or hourly rates do not vary with output. Of course, over a longer term period, promotion opportunities and the simple continuation of the employment relationship are very likely to depend on y even in salaried jobs. So the sharp distinction between "output-based" pay and "input-based" pay is likely to be blurred in reality: people have to account for their actions at one point, irrespective of how they are paid.

Still, this simple model offers sharp predictions concerning the use of incentive pay:

- More productive workers choose piece rate contracts because only for them is it worth it to pay the measurement cost (through a reduced paycheck). Thus, any variable that proxies worker productivity such as years of schooling should be positively associated with the use of piece rates.

[6]Brown (1990) also suggests that there may exist in-between cases, such as "merit pay".

- The variance of output will be larger in incentive jobs than in salaried jobs because of the sensitivity of pay to individual productivity.
- Piece rate contracts will tend not to be offered the more costly it is to measure output. Consequently, we would expect that any factor representing (or proxying) costs of measurement to be negatively related to incentive pay.
- The more heterogeneous is the work force in terms of productivity, the more there is to gain from separating the more productive from the least productive workers.[7]

Note that the cost of measuring individual output will depend in particular on the extent of teamwork. A teamwork environment means that it may be very difficult to separate each individual's contribution from the total team contribution. I will return to this point later.

3.1.2 The Principal-Agent Model

The basic Lazear model emphasizes the selection process by which more productive workers choose to be paid explicit incentives. Also, for output to be observed and contracted upon, the firm must incur a cost. Otherwise, only piece rate firms would exist in equilibrium. In addition, risk aversion is not a central theme of that model. On the other hand, the main point of the basic principal-agent model is to emphasize how risk aversion on the part of workers combined with unobservability of effort affects the optimal sharing of risks and the provision of incentives. The risks are in the form of environmental factors (machine breakdowns, weather, depressed market conditions, etc.) over which the worker has no control and which induce variability in output (and compensation) that the worker would like to avoid. The firm is assumed to costlessly observe output, but cannot separate the effects of the risky environment from the level of effort put forth by the worker. The best assessment it can make is that high effort is more likely to have been exerted when output is high than when output is low. Even that may be hard to infer if random factors become too important.

Under these conditions, the optimal contract links pay directly to output, possibly in a linear fashion in which $w = a + by$ where w is the wage and

[7] See Lazear (1986) for a proof.

y is the output.[8] Since there are "environmental" risks associated with the production process, we assume that output y depends linearly on both effort and a normally distributed noise term ε. The slope of that contract (i.e. the parameter b) is what the firm tries to set optimally. Assuming that the firm is risk neutral while the worker is risk averse with a coefficient of risk aversion represented by r, the optimal piece rate contract sets the slope at

$$b^* = \frac{1}{1 + r\sigma^2 C''} \tag{7}$$

where σ^2 is the variance of the noise term ε representing the degree of risk (or "luck") involved, and C'' is the rate at which the marginal cost of effort increases.[9] Given an optimal piece b^*, a is then chosen to make sure that the worker earns exactly the same amount as she would earn in her next best alternative. Thus, a can be seen as the "base salary" to which is added an incentive pay component b^*y. We can see from equation (7) that the more risk averse is the worker, the more muted will be the incentive component. On the other hand, if the worker is risk neutral ($r = 0$), then $b* = 1$, no matter what is the magnitude of the variability in output. However, with risk averse workers, the optimal piece rate is generally lower than 1. From equation (3) we saw that the first best level of effort is such that $b = 1$. Here, the unobservability of effort combined with risk aversion forces the firm to optimally settle for a "second-best" solution in which incentives are muted. The firm does so by balancing the costs and benefits of changing the strength of the incentive component. If it did set $b = 1$, the benefit would be a higher level of effort coming at the cost of having to compensate the worker for the added risk she would bear. In essence, the optimal contract involves *delegation* by the firm to the worker of the appropriate choice of actions to take. The firm knows that its interest conflicts with the worker's (for a given amount paid, he would like to provide as little effort as he can) so it sets up the optimal contract in such a way as to induce the worker to take the

[8] In general, the optimal contract is not linear unless one assumes that the worker's utility function is exponential (which implies a constant absolute risk aversion). In what follows I will assume that the conditions required for linearity to be the optimal form are met. In practice, many commission or piece rate contracts are linear. Another commonly observed form involves a bonus for surpassing a quantitative target which is added to the base salary. This sort of piece rate contract is not linear.

[9] See Prendergast (1999) for a more general formulation in which both objective and subjective performance measures are used to determine the optimal incentive contract.

appropriate actions.[10]

The foregoing clearly suggests that unless we have either "extreme" risk aversion or output is virtually random ($\sigma^2 \to \infty$), workers should all be paid a piece rate. In addition, the so-called Informativeness Principle (Milgrom and Roberts (1992)) states that any signal observed by the firm that informs it on the worker's effort (e.g. customer complaints, bad weather, other salespersons' output in the same market) should be incorporated into the optimal contract. In other words, anything that reduces the "luck" element and helps the firm extract a better signal of effort should be used so as to increase the incentive component of the worker's pay. Consequently, workers' pay would likely change frequently. The simple facts are that 1) relatively few people are paid piece rates or commissions; and 2) people's pay does not change all the time.

This forces us to think about what factors may contribute to the overall very low incidence of explicit pay-for-performance contracts. The first issue that comes to mind is about measurement. Although the firm may have a good idea of what it wants workers to enhance by their choice of actions, it is not clear that it can actually base the compensation scheme on that dimension. Sometimes, the firm will instead base the worker's pay on another measure that *appears* to be equivalent. However, as pointed out by Baker (1992), it is not clear that the worker will act in the firm's best interest by responding to the incentive scheme that he is being offered. An often-cited example (e.g. in Brown (1990) is that of the incentive contract offered to former quarterback Ken O'Brien of the New York Jets. After a year in which he threw a lot interceptions (and also a lot of touchdown passes-i.e. he took some risks), he was offered a contract in which is final pay would be a function of many factors. In particular, his paycheck would be negatively affected by the number of interceptions he would throw. The end result was that O'Brien held onto the ball a lot more than in the previous year instead of throwing it and, in the process, was sacked much more often. While it is part of this particular sport's gospel that "not turning the ball over" is a key to winning games (which is presumably the ultimate goal pursued by management), putting such an explicit emphasis on that aspect alone likely turned out to be counterproductive. This is just one example among many

[10]In a related vein, see Garen (1998b) on the issue of how control of the work routine will be assigned when the cost of measuring output varies and how this affects the provision of incentives.

illustrating what Kerr (1975) called "The Folly of Rewarding A, while Hoping for B". That problem can also be formulated in a different way. Suppose that in a job in which a good measure of output for a particular task does exist, workers are asked to perform many value-enhancing activities, all of which compete for the worker's limited time. Depending upon whether these different tasks are substitutes or complements (e.g. maximizing the number of units produced by a machine in a given time period is likely to be in conflict with the maintenance of the equipment), then it may be optimal to set the piece rate slope to zero Holmström and Milgrom (1991). The worker's pay then consists only of a base salary and has no incentive pay component. Another possibility for the firm is to base part of the worker's pay on the aspect of the job for which an objective performance measure exists and then to complete the compensation package by basing the rest on a *subjective* measure which, by its very nature, is practically impossible for a third-party to verify. I will come back on this aspect of incentive provision below when I discuss implicit incentives.

Finally, some recent literature, most of it theoretical, has pointed out that how firms carry out their activity or organize production is in fact closely related to the provision of incentives. In particular, Holmström and Milgrom (1994) emphasize how firms that cannot easily monitor inputs will tend to subcontract that activity and provide incentives by paying agents according to output. Conversely, when the output is a complex object to define and the firm both controls the technology and can relatively easily monitor workers' effort, it should tend to produce "in-house" and offer relatively low-powered incentives. This goes back to the notion that delegation of authority must be accompanied by incentive provisions. More generally, it points to the importance of designing organizational structures that complement one another instead of viewing each part on a stand-alone basis. Garen (1998a) further explores the same line of argument by pointing out that salary pay systems and hourly pay systems emerge as optimal response to different work environments. Following Fama (1991), he argues that when effort is tightly linked to output, firms will monitor effort and pay by the hour. On the other hand, when effort is a very poor signal of output (poor in the sense of being very noisy), salaries will instead be paid. MacLeod and Parent (1999a) make much the same argument in considering the determinants of piece rates, hourly rates, salaries, commission, and bonuses.

In summary, refinements made to the basic principal-agent model have made it possible to further our understanding of the factors explaining the

low incidence of explicit pay-for-performance contracts. In addition, even in its basic formulation, that model can rationalize the use of *relative* performance evaluations to provide incentives. By the Informativeness Principle, the firm should use any information that helps to reduce the noise present in the output signal. In the limit, if it could get a perfect signal, there would not be any risk ($\sigma^2 = 0$) and the firm would set $b^* = 1$. One way for the firm to reduce the importance of the noise is by comparing different workers in the same job, such as sales persons operating in the same market. Given that they face more or less the same common risks, those can be purged through relative performance evaluation. Thus, the only thing that matters in terms of performance is the rank of each worker. The solution for the firm in such a setting is to let the workers compete for, say, performance bonuses or promotions, much like in a sports tournament Lazear and Rosen (1981), Rosen (1986)). However, tournaments may prove to be counterproductive in situations where teamwork is important or, more generally, where the firm benefits from having people collaborating with one another even in an informal way. Attributing rewards based solely on the ranking of individuals may trigger "sabotage activity" (or, less spectacularly, the absence of cooperation) Lazear (1989) because the interdependency between the workers' output levels is such that each one benefits from decreasing the others' output. Consequently, any job that involves teamwork should tend not to be associated with incentives based on individual performance.

3.2 The Role Played by Institutions

Other factors that may or may not be correlated with those in the list above include the gender mix of the workforce and the presence of a collective bargaining agreement. Concerning unions, their role in determining contract form is likely to be ambiguous. On the one hand, it is well known in the wage inequality literature that unions tend to reduce it, at least within unionized establishment. Indeed, the dramatic decline in the percentage of workers covered by a CBA over the last thirty years has been shown to be an important factor behind the substantial increase in earnings inequalities in the United States (DiNardo, Fortin, and Lemieux (1996)). Assuming that unions favor wage compression, it may appear that they would tend to favor hourly rates or salaries over incentive pay. On the other hand, unions may also play an active role in facilitating the operation of a piece rate system by getting the firm to commit to a piece rate schedule that would need union

approval before being modified. Such an outside constraint on the freedom of management to adjust rates in the face of a changing environment may actually be beneficial because firms would often be tempted to "cut the rates" or adjust the performance threshold upward for a bonus once workers would have revealed how difficult the job really is. Implicit in this statement is the fact that firms are assumed to lack some information that the workers possess about the production process. Workers, knowing that firms can not commit not to adjust the rates once it has all the relevant information, will reduce their effort level at the start in order to guarantee themselves higher rates in the future. This is the so-called *ratchet effect* problem (Gibbons (1987), Kanemoto and MacLeod (1992)). Therefore, a contractually agreed upon piece rate schedule, because it cannot be easily modified at management's will, could actually provide benefits over the long run.[11]

As for the effect that gender might have on the observed choice of pay scheme, much of the argument rests on the potentially weaker degree of labor force attachment by women (Goldin (1986)). The reason, of course, is that women are more likely than men to have career interruptions due to child-bearing decisions. If firms provide incentives to their workers by "tilting" the wage profile (Lazear (1981)) so that young workers are paid less than their full marginal productivity in exchange for wages higher than the value of their marginal product when they have more seniority (conditional on not having been caught shirking), then this sort of deferred compensation arrangement may not suit women who plan to leave their job at one point. And, precisely because female workers tend not to stay, firms would not expect deferred compensation to have a strong incentive effect on them. Consequently, as argued by Goldin (1986), female workers may be more likely than men to choose piece rates because of the lower value of the deferred portion of the compensation package. Piece rates, in effect, provide short-term incentives which may suit women better. Although the same argument could be offered to suggest that women are more likely than men to prefer commission contracts, Geddes and Heywood (2000) argue instead that commission workers often interact on a repeated basis with customers, which again makes the relationship long term in nature. Consequently they argue that men should be more likely to be paid commissions.

In addition, if women prefer to have more flexibility in their work schedule

[11]See also Lazear (1986) for a "solution" to the problem of underprovision of effort by the worker when the firm cannot commit not to renegotiate the rates.

because of family responsibilities in general (illnesses, picking up the children at daycare, etc.), then they may choose (or firms may choose for them) jobs for which teamwork is not a crucial factor. If teamwork requires cohesion among the members of the team, then any disruption of the work routine due to a worker being absent might be costly to the firm. Since teamwork is likely to make the cost of measuring individual output higher, it it likely that firms may favor implicit incentives (possibly deferred compensation packages) over explicit individual incentive schemes. This, as Geddes and Heywood (2000) suggest, might be an additional factor why women are more likely than men to be paid piece rates.[12]

3.3 The Evidence

In the previous subsection, we saw that whether firms use an explicit incentive pay scheme or not will depend mainly on:

- whether individual output can be measured.
- the cost of such measurement.
- the monitoring technology.
- the degree of risk aversion on the part of workers.
- the randomness in output.
- the level of worker skills.
- the "complexity" of the job (e.g. number of tasks).
- whether part or all of the randomness associated with a particular job can be filtered out by performing relative performance evaluations of a given group of workers. The *rank* of the workers in terms of productivity, as opposed to their absolute productivity, will determine who gets the largest rewards.

[12]Concerning the relationship between risk aversion and the optimal incentive contract, much of the evidence available so far comes from work done on executive compensation. Recent contributions on this line of research include Aggarwal and Samwick (1999), who find that incentive contracts are sensitive to risk aversion, and Garen (1994) who finds contrary evidence. As mentioned in the introduction, the emphasis in this survey is clearly on rank and file workers. For a comprehensive survey on executive compensation see Murphy (1999).

- Gender.

Before looking at the effect of some of the factors outlined above in a multivariate context, it may be useful to show some descriptive statistics linking job characteristics to either pay schemes or to occupations. Table 3 shows, using both the QES and the NLSY, the fraction of workers in each of the twenty occupations listed who reported that their job was associated with the characteristics listed. It is interesting to note that the occupations for which teamwork appears to be less important (sales, operatives) are the ones in which we find almost all the piece rate or commission workers in the samples. Figure 2 shows more directly the degree to which the characteristics are linked to pay methods. While this evidence is purely descriptive, we can see that many of the factors predicted by theory not to be associated with piece rates or commissions seem to go in the right direction. More particularly, teamwork clearly appears to favor salaries or hourly rates over piece rates or commissions while multitasking is most prevalent in salaried jobs. Figure 2 also suggests that treating commissions as being similar to piece rates may not be totally appropriate: commission work seems to be more "complex" in the sense that workers report learning more new things and having less repetitive tasks than is the case for piece work.[13]

Table 4 summarizes the evidence concerning the impact of some of the factors in determining contract form. Naturally, some of those variables are difficult to measure and it has been a challenging exercise to arrive at convincing evidence, especially taking into account the fact that the variables researchers are trying to explain are categorical, and, as explained in Appendix 2, misclassification of pay methods makes it harder to measure relationships than when the variable to be explained is continuous.

Still, both Brown (1990) and MacLeod and Parent (1999a) find fairly strong evidence that multitasking tends not to be associated with piece rates or commissions. In addition MacLeod and Parent find that worker autonomy, does tend to go hand in hand with such explicit incentive contracts.[14] A similar result is found by Garen (1998a) in the context of studying the

[13]In fact, in MacLeod and Parent (1999a) we put piece rate and hourly rated workers in one group and commission and salaried workers in another.

[14]In MacLeod and Parent (1999a) we put piece rate and hourly rated workers in one group and commission and salaried workers in another. So, even though figure 2 would suggest that, contrary to the theory, multitasking is prevalent in commission work, relative to piece rate or hourly rated jobs, such is not the case when we compare commissions with salaries.

determinants of salaries vs. hourly rates. The result on autonomy is very much consistent with the story in Holmström and Milgrom (1994), who emphasize that delegation of authority is likely to come with output-based incentives. Among other results reported in both MacLeod-Parent and especially in Brown, teamwork appears to favor the use of hourly rates or salaries instead of output-based incentive contracts. This is as expected since teamwork means that individual output may in fact be difficult to measure and/or cooperative behavior among workers is likely to be easier to achieve when incentives are not individual-based.

As for the other factors affecting the probability of observing explicit pay-for-performance contracts, Geddes and Heywood (2000) find that, as hypothesized, women are at the same time more likely to be paid piece rates *and* less likely to be paid commissions than men. This is consistent with the view that those two types of explicit contracts are in fact different even though in both cases pay depends directly on measured output. The argument raised by Geddes and Heywood that commission work may be better characterized as having more of a long term perspective than a piece rate job is actually complementary to a certain extent to the point made in MacLeod and Parent (1999a) that output under a commission contract is very likely to be much more noisy than in piece work. Hence, it may take some time before an individual doing commission work develops the skills and contacts necessary to reduce the idiosyncratic variability in output.

4 Incentive Effect of Pay-for-Performance Contracts

In the previous section we saw that firms who offer output-based compensation schemes can expect to attract and retain more productive workers. Naturally, firms would also like to induce workers of a given ability level to work harder by tying part of their paycheck to an explicit measure of output. Although this may seem obvious, it is not: even if a worker is paid a straight salary that does not vary with the (unobservable to the firm) effort exerted by the worker, the firm can still provide some incentives to its workers by, for example, basing the continuation of the employment relationship upon past performance: a worker cannot expect to keep his job forever if he shirks continually. I will come back later on the issue of providing incentives in the

course of a long term relationship.

That being said, it is in principle possible to assess whether commission or piece rate contracts have any "incentive effect" provided that we control properly for the selection process governing the choice by individuals of those types of compensation methods. In other words, the issue boils down to whether one can "purge" any estimated productivity or wage effect of its unobserved ability component. As such, the main problem facing researchers has to do with the existence of the appropriate data and the use of the appropriate statistical tools. In essence, as in all evaluation problems, researchers try to answer the so-called "what if" question: what would have happened to the same workers paid piece rates or commissions had they been working under a time rate or a salary?

4.1 Cross-Sectional Evidence

Somewhat arbitrarily, I will focus on four specific studies carried out over the last 20 years or so. I choose as a starting point John Pencavel's (1977) work since it signalled a renewed interest in the topic among economists.[15] To be sure, many studies had been carried out over the years by researchers, particularly in industrial relations.

The main characteristic shared by these four studies is that they are all cross-sectional. In other words, none of these papers follow a sample of workers or firms through time. Consequently, they all implicitly assume that the "what if" question can be answered satisfactorily by simply comparing the average pay or productivity of incentive workers to that of the time rated workers, conditioning out the effect of the observable characteristics present in the samples. In other words, the control group made of workers not paid under an explicit pay-for-performance contract is a good approximation of the counterfactual state for workers who are operating under pay-for-performance contracts. This is a strong assumption, especially considering the theoretical model outlined above which stresses that, in fact, the two types of workers are likely to be different. Of course, if all the characteristics present in the data control perfectly for the difference in individual productivity, using cross-sectional data poses no particular problem. However, this strong requirement

[15]Indeed, Pencavel starts his paper by citing Alfred Marshall's Principles to illustrate that economists used to be interested in the notions of "intensity and diligence of worker effort manifested by workers" before essentially leaving this field to industrial relations researchers.

is very unlikely to be met.

The other three studies are by Seiler (1984), Brown (1992), and Ewing (1996). Seiler and Brown used very large samples of establishments from the Bureau of Labor Statistics' Industry Wage Surveys, each survey covering a particular industry, while Seiler uses just one wave of the NLSY.

All four papers find a positive relationship between the use of "incentive" contracts and wages, consistent with many of the previous studies that had been carried out before the Pencavel paper.[16] Note that all of the above-mentioned authors were aware that some selection process was involved, either on the firm side or on the worker side, the title to Pencavel's paper making explicit reference to the screening of workers by firms and Ewing being careful not to mention that the estimated relationship represents any incentive effect: in fact, he simply highlights that his results are consistent with the model exposited in Brown (1992), which itself is a straightforward extension of the Lazear (1986) model.

The safest conclusion that can be reached looking at these studies is that, indeed, higher productivity does seem to go hand-in-hand with explicit pay-for-performance contracts, but that we really do not know whether the effect is causal or simply reflects the selection of intrinsically more productive workers into jobs that pay according to some form of explicit incentive contact. Note that from the firm's perspective, even if incentive pay does not induce workers to supply a higher level of effort, the screening mechanism embodied in incentive contract is such that it will hire more productive workers anyway. Consequently, one would expect firms to use incentive pay whenever a good objective measure of output exists, such as in sales. However, even when what appears to be a very good objective measure of productivity exists, there are situations when firms are very reluctant to put a lot of incentive pressure on just one dimension of the job. I will return to this issue later when implicit incentives are examined.

4.2 Longitudinal Evidence

Given that we can expect workers to be "positively selected" into piece rate or commission jobs, it is very important that we can somehow find a way to answer the usual "what if" question: what would have happened to workers paid piece rates had they been paid a salary or an hourly rate? Answer-

[16] See either Pencavel (1977) or Seiler (1984) for the specific references.

ing that question requires the use of longitudinal data where workers are observed in both circumstances.[17] By using the so-called "within worker" variation in pay methods we can then recover the incentive effect. While this is no doubt a major improvement over cross-sectional data, it is not bullet proof. First of all, it assumes that whatever unobserved worker characteristic (or unobserved productivity) drives the selection into incentive contracts, that characteristic does not change through time. Consequently, we can apply standard fixed-effect estimators in which a simple transformation of the data purges all time-invariant worker components. That assumption would be violated, for example, in the case where both the firm and the worker are actually unsure about the latter's productivity which would be inferred from observing the worker's output through time. In that case, the unobserved (to the econometrician) worker productivity component would not be time invariant and could not be differenced out from the wage equation. However, most applied researchers are willing to consider the fixed-effect assumption as a reasonable approximation to what they realize is a more complex process. Secondly, all unobserved dimensions of the wage determination process are not limited only to the worker: how productive a worker is with a particular firm depends to a certain degree on how well that worker is matched with her/his current employer. Consequently, it may not be enough to have multiple observations of the same worker across jobs; we would need also to have multiple observations within the same employment relationship. In that case, the incentive effect is identified through the changes in pay methods *with the same employer.* Now, of course, staying with the same employer does not necessarily mean that one's tasks have stayed the same: a worker's position within the firm could change, which could possibly lead to a much different job. Still, the ability to control for unobserved employer-employee

[17]Strictly speaking, that is not necessary. One could still identify the true causal effect of piece rate contracts with cross-sectional data provided there existed an exogenous determinant of that form of payment that played no direct role in the log wage equation. It is very difficult, a priori, to think of such a credible exclusion restriction. Another way to identify the incentive would be to impose distributional assumptions (usually normality) and calculate the conditional expectation of the selected residuals in order to include it in the wage regression. This is, of course, the now familiar "two-step" Heckman selection procedure. In principle, the incentive effect could be identified even without the aid of credible exlusion restrictions (or instruments). However, the identification would be achieved solely because of the specific functional and distributional form assumptions imposed. Most researchers are very wary of using a Heckman-type procedure without an exclusion restriction.

matching effect is likely to represent a major improvement over simply being able to control for unobserved worker attributes.

Three recent studies have used multiple observations of the same workers with given employers to try to identify the incentive effect of incentive contracts. Parent (1999) exploited the fact that questions on pay methods were asked three straight years in the National Longitudinal of Youth which, combined with ability to identify each jobs separately, allowed the use of fixed-effect methods to purge the estimates of some the biases caused by omitted variables, including unobserved job-match characteristics. On the other hand, Lazear (1996) used instead repeated observations of workers in a single firm which changed the way it paid its workers, going from hourly rates to piece rates. The Lazear sample offers a cleaner way to identify the "true" causal effect in that the only thing that changed in that company was the pay scheme: the job (windshield installation) did not change before and after the switch. He is also able to analyze other results of the pay change, such as the extent to which the company (Safelite Glass Inc.) retained its best employees while the least productive ones left after finding out that working under a piece rate was not to their advantage compared to the being paid an hourly rate.

Interestingly, while the data set used greatly differ, it turns out that the estimated average wage effects are fairly similar (a little over 6% for Parent and at least 9% for Lazear) and that the portion of the cross-sectional wage differences between incentive workers and other workers that can be attributed to worker selectivity effects is also similar. In addition, Lazear shows that absenteeism decreased following the change, thus giving an added source of productivity growth. Although piece rates do seem to induce greater worker effort (although there are still some unresolved issues, see below), Parent finds that workers earnings bonuses do not appear to be earning any wage premium once selection effects are taken into account.

In addition to trying to estimate the incentive effect, Parent (1999) also estimates a simple model of the covariance structure of wages in order to identify the main sources of the larger observed residual variance of wages for incentive workers.[18] Seiler (1984) had previously observed the same phenomenon in his sample of Industry Wage Survey workers. He conjectured

[18]Residual in the sense of netting out the effect of schooling and labor market experience. In fact, it makes little difference whether one looks at the raw wages or the wages net of experience and schooling; in both cases, the variance is larger in the subsample of workers paid either a piece rate or a commission.

that this reflected the greater risk (of output variability) faced by incentive workers. Consequently, assuming risk aversion on the part of workers, those that were paid piece rates required a compensating differential for the higher earnings risk they faced. This would partly explain the wage premium enjoyed by incentive workers, the rest of it being due to effort inducing effects. By decomposing the residual variance into a worker-specific component reflecting the underlying heterogeneity of worker productivity and a "true" residual term, Parent (1999) showed that the variance in wages for incentive workers was accounted for by the worker component to a much greater extent than was the case for other workers. That result is not consistent with incentive workers being compensated for higher risks. In fact, it turns out that the variance net of the worker component, which would better reflect income risk, is actually larger for the subsample of hourly rated and salaried workers than it is for piece rate workers.

The most robust conclusion that can be drawn from both papers is that selection effects are very important. In fact, even if there were no true incentive effects, the evidence presented in Lazear's paper shows that the overall productivity of the workforce *that stayed with Safelite* was substantially higher than the productivity of Safelite's workforce before the management switched to incentive pay. Secondly, Parent shows that, not too surprisingly, there is a great deal of heterogeneity in worker productivity in incentive jobs. Indeed, that's partly what a piece rate contract aims to achieve: to allow the best workers to separate themselves from the least productive ones.

Finally, in perhaps the most interesting study of the three, Ichniowski, Shaw, and Prennushi (1997) used plant-level data to examine the effects of many different human resource management (HRM) practices, including incentive pay, on the productivity of steel finishing lines. Their study is of particular interest because it provides evidence on the importance of complementarity of HRM practices, a point that was emphasized among other by Holmström and Milgrom (1994). In other words, successful systems of HRM practices are likely to occur when they are well designed and implemented as a group. In particular, they find that changes in individual work practices have little effects on productivity.[19]

[19]See also Athey and Stern (1998) on the topic of complementarity and on how to devise the appropriate empirical framework in the presence of such complementarities.

4.3 Unresolved Issue

Although recent attempts at trying to measure the incentive effect of incentive contracts have been able to exploit the longitudinal nature of the data sets to purge out unobserved components, the assumption that these effects are fixed may not be totally realistic. We can gain some insight about the proper way to handle the underlying selection process by looking at the Lazear-Brown model exposited in the section 3.

If we look at Figure 1 or at equation (6) we can see that the sensitivity of earnings to workers productivity varies with the pay method. In the extreme case illustrated in Figure 1, people under a salary system are paid the same, regardless of their individual productivity. On the other hand, piece rate workers are paid exactly according to their productivity. Thus the "return to skills" varies across pay methods, *even for the same individual*. I emphasize that part because the recent papers exploiting longitudinal data sets to difference out unobserved worker productivity components have failed to take into account the fact that if the return to productivity varies across pay methods and if people self-select into different pay methods according to their comparative advantage, then using standard fixed-effect methods to identify the true causal effect from pay method switchers *will not* eliminate the selection bias that plagued the cross-sectional studies. In other words, the fixed-effect model answers the "what if" question by assuming that the relationship between an individual's skills and her pay is invariant across pay methods. If the Lazear model is the appropriate way to think about the choice of pay method, it is clear that this identifying assumption is violated. Future empirical work should therefore attempt to be more careful in modeling the selection process.[20]

[20]Of course, an ideal empirical setup would have random assignment of the workers to incentive contracts, in which case a simple before-after comparison would provide direct estimates of the average incentive effect. To the best of this author's knowledge, while such an experiment was recently carried out in Canada (see Shearer (1999)), no comparable work has been done in the United States.

Part II
Incentive Contracts Based on Group Performance

Outline

In this part, instead of reviewing the existing large literature on the use of profit sharing plans, I will offer new evidence suggesting (and confirming previous studies) that profit sharing plans do seem to "work", conditional on certain identifying assumptions. For a very comprehensive overview of the effect of profit sharing plans, readers are referred to Kruse (1993).

5 Why Provide Incentives Using Group Performance and Why Does it Work?

In short, people cannot really provide compelling answers to those two questions! On the one hand, profit-sharing plans, as an incentive device, suffer from free-rider problem: why would an individual in a group of N workers really be induced to provide effort when he will get only 1/N of the increased productivity or profits. Put differently, why would he not rely on the N-1 other workers' efforts to increase his wage without changing his own behavior. On the other hand, explanations based on the possible beneficial effects of profit sharing plans on team morale, worker cooperation, better internalization of the firm's objectives by the workers, etc., suffer from the problem that they are very difficult to verify empirically.

Given the a priori major problem of free riding, it would seem that firms would think of another mechanism to provide incentives. However, as is clear from Table 1 when looking at data from both the QES and especially the NLSY, profit sharing plans are very common. One possibility is that firms use such plans mainly because of tax incentives, not because they really think that workers' productivity will be positively impacted.

Concerning the productivity effect, basically all studies find a positive relationship between the use of profit sharing plans and either measures of productivity or workers' wages. Of course, one difficulty in assessing a causal

effect is that it could well be that while high profit firms tend to have profit sharing plans, they may still earn high profits without them. In other word, more convincing evidence would show positive effects following a given firm's *adoption* of a profit sharing plan. Such evidence is offered in Kruse (1993). Essentially, even controlling for firm's unobserved characteristics, those that switch to profit sharing plans do seem to exhibit higher productivity. However, Kruse notes that firms adopting profit sharing plans tended to have rising productivity *before* the adoption of the plan. In principle, this can be accommodated by simply adding an interaction term between the profit sharing dummy and a trend term. This control is intended to capture any differential trend in the growth rate of productivity between profit sharing firms and other firms. As it turns out, when Kruse (1992) controls for differential trends on productivity, results are basically left unchanged.

5.1 The Wage Effects of Profit Sharing Plans.

Given that the NLSY data does not contain firm-level data, such as productivity per worker, I will use instead wage data in addition to a question asked to workers about the use of profit sharing plans.[21] I then exploit the longitudinal dimension of the NLSY, including the fact that we know whether a worker stays in the same job or changes employer, to estimate fixed-effect models of the wage impact of such plans.

In levels, the model I estimate is :

$$w_{ijt} = X_{ijt}\Gamma + PS_{ijt}\delta + \alpha_i + \theta_{ij} + \varepsilon_{ijt} \tag{8}$$

where w_{ijt} is the log hourly wage of individual i in job j at time t, X_{it} is a vector of individual characteristics which may be time-varying, Z_{ijt} is the vector of time-varying job-match attributes (e.g. tenure with the current employer), and PS_{ijt} is a dummy indicator for a profit sharing plan. The other terms represent unobserved components of variance which may be correlated with the observable characteristics. As is well known, estimating equation (8) in levels may produce a biased estimate of δ if profit sharing plans tend to be offered to intrinsically more productive individuals (high α's) or to workers in particularly good matches (high θ's). Consequently, one can alleviate the unobserved worker quality problem by estimating equation (8)

[21] It is not possible to know the exact nature of the plan (cash based or deferred based plan, such as a pension fund in which firms put a percentage of their profits).

in differences or by transforming the variables so that they are expressed in deviations from individual means form. Although unobserved fixed worker attributes are purged out doing this, we may still have a bias stemming from unobserved job-match characteristics. Consequently, it would be even better to use differences within jobs so as to eliminate all time invariant unobserved components. Exploiting the longitudinal structural within jobs then allows to measure the effect of adopting a profit sharing plan in a given employment relationship

Results are presented in table 5. As we can see, although the coefficient does change when one goes from estimating in levels to fixed-effect estimation, there is still a statistically (and economically) significant effect of profit sharing plans in the case of men.[22] In fact, the results for men are very much in line with what researchers have previously found. Thus, the results presented here provide further evidence of the beneficial effects that profit sharing plans have on productivity. This assumes, of course, that workers are paid their marginal productivity. It could be that profit sharing plans have a wage effect but no productivity effect if firms simply use profit sharing plans as a simple way to share rents with workers. Although the question remains as to why they would do that if it has absolutely no effect on worker productivity. [23]

The results for women are intriguing in that one possible explanation for the apparent lack of a relationship between profit sharing plans and productivity (or at least wages) is that, similar to the argument raised by Goldin (1986), women with lower levels of labor force attachment would not be influenced in their work effort decision quite the same way that men would in the presence of profit sharing. If most profit sharing plans consist of contributions to pension plans (something we do not know from the NLSY data but which Kruse (1992)'s work strongly suggest is the case), then the perspective of benefitting from contributions that will likely turn out to be much smaller because of a shorter duration in the employment relationships is potentially

[22]Moreover, the decrease in the magnitude of the coefficient likely results in part from misclassification of the profit sharing dummy. See Appendix 2 for more details.

[23]Note also that, given the short time dimension of the NLSY panel, I did not include lagged dummy indicators for the presence of profit sharing plans in previous years, which would have reduced the sample size considerably. The idea of including lagged dummy indicators is that the positive productivity effect of profit sharing plans may take some time to develop, as the evidence in Kruse (1993) suggests. Thus, the measured effects shown in Table 5 may actually represent a lower bound of the true effects.

less effort-inducive to women.[24]

5.2 Other Explanations for the Use of Profit Sharing

Is the incentive motive necessarily the only reason why firms would use profit sharing plans? My reading of the literature on profit sharing is that researchers always emphasize the 1/N problem and view the use of profit sharing plans as a puzzle. In other words, from the start, people take the view that profit sharing plans are used to provide incentives to groups of workers and, as such, free riding should discourage its use.

Yet, it is striking that few people, with the exception of FitzRoy and Kraft (1987) have looked at another, possibly less puzzling motive: firms may use profit sharing schemes because they provide a simple way for the firm to commit itself to reward firm-specific skills acquired through on-the-job formal or informal training programs. Why the need for commitment? Simply because by their very nature, firm specific skills have *no* market value, and thus firms cannot be trusted to share the rents over those skills with their workers unless one invokes reputation effects. By writing an explicit contract in which it is specified that workers get a certain percentage of the profits, workers can feel more confident that they will not be held up ex post. Consequently, they may choose to devote some time to learning firm-specific skills.[25]

[24] One way to assess that conjecture would be to re-estimate the same models, but with women who are past their childbearing years, either because they have already had the number of children they planned to have or simply because of age.

[25] Note that even if the firm pays all training expenses, getting involved in activities that are of value only to the current firm is costly to the workers: they have to use some of their time and that time could have been used to enhance their market value. A common example in academia is participating in committees. This can be time consuming and that time could have been used to write papers. It is safe to say that the market rewards the writing of (good) papers but certainly not an impeccable dossier in terms of committee work.

Part III
Implicit Incentives, Dynamic Considerations, and Long Term Relationships

Outline

We can see from Table 1 that although workers on piece rates or commissions account for less then 10% of the work force, a significant fraction of workers report having received either a bonus or a promotion, or both. This part of the chapter will look at some of the main theoretical issues involved and at the somewhat sparse evidence we have available so far. This review of implicit and life-cycle incentives is by no means comprehensive. I will focus only on the theoretical notions that yield testable implications. For an excellent "refresher-type" survey of the main issues surrounding the provision of life-cycle incentives, see Carmichael (1989).

6 Efficiency Wages

The basic idea of the "shirking style" efficiency wage theory, as described by Shapiro and Stiglitz (1984), is that firms facing a moral hazard problem on the part of the workers will consider either monitoring their choice of actions (or effort) to ensure that they do what the firms want them to do, or it may choose to save on monitoring costs by paying wages that are higher than what is necessary to attract workers. In other words, by giving rents to the workers and by threatening to fire them if they are caught shirking, in which case the workers will lose all future rents from the employment relationship, the firm can induce workers to exert effort. In equilibrium, firms all pay higher than market wages which can only be sustained at the cost of reducing employment. The endogenous creation of this pool of unemployed workers, who have to spend some time in that state before being re-employed, acts as a potent threat and thus workers do respond by working harder.

The only direct implication from this sort of model that has been tested is the tradeoff faced by firms between monitoring and wages. In short, all

else being equal, firms that monitor more intensively should pay lower wages because wages act as a substitute for monitoring in motivating workers. That is, assuming that one controls for the firms' marginal return to effort provision by workers. If the return to effort varies across firms, then a cross sectional look at different firms may fail to identify the tradeoff between monitoring intensity and wages; in fact it may find the two to be complements instead of substitutes, a point raised by Prendergast (1999). In fact, the two attempts at trying to identify that tradeoff have produced mixed results, with Groshen and Krueger (1990) finding evidence in favor of the prediction and Neal (1993) not find much support.

7 Bonuses and Promotions

7.1 Theoretical Issues

Suppose that a perfectly usable measure of output exist but that the firm, because it wants its workers to devote part of their time to certain aspects of the job that are not so easily measurable, chooses not to base the worker's pay only on the objectively quantifiable aspect but also on other dimensions of job performance that influence firm value. Such components of pay will therefore be based on a subjective evaluation of performance which will lead to so-called "merit pay" or subjectively determined bonuses.[26]

It would seem that, for a variety of reasons, such arrangements would be difficult to implement. First, given the subjective nature of the performance evaluation, it would be very hard for a third-party such as a court to enforce the terms of such a contract if one of the party felt short changed. The possibility for "cheating" on the terms of the implicit contract is real if the individual who performs the merit evaluation is a residual claimant to the firm's output. For then there is a clear incentive to under report the worker's contribution. Anticipating this, the worker is likely not to put forth as high an effort level as if he knew the evaluation process to be "fair". Second, in the case where the performance is evaluated by somebody who is not a residual claimant to the output, the evaluator will possibly take actions that may not be in the firm's interest. These may take the form of either compressing the distribution of the evaluation ratings around a norm or simply overrating

[26]As opposed to bonuses paid to workers who surpassed a certain quantitative target, such as is often the case in sales work.

the poor performers because penalizing them is not a pleasant task. Indeed, the human resources management literature is full of cases reporting such "centrality biases" and "leniency biases".[27] In addition, because performance evaluations are subjective and thus depend on perceptions, there clearly are incentives for workers to engage in rent-seeking activities in order to enhance their chances of getting good evaluations.

As for promotions, they may result from either a subjective performance evaluation process, or from a tournament-like setup in which a group of workers are offered a fixed set of prizes over which to compete, with the top performer in the group getting the top prize. The discussion earlier emphasized the fact that tournaments allow the firm to filter out the randomness in output that is common to all the workers in the group. If output is contractible, as was assumed in Part 1, then promotions are based on an explicit measure of performance. But even when output is "too complex" to be contracted upon, firms can still set up tournaments in which the top prize goes to the best performer, as subjectively determined by the firm. As is the case for merit pay, it is not clear that the firm will not be tempted to deny workers their promotion since, in effect, no one outside the firm can verify either party's claim as to the quality of the worker's performance.

Before offering arguments as to why bonuses and/or promotions are nevertheless quite common, it should be noted that it is not clear that the bonus measures calculated using the PSID truly reflect subjective performance rewards. However, the data show that the incidence of bonuses is much lower for workers paid on an hourly basis or paid a piece rate than it is for salaried workers. In fact, in MacLeod and Parent (1999a), we argue that workers paid commissions or salaries should be treated separately from other workers for the simple reason that the output of workers on salaries and/or commissions is likely to contain a much more important random element than the output of workers on an hourly rate or a piece rate. For example, a salesperson might work for long hours before closing one deal while another sale could be made very rapidly. On the other hand, working for a few more hours on an assembly line will almost certainly lead to higher output. Consequently, the bonus measure computed from the PSID, while noisy, still seems to contain some information. In the NLSY, workers are asked directly to report whether part of the labor income was earned through performance-based bonuses. Although this does not exclude payments for team production, Brown (1990)

[27]See Prendergast (1999) for a list of references.

reports that those are relatively rare. Thus, we can be relatively confident that the measures of bonuses present in the data sets capture, at least to some extent, the notion that these reflect a subjective evaluation of performance. Assuming that this is the case, then it seems clear that their use is not rare. Indeed, as can be seen in Figure 3, the evidence from the PSID even suggests that it has been increasing over time for salaried workers *excluding* sales workers.

Two main lines of arguments are usually offered to account for this. One invokes the fact that employment relationships are often long term in nature. Consequently, even when there are short term incentives to, say, underreport the performance of the worker during a given year, the fear that the worker will retaliate by not working quite as hard the next year or by simply leaving the firm can prevent such opportunistic behavior on the part of the manager.[28] Examples of papers in the literature that have looked at these issues of repeated interactions and at how these create the possibility for cooperation between parties with diverging short-term interests are given by e.g. Bull (1987), MacLeod and Malcomson (1989), and Baker, Gibbons, and Murphy (1994). I will come back below on what sort of empirical implications can be tested using these models.

Another line of argumentation has raised the point that firms may penalize themselves by *not* rewarding superior performance when it occurs. Take the example of a firm deciding whether or not to promote a worker to a different job in the organization. The implicit agreement here is that the worker expects to get a promotion under the condition that she performs well. Assuming that the efficient assignment of that worker (efficient in the sense that the worker's productivity is higher in the new job) implies that the worker should be promoted, the firm would actually penalize itself by not allowing the worker to realize her potential for greater productivity. A related argument made by Carmichael (1983) is that, assuming firms can commit themselves to a fixed set of "prizes" (promotions), they would gain nothing by denying a promotion to a deserving worker since they would have to give it to somebody else in any case.

[28]One need not resort to terms like "retaliating". In other words, workers need not consciously penalize their employer by willingly lowering their effort provision. Demoralization or low motivation of the workforce due to perceived broken promises are common and have much the same effect even if workers think they are working just as hard.

7.2 Some Evidence

7.2.1 Determinants of Contract Form

By modeling a long term relationship as a repeated game between the firm and the worker, MacLeod and Malcomson (1989) first showed that each party can resist the temptation of not honoring their respective (implicit) promises only if fulfilling them creates a *surplus* with one party earnings at least as much as in its next best alternative and the other party earnings strictly more. If that condition is met, they showed that there exists a set of non trivial self-enforcing contracts in which both parties act honestly. It is called self-enforcing because the promises of higher effort on the part of the worker and of rewarding such effort by the firm cannot be legally enforced.

Possible sources of surplus include specific human capital and reputational concerns on the part of the firm: if workers realize that the firm is cheating all the time on its promises to reward high effort, then they may retaliate by withholding effort and thus produce less. Another source of surplus is involuntary unemployment. Similar to the idea of Shapiro and Stiglitz (1984), the fear of losing one's job may be sufficient to induce workers to provide effort. But for that to happen, it must be the case that the workers gain strictly more than in their next best alternative, otherwise nothing would prevent them from shirking. Consequently, when workers are "abundant" i.e. a relatively high unemployment rate, workers get all the surplus from the relationship. The firms cannot be given the surplus because they can hire workers very easily and thus it would be very difficult for them to resist the temptation to renege on the agreement. Conversely, if workers can get jobs very easily because the labor market is tight, then it would be difficult to give them the surplus. Firms will motivate workers by paying them a bonus as part of their compensation package. Workers will trust firms to pay the bonus because the cost of replacing workers who would quit if not paid the bonus would be high for the firm. On the other hand, firms could not be trusted to pay a bonus (after observing a high effort level) when unemployment is high because the cost of replacing the workers is low. Instead, to motivate the workers, fixed wages set above the workers' next best alternative are paid and the threat of firing is used to motivate them.

Thus, this model offers the testable empirical implication that there should be an inverse relationship between the use of bonuses and the unemployment rate. Fairly strong evidence in favor of this prediction is provided

is MacLeod and Parent (1999a) and also in MacLeod and Parent (1999b). In the first paper, we examine the relationship between bonus incidence and the unemployment rate defined at the level of the county, while in the second paper we find even stronger evidence when we look at the relationship between the amount paid in bonuses (using the PSID) and the unemployment rate.

Although these results provide support for the incomplete contracting model of MacLeod and Malcomson, it is nevertheless possible that the estimated relationship stems from simple rent sharing motives: in good years (i.e. low unemployment), firms have higher profits and may thus be more willing to pay bonuses. We would feel more confident about the interpretation we give to that result if we had some measures of firm profits. While certainly not a perfect substitute, it turns out that when we include year dummies fully interacted with industry dummies to reflect the time-varying conditions in a particular industry (including demand conditions), the results are actually stronger, not weaker. Still, more research in this area (with better data) seems warranted.

7.2.2 Unions and the Use of Bonuses

As we can see in Figure 3, the use of bonuses appears to be on the rise since the mid-seventies. Coincidentally, the fraction of workers covered by a collective bargaining agreement has decreased over the same time period which would suggest that union coverage wold tend not to be associated with the use of bonuses. Indeed, that's what we observe not only when we look at the aggregate data but also when we use a multivariate framework to control for other factors. It does appear that the declining rate of unionization has allowed firms to be more flexible in its compensation packages. Among the many tools that firms have to retain workers, one is to offer them wage increases. But, given the evidence we have on nominal wage rigidity (e.g. Bewley (1993) or McLaughlin (1999)) firms might very well prefer offering them bonuses which are by definition temporary instead of offering more "permanent" wage increases which cannot be undone so easily.

7.2.3 Productivity Effects of Implicit Contracts (and of Other HRM Practices)

In my mind, the most compelling empirical work done on the topic has been that of Ichniowski, Shaw, and Prennushi (1997). Although it is true they focus on just one narrowly defined sector, I think the benefits of not having to worry quite as much about unobserved heterogeneity outweigh the costs of perhaps not being able to readily generalize the results. By considering clusters of human resource management practices, they emphasize the complementarity between the different HRM practices and can they can measure the individual contribution of each.

One of the HRM practices considered in their paper is the use what they label as "line incentives" which reward not only the level of output achieved by workers on a production line, but also clearly more difficult to explicitly measure attributes like quality. As such, those line incentive plans have common features with implicit contracts.

The most important result from the paper is that incentive pay plans do have productivity effects, but *only* when used in combination with a host of other HRM practices such as flexible job assignments, training, sharing of information, etc. When only a subset of those other practices are used, the productivity effects of incentive pay are muted.

8 Rising Wage Profiles as an Incentive Device

8.1 Theory and Evidence

Earlier in Part 1, I briefly discussed how firms and/or workers might show a preference for explicit pay-for-performance contracts such as piece rates when the employment relationship is not expected to be of a long term nature. More specifically, as argued by Goldin (1986), the low labor force attachment of women at the turn of the century could have been the key factor behind the high incidence of piece rate contracts for women relative to men in the same occupation. For men, who are more likely to stay with the same employer over a longer period of time, Lazear (1981) suggested that firms might offer deferred compensation by promising workers that they will be paid more than their market value in the later portion of their tenure in exchange for being paid less than the value of their marginal product early on. The idea of tilting the wage profile is to give workers some incentives to stay with the firm long

enough so that they will be able to collect the promised rents. The implicit threat is that they will be fired if they are caught shirking and thus will be denied those rents. Lazear's result of a tilted wage profile is thus consistent with much of the early evidence on the existence of a positive return to tenure in addition to the usual return to experience.[29] Although wages that increase with tenure may result from the sharing of rents over firm-specific human capital, Lazear instead argues that the positive relationship between tenure and wages may have very little to do with the accumulation of firm-specific capital. Instead, it results mainly from the provision of life-cycle incentives.[30]

Lazear's model was to some extent a response to the evidence reported by Medoff and Abraham (1981) and Medoff and Abraham (1980) who showed with data from two large firms that the employees' wage increases were not related to the evaluation of their performance. In other words, pay increases did not seem to be related to productivity in their data. Those results are very hard to reconcile with human capital theory, but can be rationalized by appealing to a Lazear type mechanism. Also, Frank and Hutchens (1993) observe that airline pilots' wages increase with years of seniority, which is hard to reconcile with the notion that their productivity increases throughout the employment relationship.

A somewhat direct test of Lazear's model is offered in Lazear and Moore (1984) where they use self-employed workers as the benchmark. Given that self-employed workers are not, for obvious reasons, subject to agency problems, then there is no reason for them to use a Lazear-type mechanism to elicit effort. Consequently, you would expect their wage profile to be flatter than is the case for employed workers. Lazear and Moore find support for that prediction and consequently argue that most of the observed positive relationship between tenure and wages results from the provision of incentives, not from the accumulation of firm-specific capital.

[29]For an early account of a return to tenure, see Mincer and Jovanovic (1981) and Borjas (1981).

[30]In a related vein, Salop and Salop (1996) show that backloaded compensation contracts may be used to recruit inherently less "impatient" workers who will accept the lower starting wage in exchange for a higher wage later on. Those kind of workers are precisely the ones the firm would like to hire if it cared about developing long term relationships in which investment in firm specific skills were to be made. Thus, the self-selection of workers with different discount rates would bias upward any measured impact of acquiring skills on wages. This selection bias has nothing to do with incentive effects though, contrary to Lazear's model.

More recent evidence of back loaded compensation comes from the influential paper by Topel (1991) in which he estimates that workers with ten years of seniority earn on average about 25% more than other workers with the same labor market experience who are starting out in a job.

Although these two pieces of evidence would tend to provide support to the proposition that Lazear-type incentive schemes are commonly used, there is at least one problem associated with such a conclusion.[31] The latest evidence on the topic of the returns to tenure for employed workers in the United States suggests that it may actually be quite close to *zero* (see e.g. Altonji and Williams (1997), Neal (1995) and Parent (2000)). In fact, Neal shows that the wage of displaced workers in their post-displacement job is correlated with *pre*-displacement tenure, especially for workers who do not switch industries. This result is hard to reconcile with an incentive-based explanation for the relationship between tenure and wages as one would not expect incentive problems within a particular employment relationship to carry over into the next employment relationship. Consequently, if in fact wages do not rise with tenure, then this calls into question much of the literature on the reasons for deferred compensation.

9 Conclusion

In this chapter I reviewed the empirical and theoretical literature on both the determinants of the way workers are paid and on the effects of explicit incentive contracts. Although the use of incentives based on an explicit or contractible measure of performance are not very common, the data showed that firms use a wide array of other tools such as profit sharing plans and bonuses to motivate workers.

While the evidence surveyed in this paper points towards fairly large productivity effects, especially in the case of explicit incentive contracts, there remains the question of whether the identification strategies employed so far are completely satisfactory. Of course, the same comment can be made on just about any sort of empirical investigation. At least in the case of incentives we have in the Lazear model a useful tool to help us deal with that problem in that it makes explicit the connection between pay methods,

[31] See also Farber (1997) for a discussion about the lack of a tight theoretical connection between wages and the accumulation of firm-specific human capital which makes difficult the interpretation of a statistical relationship between the two.

skills, and the return to those skills. Further explorations of the implications of that model seem warranted.

Another topic that should be explored more fully is the link between profit sharing plans and skills. Although, as I mentioned in the text, the usual emphasis has been on the incentive effect of profit sharing, it might be at least as useful to consider profit sharing plans as the solution to a contracting problem in which firms would like to commit themselves to reward firm-specific skills but usually cannot, which in turn makes workers underinvest out of the concern that they will be held up. Additionally, it seems likely that if profit sharing and skill acquisition are linked, then workers will not only learn truly firm-specific skills but will also learn general skills in the process. If that is the case, then one would expect to see the wage/productivity effect of profit sharing plans to carry over into subsequent employment relationships. This is, on the surface at least, a readily testable proposition which, if confirmed, would shed doubts on the incentive motive for the presence of profit sharing plans.

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10 Appendix 1: The Data Sets

10.1 The Quality of Employment Survey, 1973-1977.

The Quality of Employment Survey, 1973 uses a national probability sample of persons 16 years old or older who are working for 20 or more hours a week. Three such separate surveys were conducted: in 1969-1970 (when it was named the Survey of Working Conditions), in 1973 and in 1977. The panel version used for some of the results in this paper consists of all those among the 1455 individuals interviewed in 1972-73 who were re-interviewed in 1977. All the usual information on wages, hours worked, industry, occupation, etc., in addition to individual characteristics, is collected. Of particular interest for this paper when I analyze the effect of the work environment on the observed method of pay is a series of self-reported measures on many job characteristics, such as the level of creativity required by one's job, the degree to which the respondent's job is repetitive, the level of skills required by the job, etc. More particularly, I make use of the following questions:

1. "MY JOB REQUIRES THAT I DO THE SAME THINGS OVER AND OVER" (variable name: job is repetitive).

2. "I GET TO DO A NUMBER OF DIFFERENT THINGS ON MY JOB" (variable name: variety of things to do).

3. "I HAVE A LOT OF SAY ABOUT WHAT HAPPENS ON MY JOB" (variable name: worker has a lot of say about what happens on her/his job).

4. "I DETERMINE THE SPEED AT WHICH I WORK" (variable name: worker sets own pace).

5. "MY JOB REQUIRES THAT I KEEP LEARNING NEW THINGS" (variable name: job makes worker learn new things).

6. "MY SUPERVISOR IS SUCCESSFUL IN GETTING PEOPLE TO WORK TOGETHER" (variable name: teamwork).

Answers are scaled in the following way for the first five questions: 1: STRONGLY DISAGREE; 2: DISAGREE; 4: AGREE; 5: STRONGLY AGREE. I have re-coded the last two possibilities to 1 and the first two to zero. The scaling

of the answers to the question about teamwork is: 1: NOT AT ALL TRUE; 2: A LITTLE TRUE; 3: SOMEWHAT TRUE; 4: VERY TRUE. Again, this variable is coded as 0 if the answer is 1 or 2 and to 1 if the answer is 3 or 4. Turning now to the method of pay, unfortunately that information is available only for the 1977 interview. We know whether people are paid salaries, hourly rates, piece rates, or commissions. We also know whether their employer is providing them with a profit sharing plan.[32]

10.2 National Longitudinal Survey of Youth (1988-1990)

The National Longitudinal Survey of Youth data set surveyed 12,686 young males and females who were between the age of 14 and 21 in 1979. In 1988, 1989, and 1990, respondents were asked whether all or part of their earnings were based on job performance. They were also asked a few questions on their work environment.

The question pertaining to pay-for-performance is the following:

"THE EARNINGS ON SOME JOBS ARE BASED ALL OR IN PART ON HOW A PERSON PERFORMS THE JOB (HAND CARD D). 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. PLEASE DO NOT INCLUDE PROFIT SHARING OR EMPLOYEE STOCK PURCHASE PLANS.

1. PIECE RATES.
2. COMMISSIONS.
3. BONUSES (BASED ON JOB PERFORMANCE).
4. STOCK OPTIONS.
5. TIPS.
6. OTHER."

They were also asked whether they had received a promotion on their current/most recent job since the last interview. We should note that it is not possible to tell a priori whether the bonuses refer to amounts paid at the discretion of the employer when the latter subjectively considers that the performance of the employee is worthy of a cash reward, or whether they

[32]See MacLeod and Parent (1999a) for details on how we used the job characteristics in the models of pay method determination and how we deal with the likely endogeneity of the responses to the job characteristics questions.

merely represent another form of piece rate. In the latter case, the employee gets a reward for achieving or surpassing some kind of quantitative target which can be objectively determined. A separate question was asked on whether their employer had a profit sharing plan.

Concerning some aspects of the work environment, although no questions pertaining to the characteristics of the jobs were asked during the 1988-1990 period, such questions were asked in 1979 and 1982. More specifically:

"WE WOULD LIKE TO KNOW WHAT KIND OF OPPORTUNITIES THIS JOB OFFERS YOU. (FIRST/NEXT) HOW MUCH OPPORTUNITY DOES THIS JOB GIVE YOU (READ CATEGORY)- A MINIMUM AMOUNT, NOT TOO MUCH, A MODERATE AMOUNT, QUITE A LOT, OR A MAXIMUM AMOUNT? [CATEGORIES]

1. TO DO A NUMBER OF THINGS (VARIETY).
2. DEAL WITH PEOPLE.
3. FOR INDEPENDENT THOUGHT OR ACTION (AUTONOMY).
4. FRIENDSHIPS.
5. TO DO A JOB FROM BEGINNING TO END (PROBE IF NECESSARY: THAT IS, THE CHANCE TO DO THE WHOLE JOB) (COMPLETE TASK)."

Answers are re-coded to 0 if respondents answer either "A MINIMUM AMOUNT", "NOT TOO MUCH", or "A MODERATE AMOUNT", while they are re-coded to 1 if respondents answered either one of the last two possibilities.

For each one of 20 occupation cells, I compute the average of the answers in both the 1979 and the 1982 surveys. I then merge these averages to each corresponding occupation category for the 1988-1990 period. Note that the local unemployment rate contained in the NLSY (as is the case with the PSID) is measured at the level of the county.

10.3 The Panel Study of Income Dynamics (1976-1991)

The sample consists of white male heads of households aged 18 to 64 with positive earnings for the period spanning the years 1976-1991.[33] Individuals in the public sector and who worked less than 500 hours are excluded from the

[33]In the PSID, data on hours worked during year t, as well as on total labor earnings, bonuses/commissions/overtime income, and overtime hours, are asked at the year t+1 interview. Thus we actually use data covering interview years 1976-1992.

analysis. We know whether each worker is paid a piece rate, a commission, an hourly rate or a salary (the structure of the question pertaining to method of pay is very similar to the one in the QES).

One interesting feature of the PSID for the 1976-91 period is the fact that one is able to determine whether a worker received a bonus over the last year. In the PSID questionnaire, workers are asked the amount of money they received from either working overtime, or from commissions, or from bonuses paid by the employer. Since we cannot separately identify the amount of income derived exclusively from commissions and/or overtime work, I have to remove these workers from the calculations.

10.4 The January 1977 Current Population Survey (Validation Survey)

A sub-sample of the January 1977 Current Population Survey were asked questions about hours worked, union coverage, earnings, and also about pay methods in addition to the usual questions on labor force status, schooling, demographics, industry affiliation and occupational status. They were also asked to provide the address and name of their employers so that the same set of questions could be asked to them. This represents the only instance in which such questions about contract form were asked in the CPS.[34] As is the case with the PSID, the CPS questionnaire allows us to compute the average amount paid in bonuses. More precisely, employers (and workers) are asked whether the workers received any extra amount in addition to their regular pay.

11 Appendix 2: Measurement Problems

As is well known from the classical measurement error model in which all variables are continuous, there are two main consequences of having error-ridden measures:

1. If y, a noisy measure of the true variable y^*, is the dependent variable in a multivariate regression model, the precision of the estimated parameters will be lower and the explanatory power of the model will also be lower. However, the estimated parameters would not be biased.

[34] Mellow and Sider (1983) provide a thorough analysis of that data set.

2. If y is the explanatory variable, then its associated parameter would be biased toward zero, i.e. we would tend to falsely accept the null hypothesis of a zero effect more often than we should. Again, if y is all noise, then it follows that its associated coefficient would be zero.

In short, the lesson is clear: measurement error makes it more difficult to find statistical relationships.

Now the special case of measurement error that is of particular interest for the literature surveyed in this paper is where the variable to be measured is dichotomous (0-1). Then the conclusion for the effect of measurement error the case of a continuous explanatory variable case carries through: misclassification of a variable biases the coefficient towards zero. However, contrary to the model with a continuous variable, if the *dependent* variable is misclassified the parameters associated with the explanatory variables are also biased.[35]

Given that this paper tries to estimate the effect of pay methods on wages and to study the determinants of those pay methods, we are facing a combination of those problems: misclassification error of the pay methods will tend to bias the estimated incentive effect toward zero while mismeasurement of wages will produce larger standard errors.

In addition, in the models that study the determinants of incentive pay contracts (e.g. Brown (1990)), the estimated effects are likely to be biased towards zero because 1) the independent variables, whether they are continuous or categorical, are noisy measures, and 2) the dependent 0-1 variable is sometimes misclassified. In part because of that, it is not surprising that studies examining the determinants of pay methods often find that relatively few variables enter significantly and that the explanatory power of the model is very low.

At the risk of overemphasizing the problem, it must be realized that measurement error, although it's a "fact of life" in empirical work, should be appropriately treated as a major worry.

But how much of a worry should it be? After all, if people do tend to accurately report the type of contract they have, then we need worry only about mismeasurement of the other variables of interest such as schooling, wages, etc. It turns out that we can get a fairly good idea of the underlying problem by exploiting the January 1977 Current Population Survey. Using

[35]See Hausman, Abrevaya, and Scott-Morton (1998) for a proof.

that data set, it is possible to compute a cross-tabulation of employer and employee responses to the question on how the workers were paid. If employers and employees' responses were always the same, then the diagonal elements of Table A1 should be equal to 100% and the off-diagonal elements should all be equal to zero. On the other hand, if employers and employees' responses were not the same then the "agreement rate" would be less then 100%. As it turns out, we can see from Table A1 that while employers and employees seem pretty much in agreement concerning hourly rates, the same cannot be said for "incentive" pay methods. Consequently, simply looking at Table A1 would suggest that we should not be surprised at the low explanatory of any model trying to analyze the determinants of pay-for-performance contracts.

Table 1
Mean Sample Statistics

	CPS (January 1977)*	QES (1973-77)	PSID (1984-1991)	NLSI (1988-1990)
Percentage paid				
Hourly	58.6	40.9	48.6	45.9
	(0.50)	(0.49)	(0.50)	(0.50)
Salary	37.5	51.1	38.5	54.1
	(0.48)	(0.50)	(0.49)	(0.50)
Piece rates	1.1	2.8	0.9	3.6
	(0.10)	(0.16)	(0.09)	(0.19)
Commissions	2.9	5.2	7.7	5.7
	(0.17)	(0.22)	(0.27)	(0.23)
Bonuses	8.4	–	10.6	14.1
	(0.2958)		(0.2902)	(0.35)
Profit sharing	–	0.1761	–	0.3319
Average amount of bonuses ($1979)*				
For hourly paid workers	1079.09	–	1203.35	–
	(1920.24)		(2935.30)	
For salaried workers	2313.05	–	4403.86	–
	(5237.76)		(8921.40)	
Sample size	4905	724	10803	8165

* Computed from workers' responses.

Table 2A
Pay Method by Occupation
Quality of Employment Survey 1977

Occupation	Hourly	Salary	Piece rate	Commission
Prof., tech, except eng. techn.	14.15%	83.02%	0.94%	1.89%
Engineering and science techn.	28.57%	71.43%	0.00%	0.00%
Writers, artists, etc.	0.00%	77.78%	0.00%	22.22%
Managers and admin. except farm	8.33%	81.82%	2.27%	7.58%
Sales workers	27.27%	9.09%	4.55%	59.09%
Clerical and unskilled 1 *	27.08%	64.58%	0.00%	8.33%
Office machine operators	30.77%	69.23%	0.00%	0.00%
Secretaries	17.65%	82.35%	0.00%	0.00%
Clerical and unskilled 2 **	46.15%	53.85%	0.00%	0.00%
Craftsmen and kindred 1 ***	59.02%	34.43%	4.92%	1.64%
Mechanics and repairmen	80.65%	17.74%	0.00%	1.61%
Operatives exc. precis. machines & text.	74.19%	22.58%	3.23%	0.00%
Precision machine operatives	68.75%	12.50%	18.75%	0.00%
Textile operators	92.31%	5.13%	2.56%	0.00%
Transport equip. operatives	63.33%	16.67%	10.00%	10.00%
Laborers, except farm	85.00%	15.00%	0.00%	0.00%
Cleaning service workers	44.44%	44.44%	11.11%	0.00%
Food service workers	80.00%	20.00%	0.00%	0.00%
Health service workers	55.56%	44.44%	0.00%	0.00%
Personal service workers	23.08%	61.54%	7.69%	7.69%

* From bank tellers to meter readers for utilities (Census 301 to 334)
** From shipping clerks to ticket agents and other misc clerks (Census 374 to 395)
*** From auto access. installers to machinist apprentices (Census 401 to 462)

Table 2B
Pay Method/Promotions by Occupation
National Longitudinal Survey of Youth 1988-90

Occupation	Hourly	Salary	Piece Rate	Commission	Bonus	Profit Sharing	Promotions
Prof., tech, except eng. techn.	27.94%	72.06%	0.43%	1.99%	15.46%	35.60%	13.76%
Engineering and science techn.	42.37%	57.63%	0.00%	5.09%	9.32%	41.25%	18.64%
Writers, artists, etc.	21.84%	78.16%	2.30%	9.20%	17.24%	30.13%	14.94%
Managers and admin. except farm	19.98%	80.02%	0.68%	9.84%	28.46%	45.26%	18.91%
Sales workers	25.07%	74.94%	0.78%	37.98%	25.58%	39.29%	11.37%
Clerical and unskilled 1*	43.18%	56.83%	1.34%	3.12%	13.21%	44.55%	16.32%
Office machine operators	43.88%	56.12%	0.84%	1.27%	13.50%	52.88%	14.77%
Secretaries	37.20%	62.80%	1.02%	1.37%	11.60%	33.40%	13.99%
Clerical and unskilled 2**	48.76%	51.24%	1.99%	1.74%	10.20%	42.91%	14.93%
Craftsmen and kindred 1***	60.32%	39.68%	2.67%	1.60%	10.68%	28.12%	17.97%
Mechanics and repairmen	53.16%	46.84%	4.54%	9.56%	9.89%	24.61%	12.16%
Operatives exc. precis. machines & text.	68.93%	31.07%	8.75%	1.79%	10.54%	30.51%	7.32%
Precision machine operatives	60.44%	39.56%	36.81%	1.10%	9.34%	37.31%	10.44%
Textile operators	66.67%	33.33%	9.76%	0.71%	11.43%	41.57%	10.00%
Transport equip. operatives	50.48%	49.52%	3.38%	8.21%	13.53%	32.06%	10.14%
Laborers, except farm	60.71%	39.29%	6.02%	1.88%	10.34%	22.22%	13.16%
Cleaning service workers	54.46%	45.55%	1.49%	0.50%	7.43%	23.35%	9.90%
Food service workers	52.46%	47.55%	0.52%	1.29%	7.49%	13.39%	11.37%
Health service workers	65.99%	34.01%	2.03%	0.51%	8.63%	16.05%	9.65%
Personal service workers	36.81%	63.19%	1.84%	20.25%	9.20%	18.03%	9.82%

* From bank tellers to meter readers for utilities (Census 301 to 334)
** From shipping clerks to ticket agents and other misc clerks (Census 374 to 395)
*** From auto access. installers to machinist apprentices (Census 401 to 462)

Table 3A
Average Job Characteristics by Occupation
Quality of Employment Survey

Occupation	Repetitiveness	Variety of things	Worker has say	Learn new things	Teamwork	Set own pace
Prof., tech, except eng. techn.	51.6%	94.8%	92.3%	95.8%	57.8%	75.8%
Engineering and science techn.	58.4%	80.7%	52.9%	94.6%	82.5%	88.4%
Writers, artists, etc.	28.0%	96.1%	96.1%	96.1%	58.4%	58.4%
Managers and admin. except farm	60.5%	97.7%	93.2%	94.5%	41.3%	84.4%
Sales workers	59.3%	79.7%	85.9%	92.0%	42.4%	77.1%
Clerical and unskilled 1*	74.9%	75.5%	65.0%	80.8%	67.1%	86.9%
Office machine operators	83.9%	75.8%	48.7%	86.6%	62.6%	62.6%
Secretaries	64.1%	88.7%	49.3%	78.9%	76.4%	71.6%
Clerical and unskilled 2**	81.1%	82.0%	48.8%	76.5%	69.3%	71.7%
Craftsmen and kindred 1***	65.5%	83.4%	68.8%	87.7%	69.2%	79.7%
Mechanics and repairmen	63.2%	85.3%	68.1%	84.7%	48.9%	79.1%
Operatives exc. precis. machines & text.	77.4%	52.7%	45.3%	56.0%	66.0%	54.5%
Precision machine operatives	84.1%	53.7%	51.9%	48.3%	56.4%	62.6%
Textile operators	77.5%	52.6%	40.2%	56.4%	56.3%	63.7%
Transport equip. operatives	86.8%	62.0%	48.5%	60.9%	52.8%	81.9%
Laborers, except farm	97.6%	85.4%	46.1%	56.9%	76.4%	90.7%
Cleaning service workers	79.6%	88.1%	76.7%	73.9%	28.7%	78.9%
Food service workers	89.1%	76.4%	52.5%	62.1%	64.9%	76.7%
Health service workers	76.6%	67.7%	41.2%	91.3%	54.6%	54.6%
Personal service workers	68.8%	80.1%	66.5%	83.4%	44.3%	76.7%

* From bank tellers to meter readers for utilities (Census 301 to 334)
**From shipping clerks to ticket agents and other misc clerks (Census 374 to 395)
***From auto access. installers to machinist apprentices (Census 401 to 462)
Cell entries represent the percentage of workers who consider their job as having the various features listed.

Table 3B
Average Job Characteristics by Occupation
National Longitudinal Survey of Youth

Occupation	Autonomy	Complete task	Variety of tasks	Friendship	Deal with people
Prof., tech, except eng. techn.	57.36%	73.81%	55.41%	56.93%	76.41%
Engineering and science techn.	61.59%	78.81%	56.95%	58.28%	65.56%
Writers, artists, etc.	67.68%	76.77%	54.55%	65.66%	73.74%
Managers and admin. except farm	61.13%	77.48%	64.61%	56.84%	90.08%
Sales workers	42.08%	63.34%	30.67%	55.92%	84.02%
Clerical and unskilled 1*	32.77%	65.88%	30.67%	49.87%	72.79%
Office machine operators	39.75%	62.46%	38.80%	48.90%	59.62%
Secretaries	44.95%	73.70%	51.07%	54.74%	72.48%
Clerical and unskilled 2**	39.13%	67.69%	37.02%	49.47%	63.69%
Craftsmen and kindred 1***	47.10%	71.65%	47.99%	56.03%	46.65%
Mechanics and repairmen	48.48%	73.19%	48.96%	50.40%	52.49%
Operatives exc. precis. machines & text.	32.64%	57.46%	33.01%	48.29%	44.99%
Precision machine operatives	28.92%	48.59%	26.51%	50.20%	33.33%
Textile operators	31.73%	59.37%	33.21%	47.31%	33.21%
Transport equip. operatives	40.25%	65.50%	33.25%	48.50%	62.25%
Laborers, except farm	33.04%	62.73%	30.89%	48.44%	45.49%
Cleaning service workers	34.93%	64.95%	27.34%	40.07%	33.65%
Food service workers	28.26%	64.37%	22.19%	54.45%	70.57%
Health service workers	37.99%	68.16%	39.94%	62.01%	82.12%
Personal service workers	47.96%	66.19%	33.09%	58.03%	74.10%

* From bank tellers to meter readers for utilities (Census 301 to 334)
**From shipping clerks to ticket agents and other misc clerks (Census 374 to 395)
***From auto access. installers to machinist apprentices (Census 401 to 462)
Cell entries represent the percentage of workers who consider their job as having the various features listed.

Table 4
Job Characteristics and Piece Rate/Commission Contracts

PAPERS	Gender	Presence of union	Variety of tasks	Worker autonomy	Capital intensity	Establish-ment size	Teamwork
Brown (1990)							
Data sets: Industry Wage Surveys + Dictionary of Occupational Titles(DOT)							
Pay method: Piece rates vs Time rates	++	0	--		0	++	
MacLeod and Parent (1999)							
Data sets: NLSY, PSID, QES, CPS							
Pay method: Piece rates vs Hourly rates	+	0	--	++			-
Commissions vs Salaries	--	0	--	++			-
Garen (1998a)							
Data sets: CPS + DOT							
Pay method: Hourly rates vs Salaries				++			
Geddes and Heywood (2000)							
Data set: NLSY							
Pay method: Piece rates vs Time rates/Salaries	++	0					
Commissions vs Time rates/Salaries	--	-					

++ (--) denotes strong evidence of positive (negative) association effect while + or – denotes weaker evidence.

Table 5
Wage Effect of Profit Sharing Plans, NLSY (1988-1990)
Dependent variable: Log of hourly earnings

	OLS	Within-Worker	Within-Job
PART A: Men			
Profit sharing indicator	0.0811	0.0396	0.0359
	(0.0195)	(0.0134)	(0.0141)
R-Squared	0.4314	0.8927	0.9434
Sample size: 4582			
PART B: Women			
Profit sharing indicator	0.0475	0.0004	-0.0005
	(0.0125)	(0.0149)	(0.0173)
R-Squared	0.4291		
Sample size: 3555			

The R-Squared statistics for the fixed-effect models are only indicative of a better fit: without an intercept, they cannot be interpreted in the same way as in the case of models with intercepts.

Table A1
Cross-Tabulation of Employer and Employee Reports of Pay Methods
January 1977 CPS-Percent of Number of Employee Reports

	Employer Report (same categories)				
Employee Report					
Hourly rate	0.9109	0.0753	0.0091	0.0012	0.0036
Salary	0.1297	0.8446	0.0013	0.0175	0.0069
Piece rate	0.2059	0.0000	0.7647	0.0294	0.0000
Commission	0.1238	0.1524	0.0095	0.7048	0.0095
Daily	0.2051	0.2307	0.0000	0.0000	0.5641

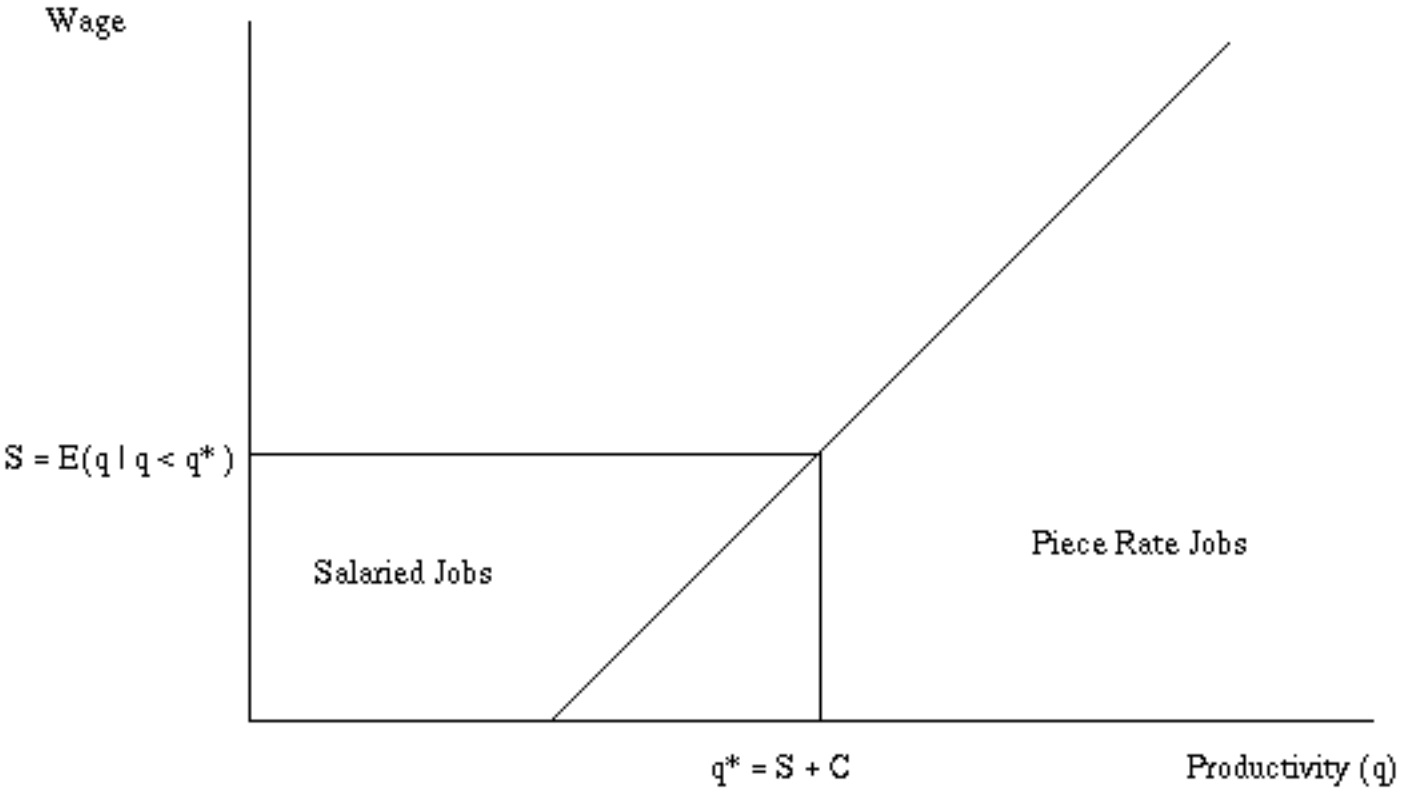


Figure 1. Optimal Sorting of Workers

Figure 2. Job Characteristics and Pay Methods

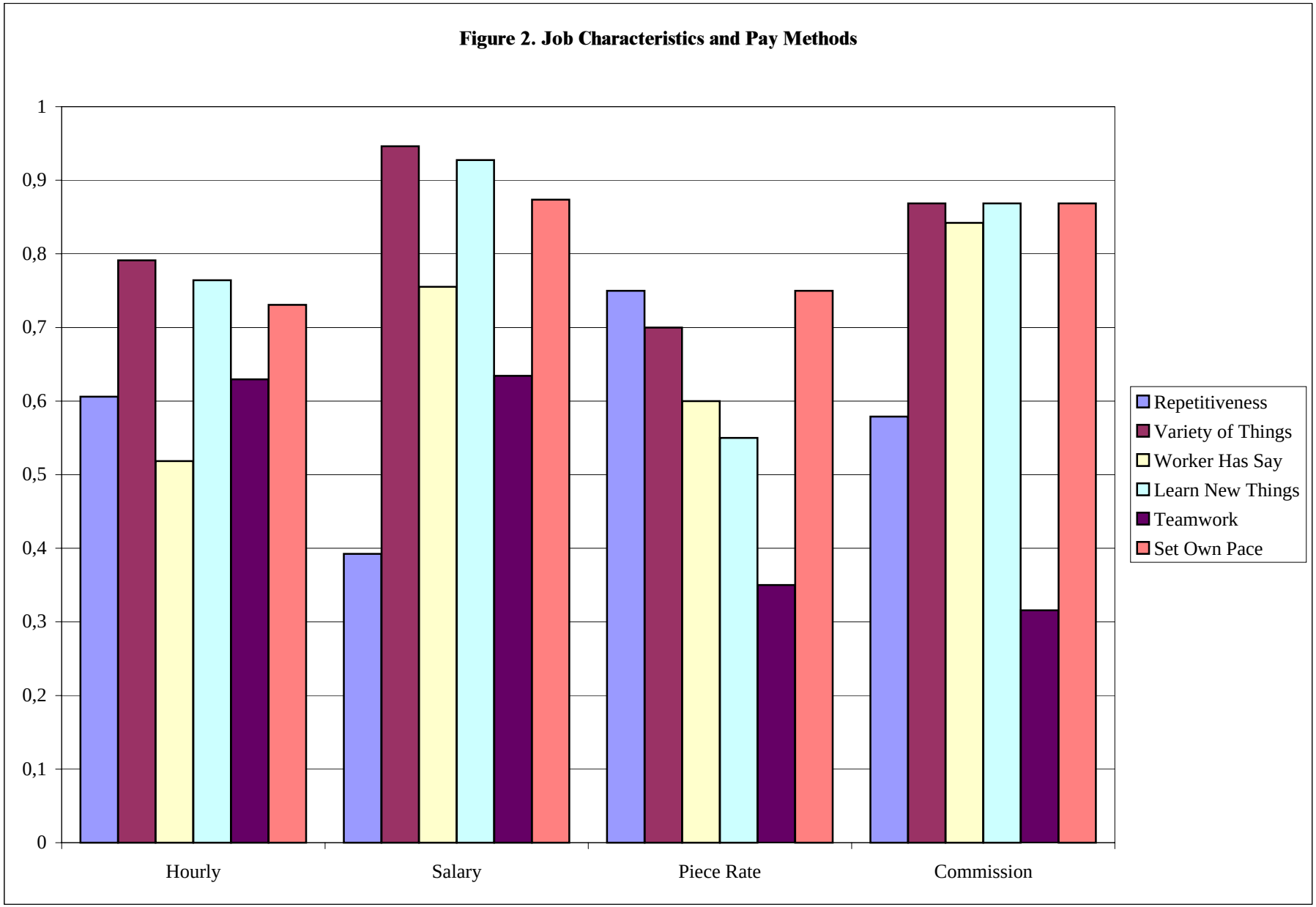
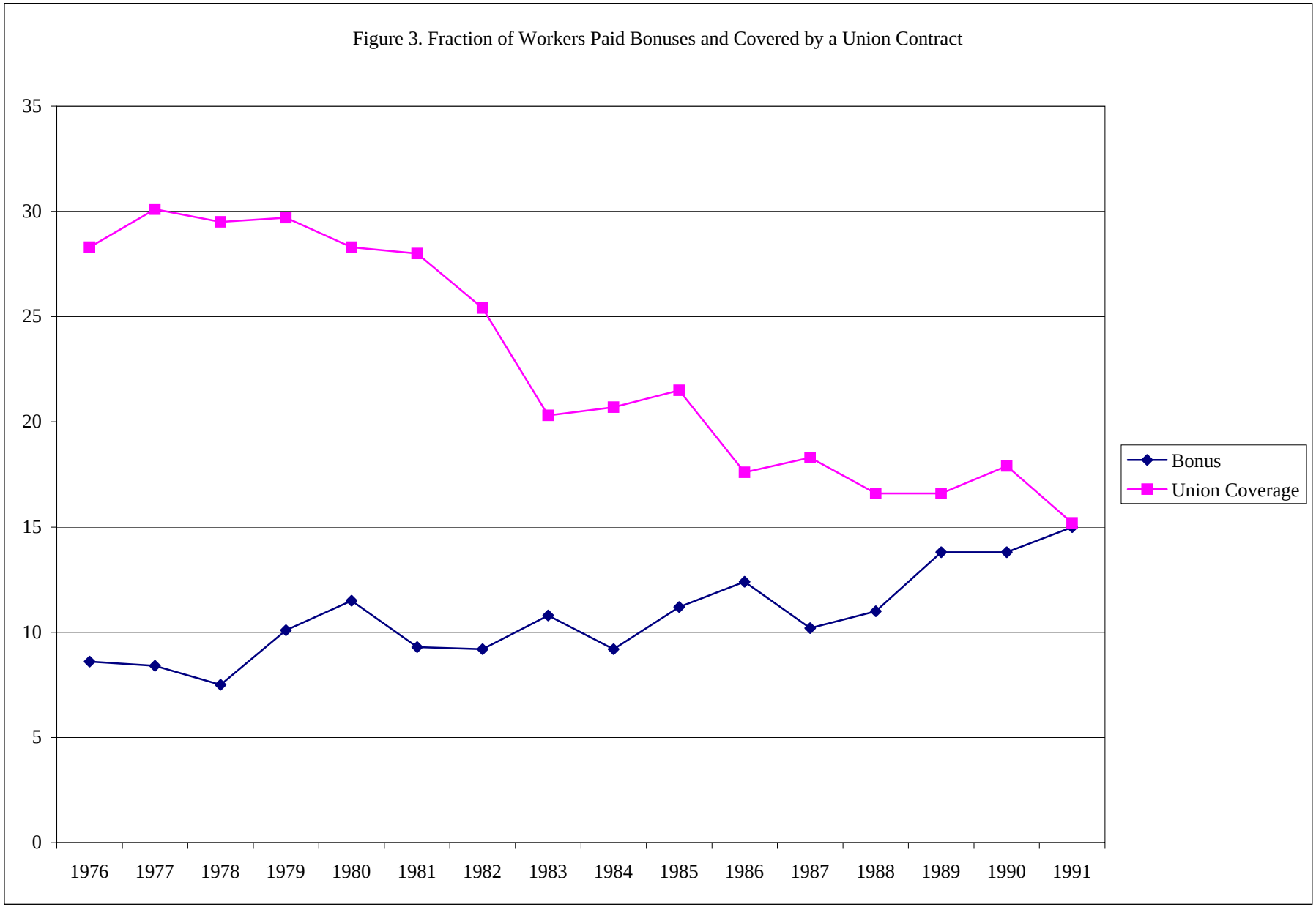


Figure 3. Fraction of Workers Paid Bonuses and Covered by a Union Contract



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