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# The Effect of Pay-for-Performance Contracts on Wages

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# The Effect of Pay-for-Performance Contracts on Wages*

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**Résumé / Abstract**

Cet article cherche à évaluer le rôle joué par les avantages comparés eu égard au résultat fréquemment rencontré qui veut que les travailleurs payés à la pièce ou par commission soient mieux rémunérés que les travailleurs salariés (incluant ceux qui sont payés à l'heure). Selon le modèle de Lazear (1986), la sensibilité de la rémunération à la pièce par rapport à la productivité est plus grande que dans les boulots salariés. Les avantages comparés impliquent donc que les travailleurs intrinsèquement plus productifs choisiront des contrats dans lesquels la rémunération est à la pièce. Ce processus d'auto-sélection selon les avantages comparés a comme conséquence que l'utilisation des méthodes dites d'effets fixes donnera des estimés biasés du véritable effet incitatif de ce type de contrat. Avec des données du National Longitudinal Survey of Youth et du Panel Study of Income Dynamics, je trouve que l'auto-sélection selon lesavantages comparé de même que l'apprentissage des caractéristiques individuelles semblent jouer un rôle important dans les cas des travailleurs qui sont soit en début de carrière ou qui débutent une nouvelle relation d'emploi. En ce qui concerne les travailleurs plus âgés, le rendement à la productivité est essentiellement le même peu importe la méthode de rémunération employée par la firme. Enfin, j'estime un effet incitatif d'environ 11 % en exploitant les variations dans la méthode de rémunération pour un même individu à l'intérieur d'une relation d'emploi.

*In this paper, I investigate the role played by learning and self-selection according to comparative advantage in the often reported result that piece rate workers (including commissions) earn more on average compared to other workers. With comparative advantage, the returns to skills are different according to whether one works under a piece rate contract or not. If that is the case, as Lazear (1986)'s model suggests, then using standard fixed effects methods will not provide consistent estimates of the true causal (or incentive) effect of explicit contracts. Using non-linear instrumental variable techniques with data from the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics, I find that comparative advantage along with learning about worker skills seem to play a significant role for workers who are either at an early stage in their career or who are observed for the first time in a given job-match, when the learning process matters. In other words, for those younger/early tenure workers, the*

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*return to skills is lower in non-incentive jobs. For older workers, the return to skills is basically the same across pay methods, which is consistent with the notion that workers are eventually paid according to their fully revealed skill level, irrespective of how they are paid. Finally, by exploiting the within-job variation in pay methods, I am able to identify an incentive effect of about 11%.*

**Mots Clés :** Rémunération à la pièce, auto-sélection, effets incitatifs

**Keywords:** Pay-for-performance, self-selection, incentive effects

# 1 Introduction

There is a growing body of evidence which shows that "performance pay" workers (piece rate or commission workers) earn more on average compared to salaried or hourly rated workers (see, e.g., Pencavel (1977), Seiler (1984), Brown (1992), Ewing (1996), Lazear (1996)). One can think of two main explanations for this relationship between explicit pay-for-performance contracts and the wage structure. On the one hand, it is possible that piece rate workers have stronger incentives to work hard. However, even absent any incentive effects, the self-selection of inherently more productive workers into those jobs will produce a positive correlation between average earnings and the occurrence of pay-for-performance contracts (see Lazear (1986)). More productive workers will self-select into those jobs because only for them is it worth it to indirectly pay the (higher) monitoring costs associated with piece rates through reduced wages. Recent papers by Lazear (1996) and Parent (1999) have attempted to control for this selection effect by exploiting the longitudinal dimension of the data sets employed to control for unobserved worker productivity and job-match quality. In Lazear's case study, he was able to exploit the fact that the workers' pay method changed from an hourly rate to a piece rate to do a simple before-after comparison in productivity, wages, turnover, and absenteeism. His results showed a substantial increase in productivity that was partly the result of selection effects and partly the result of a "pure incentive effect". With a very different data set (the National Longitudinal Survey of Youth (NLSY)), Parent (1999) qualitatively found the same result in terms of the impact on wages. In both papers, the underlying assumptions are that the return to skills is the same across pay methods and that conditional on the time-invariant unobserved productivity component (as well as on all observables), the choice of pay methods is strictly exogenous. While this rationalizes the use of fixed-effect methods to control for unobserved factors, it may not provide consistent estimates of the true causal (or incentive) effect of explicit contracts in the event that, as implied by comparative advantage, the return to skills do vary with the form of compensation.

The objective in this paper is to determine the extent to which workers do select themselves into jobs offering different methods of compensation based on comparative advantage and on learning about one's skills. This later aspect is important because it influences both the initial choice of a compensation scheme and the decision to eventually switch pay methods. To empirically study these issues, I make use of the method of moments estimation methodology developed by Lemieux (1998) and applied in the context of the effect of unions on the structure of wages.[1]

The Lazear (1986) model (see also Brown (1990)) predicts that wages should be more sensitive to skills in piece rate jobs. Therefore, assessing the empirical validity of this prediction is of obvious interest. For example, if the return to skills is not very different across pay methods, thus suggesting that comparative advantage considerations may not be all that important, then this may call into question some of the assumptions underlying the theoretical model. Note also that this would validate the use of simple fixed-effect methods. In fact, if selection was completely random, one would not even need to use first-differences: ordinary least squares would suffice to produce an unbiased estimate of the incentive effect. However, researchers routinely use first-differences to control for non random selection even though it is not clear that it necessarily follows from an *economic* model of self-selection.

In a related vein, Prendergast (2000) notes that explicit incentive contracts tend to be found in environments with often large random fluctuations, such as sales. This observation seems at odds with the notion that

[1] See also Gibbons, Katz, and Lemieux (1997) for a closely related application to inter-industry wage differentials in which workers sort themselves across industries based on comparative advantage and on learning about their initially unobserved skills. I come back below on why learning is a necessary ingredient in the present context to make the model empirically meaningful.

incentives are offered in less risky environment, as the basic principal-agent model would suggest, yet is consistent with the fact that the empirical evidence on the trade-off between risks and incentives is somewhat controversial, some authors finding support for the predicted tradeoff (e.g. Aggarwal and Samwick (1999)) while others do not (Garen (1994)). Prendergast then notes that in relatively stable or less risky environments, firms may intensify the monitoring of inputs simply because the link between effort and output is quite direct. Consequently it is not clear that monitoring considerations are completely absent from non-incentive jobs. If that is the case, one may expect to find that perhaps the sensitivity of output and wages to skills is not that different in piece rate jobs compared to time rated/salaried jobs.

Many other models of optimal sorting rely on the notion of comparative advantage. For example, Rosen (1982) develops a model in which it is optimal for high ability people to be sorted in jobs where their "impact" is the greatest, such as in executive jobs. As a consequence, the earnings distribution will be skewed because those at the top will tend to see their wages grow more than proportionately with respect to their skills. Hence, the return to skills will be higher in "high impact" jobs. A related model combining learning and human capital accumulation can be found in Gibbons and Waldman (1999). Also, MacLeod and Malcomson (1988) explore the endogenous creation of a hierarchy in an environment characterized by moral hazard and adverse selection. Workers are initially pooled and through the accumulation of output observations, the firm optimally assigns the best/hardest working among them to the higher ranks. This implies that pay should not be very sensitive to skill differentials for new labor market entrant. In contrast, older workers, about whom all available information has been accumulated will be sorted to the ranks that correspond to their skill level. Consequently, we would expect to observe that the return to skills is lower for younger compared to older workers, all else being equal.

The focus in this paper is not on testing whether workers progress through the ranks according to their comparative advantage. Instead, I more narrowly focus on the related question of whether wages reflect the fact that piece rate contracts essentially reveal worker productivity immediately while salaries or hourly rates may not do so initially. If this is true, then we should see evidence of differential returns to skills across pay methods. Also, taking into account this selection process, I want to estimate whether switching workers to piece rate contracts can be expected to have a true incentive effect.

With data from the National Longitudinal Survey of Youth (NLSY) and the Panel Study of Income Dynamics (PSID), the results show that unobserved skills are rewarded differently in "incentive" jobs compared to jobs which pay either hourly rates or salaries. Interestingly, this is true only in the case of workers who are either early in their careers, such as the workers surveyed in the NLSY, or who are observed for the first time in a given employment relationship. If I focus only on job stayers only, there is somewhat less evidence that self-selection into piece rate contracts according to comparative advantage matters a great deal, although I still find evidence of it in the case of younger workers in both the PSID and the NLSY.

Thus, on the one hand, the results provide support to the idea that comparative advantage does play a role. Secondly, the fact that the return to skills does not seem to vary across pay methods in the case of either older workers or workers repeatedly observed in the same job is suggestive that whatever information labor market participants need to learn about the productivity of the workers is fully revealed eventually. In addition, even though selection according to comparative advantage is shown to be empirically relevant, I provide evidence that the selection mechanism does not appear to be "one-sided" in the sense that workers at both ends of the skill distribution seem to choose piece rate contracts. Although this is not consistent with the basic Lazear model in which selection is one-sided, it can easily be reconciled with Brown (1990)'s reformulation of the Lazear model in which workers are heterogeneous in their cost of effort function and effort is increasingly costly for a given worker.

The results on the extent of the "true" incentive effect are somewhat mixed. On the one hand, results obtained by exploiting the within-worker variation in pay methods suggest that selection effects are the primary reason for the often reported result that "incentive" workers are paid more than others. This is particularly true in the case of the NLSY, but also for the subset of workers in the PSID who are in the same age range as those in the NLSY. On the other hand, if I use instead the within-job variation (thus controlling for any unobserved matching effect), the results using the PSID point toward a true wage effect of about 11 percent. Interestingly, it turns out that while the measured effect of explicit pay-for-performance contracts on wages is within range of the wage impact estimated previously in the literature, it is substantially *larger* than the impact estimated with standard fixed-effects, at least in the case of the PSID. This latter, perhaps surprising, result stems mainly from taking into account the endogeneity of the change in pay method. This endogeneity problem is caused by either learning effects or, more likely, by misclassification of pay methods. As is well known (e.g. see Card (1996)), this aspect is a potentially important source of attenuation bias in a wage equation, especially in a longitudinal setting.

The paper is structured as follows. The next section summarizes previous results in the literature. Section 3 then briefly outlines the Lazear model and its optimal assignment rule in which workers with more skills end up in jobs where such skills have a higher return. I also outline Browns' reformulation in terms of workers being homogeneous in productive ability but heterogeneous as to the cost of effort. The estimation methodology is discussed next in Section 4. The data are presented in Section 5 and the results in Section 6. The paper concludes with Section 7.

# 2 Previous Results on the Identification of Incentive Effects

Although researchers have in the past found that "incentive" workers earn more on average than other similar workers, much of the evidence came from cross-sectional data and one can never rule out that the wage premium earned by performance-pay workers in these data was simply the result of confounding effects, such as unobserved worker productivity or systematically different unobserved working conditions.

One way to at least partly get around those problems is to use panel data. Three recent papers, two of which being case-studies, have exploited that dimension. Lazear (1996) measures the changes in productivity and wages following the decision by a large auto glass company to switch pay methods, going from time rates to piece rates. He finds large effects on productivity (24% when one controls for selection effects through first-differences). Also, controlling for worker quality, he finds that the switch involved an increase of about 9% in wages. Using three straight waves of the National Longitudinal Survey of Youth, Parent (1999) finds that controlling for unobserved worker and job-match effects, piece rate workers still earn a 6-7% wage premium. Unfortunately, it is impossible with the NLSY to determine the productivity effect, although one could argue that unless firms have some monopsony power, workers should be paid according to the value of their marginal product. Shearer (1999), on the other hand, solved the self-selection issue by having a tree planting company in British Columbia randomly assign some of its workers to either fixed wages or piece rates. The productivity of the workers was recorded on a daily basis and controls were included for land quality.[2] His experimental results indicate that the incentive effect of paying workers a piece rate is about 20%, and it should be emphasized that given

[2] Some plots were flat and thus workers could plant a lot of trees in a day, whereas others were steeply sloped with a lot of debris on the ground, which made it very difficult for the workers.

the nature of the experiment, his results do not suffer from the potential biases that selection according to comparative advantage might create.

The goal of this paper is to find out, using non-experimental data, whether such a selection mechanism is important, which has implications on how confident we can be in interpreting standard fixed-effect estimates as representing the true incentive effects of piece rate contracts. Put differently, this paper's objective is to see whether one can readily generalize the existing results from case studies to the overall population of workers.

# 3 Theoretical Considerations

## 3.1 Relationship between Piece Rates and Wages

As shown in Lazear (1986), different methods of pay allow workers to sort themselves among firms. Zero expected profits are assumed throughout and we also assume that workers know their productive ability but not the firms unless they incur a monitoring cost M. Thus workers can be in a firm that pays a salary S which is independent of productivity $\alpha$ or in a piece rate firm that pays a wage $W = \alpha - M$. Thus the worker chooses the piece rate firm if and only if:

$$\alpha - M > S$$

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:

$$S = E(\alpha|\alpha < \alpha*)$$

where $\alpha* = S + M$. 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:[3]

$$ln w_m = a_m + b_m \alpha$$

with m=(salary,piece rate) and

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

This optimal sorting of workers is illustrated in Figure 1 in the case where $b_s = 0$.

## 3.2 Brown's Reformulation

Note that the foregoing model says nothing about the incentives provided to a given worker, which are commonly thought to be the reason firms might want to offer explicit pay-for-performance contracts in the first place. To examine the effort dimension, Brown (1990) reformulates the problem by assuming that homogeneous workers in terms of productivity are heterogeneous in terms of their effort cost function. In addition, he assumes that firms offering salaries can costlessly enforce a minimum level of effort $\overline{E}$.

Let the worker's utility function be expressed as:

$$U(w, e) = w - C(e)$$

where w is the wage, e is effort and we have $C' > 0$ and $C'' > 0$. For example, one could assume the following functional form:

$$U(w, e) = w - \frac{e^2}{N}$$

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

where N indexes the worker's "energy" level. High N workers can more easily work harder. The worker's problem is then to choose the job which provides the greatest utility. With the firm not monitoring effort in the salary job, the worker chooses $e^* = \overline{E}$ and gets utility $U = S - \frac{\overline{E}^2}{N}$ where, again, S is the constant salary paid by the firm. Assuming, as in the previous paragraph, that the wage in the piece rate job is $w = e - M$, where is M the monitoring cost, the piece rate worker chooses an effort level that maximizes the following expression:

$$U(e) = e - M - \frac{e^2}{N}$$

The first order condition for this problem is such that

$$e^* = \frac{N}{2}$$

with utility given by

$$U(e^*) = \frac{N}{4} - M$$

From figure 2, we can see that, given the chosen parameterization, not only will "high energy" workers self-select into piece rate jobs but so will low energy ones as well.[4] Consequently, even though the problem is reformulated in terms of the choice of "effort", we still basically have a self-selection model, the major difference with the basic Lazear model being that we do not have a "single threshold" selection mechanism. Instead, both low and high cost of effort workers may prefer piece rates. I will present below some evidence on the nature of the selection process. This is potentially important because standard selectivity correction procedures, such as Heckman's two-step estimator, provide consistent estimates of the parameters of interest only when selection is one-sided.

[4]Brown fully recognized this possibility, but dismissed its empirical importance based on the results in e.g. Pencavel (1977) and Seiler (1984). Consequently, he focused on the one-sidedness of the selection effect.

## 3.3 Observed and unobserved skills

The reason that pay is not as sensitive to skills in salaried/hourly paid jobs compared to incentive jobs is that firms do not take the trouble of monitoring performance to reveal the exact skill level of its workers. In other words, as pointed out in the previous section, if skills were all perfectly observables there would be no salary jobs and everyone would be paid a piece rate.[5] In the present context, I assume that the standard controls such as years of completed education, age, and health conditions are in fact observed by all labor market participants. Consequently, there is no reason that firms should pay for them differently according to the pay method.[6]

The more interesting case involves skills which are unobserved to both the econometrician and all labor market participants. Two types of contracts are offered in the labor market, piece rate contracts and straight salaries. Salaries are assumed to be set at the start of each period based on all the information available. Firms and workers alike form beliefs about the workers' productivity based on some prior distribution and they update those beliefs in a standard Bayesian manner after observing the output at the end of each period.[7]More specifically, let $\alpha_i$ be the unobserved ability of worker i. The standard normal learning model assumes that $\alpha_i$ comes from a prior distribution and each period all participants observe an output signal and update their beliefs about $\alpha_i$. Let $m_{it}$ be the best predictor of $\alpha_i$ given the full

[5]Leaving aside, of course, important issues such as multitasking which may render counter-productive the the use of piece rates even when skills are perfectly observable.

[6]In other contexts, it makes sense to assume that the returns to all observable dimensions are rewarded differently depending on the institutional environment. For example, in Lemieux (1998)'s union case, pay compression is likely to be a union policy. Consequently, highly educated individuals may be rewarded differently in the union vs. non union sector. Another way of justifying the assumption that observable skills are equally rewarded in all firms, irrespective of the pay method, is to observe that most commission or piece rate workers earn a base salary or a base hourly rate. This base pay would reflect differences in skills across workers.

[7]See Farber and Gibbons (1996) for a full exposition of this bayesian learning mechanism and an application to wage dynamics. The learning apparatus is also essentially the same as the one developed in Jovanovic (1979)'s matching model.

history of output signals up to period t-1. The main result is that $m_{it}$ is a martingale:

$$m_{it} = m_{it-1} + v_{it} \tag{1}$$

where $v_{it}$ is the innovation term orthogonal to $m_{it-1}$. As we will see below, this martingale property, even in the absence of comparative advantage, renders fixed-effect inconsistent because, in fact, the unobserved productivity component is not fixed. I turn next to the empirical implementation of a self-selection model according to comparative advantage and in which learning about unobserved skills is allowed.

# 4 Empirical Framework

## 4.1 The Model

In this section I outline the method-of moments estimator proposed by Lemieux (1998) in the context of the effect of unions on the wage structure. The application here is a straightforward adaptation of Lemieux's methodology with the exception that I will also take into account learning on the part of labor market participants, as is done, in e.g. Gibbons, Katz, and Lemieux (1997).

Assuming for the moment that the unobserved productivity component is fixed, let the log wage of worker i at time t be written as:

$$lnw_{it}^p = \delta^p + X_{it}\beta + \alpha_i + \eta_i^p$$

in piece rate jobs, and

$$lnw_{it}^s = \delta^s + X_{it}\beta + \psi\alpha_i + \eta_i^s$$

in salaried jobs, where $X_{it}$ is a vector of controls including age, education, region, etc., $\alpha_i$ is a time-invariant unobserved (to the econometrician) productivity parameter, $\delta^p$ and $\delta^s$ are pay method-specific intercepts, $\psi$ is the return to skill parameter in salaried jobs (it is normalized to one in piece rate jobs), and the $\eta's$ represent residual terms. If I re-write this expression in concentrated form we have:

$$lnw_{it} = \delta^s + X_{it}\beta + P_{it}\overline{\delta} + \alpha_i[P_{it} + \psi(1 - P_{it})] + \varepsilon_{it} \quad (2)$$

where $P_{it}$ is a performance pay dummy indicator, $\overline{\delta} = \delta^p - \delta^s$ represents the "true" average effect of performance pay contracts on wages (excluding selection effects), and $\varepsilon_{it}$ is the residual error term. According to the model presented in the previous section, we would therefore expect to have $\psi < 1$ for salaried workers. In fact, in the extreme case, $\psi = 0$ i.e. compensation does not depend at all on skills.

In standard fixed-effect models, $\psi = 1$ regardless of the method of pay and therefore can be differenced out. In general though, as is clear from equation.(2), first-differences methods will not produce consistent estimates of ($\overline{\delta}$, $\beta$) unless we have good reasons to believe that unobserved skills are independent of the choice of pay method. But then, we would not even need to use first-differences to get consistent estimates of the parameters: ordinary least-squares would be appropriate. However, it is clearly not appropriate in this case if self-selection into pay methods according to comparative advantage is relevant.

Now if the unobserved productivity component is unobserved to both the econometrician and all labor market participants, then equation (2) can be rewritten as

$$lnw_{it} = \delta^s + X_{it}\beta + P_{it}\overline{\delta} + m_{it-1}[P_{it} + \psi(1 - P_{it})] + \varepsilon_{it} \quad (3)$$

Note that I am implicitly assuming that the worker's pay in the salary job is based on all the information available at the end of period t-1, $m_{it-1}$. It is immediately apparent from equation (3) that even in the absence of comparative advantage (i.e. $\psi = 1$), estimating with standard first-differences will fail to eliminate the unobserved component. To see this, with $\psi = 1$, the first-differenced wage equation is

$$\Delta lnw_{it} = \Delta X_{it}\beta + \Delta m_{it-1} + \Delta P_{it}\overline{\delta} + \varepsilon_{it} \tag{4}$$

As it is, the innovation term $v_{it-1} = m_{it-1} - m_{it-2}$ is correlated with the change in pay method through the same process that led unobserved ability to be correlated with the original choice of pay method. Consequently, the pay method change dummy is an endogenous variable and has to be instrumented. Suitable instruments can be found by exploiting the panel dimension of the data set. More on that below.[8]

Note that I am sidestepping the issue of the differential information content of an output signal in a piece rate job compared to a salary job. In fact, I am implicitly assuming that it is the same. This is simply for expositional purposes. In fact, the timing of wage payments to the workers is very different in piece rate jobs than in salaried jobs. While it is sensible to assume that salaries at time t are set prior to observing output at time t (hence salaries depend on $m_{it-1}$, not $m_{it}$), in piece rate jobs payment is made *after* output is realized and information has been revealed. The basic mechanism I have in mind is that there is a pooling of skills in salaried jobs, which results in pay not being quite as sensitive to skill differentials, while piece rates entail no such pooling, in which case pay varies directly with skills.

Now if we incorporate comparative advantage in the model and we solve equation (2) for $m_{it-1}$ we get:

$$m_{it-1} = \frac{[lnw_{it} - (\delta^s + P_{it}\overline{\delta} + X_{it}\beta)] + \varepsilon_{it-1}]}{[P_{it} + \psi(1 - P_{it})]} \tag{5}$$

Taking the first lag of this expression and substituting it for $m_{it-2}$ in the law

[8]Another reason for instrumenting the pay method dummy is measurement error. If pay methods are misclassified then the observed piece rate indicator will be correlated with the error term, irrespective of whether there is learning or not. The resulting attenuation bias can be substantial with panel data models (Card (1996)).

of motion $m_{it-1} = m_{it-2} + v_{it-1}$, we can rewrite equation (2) as:

$$lnw_{it} = \delta^s + P_{it}\overline{\delta} + X_{it}\beta$$

$$+\frac{[P_{it} + \psi(1 - P_{it})]}{[P_{it-1} + \psi(1 - P_{it-1})]}(lnw_{it-1} - \delta^s - P_{it-1}\overline{\delta} - X_{it-1}\beta) + \omega_{it}$$

where

$$\omega_{it} = \frac{[P_{it} + \psi(1 - P_{it})]}{[P_{it-1} + \psi(1 - P_{it-1})]}\varepsilon_{it-1} + v_{it-1}[P_{it} + \psi(1 - P_{it})] + \varepsilon_{it} \qquad (6)$$

The resulting equation can be estimated by non linear two-stage least squares, provided we can find suitable instruments for $ln\ w_{it-1}$. A non linear regression will produce inconsistent estimates because $ln\ w_{it-1}$ is an endogenous variable due to its correlation with $\varepsilon_{it-1}$. Also the pay method dummy needs to be instrumented because of its correlation with $v_{it-1}$ through the learning process.

## 4.2 Instruments

Given that I have at least three years of data in both panels, I can use $ln\ w_{it-2}$ as an instrument for $ln\ w_{it-1}$. In addition, the lagged pay method dummy $P_{it-1}$ can be used to instrument $P_{it}$ along with a full set of interactions between $P_{it-1}$ and the other explanatory variables (the $X's$). Similarly, $P_{it-2}$ can be used as an instrument as well as its interaction with the lagged $X's$ and with $P_{it-1}$.

I will also exploit another source of arguably exogenous variation in pay methods. Prior work by e.g. Brown (1990) and MacLeod and Parent (1999) has shown that job characteristics are also an important determinant of the use of incentive pay. For example, jobs in which workers are expected to do a variety of tasks have been found to be strongly negatively correlated with the use of piece rates. More generally, piece rate or commission jobs tend to be fairly "simple" jobs for which a good objective measure of performance exists. Conversely, more complex jobs in which multitasking is the norm avoid paying workers based on explicit incentives. One reason is the absence of a mutually agreed upon simple measure of performance. Consequently, I have merged data on job characteristics from the 1991 Dictionnary of Occupational Titles (DOT) to the NLSY and PSID data sets. More precisely, I convert (and aggregate) the DOT job characteristics data to their corresponding Census 3-digit occupation averages and merge the resulting aggregated DOT characteristics to the PSID and the NLSY by 3-digit occupations. The job characteristics measures I use are the degree of complexity with which workers in a given occupation cell have to face along the following dimensions: dealing with either data, people or things; mathematical, language, numeric, verbal, spatial skills; the required level of vocational training, and how much learning is needed to perform the tasks. Of course, it can be argued that workers form an occupation match based in part on their tastes for either one of these job characteristics, which calls into question whether they can really be used as instruments if, for instance, workers are willing to trade wages for more or less of the job attributes. It turns out that relying only on the lagged values of the regressors as instruments produces very similar results, except for the precision of the estimates.

Letting Z be the matrix of valid instruments, it is then straightforward to exploit the standard orthogonality condition between the variables in Z and the residual term $\omega_{it}$ in equation (6) to obtain consistent Generalized Method of Moments estimates by minimizing the following quadratic form

$$S(\Theta, W) = \frac{1}{N}(\omega' Z) W (Z' \omega) \tag{7}$$

where $W$ is a positive definite weighting matrix and $\Theta = (\beta,\ \psi, \overline{\delta})$.[9] Note

[9]The optimal choice of the weighting matrix is $W = Z'\Omega Z$ where $\Omega$is the covariance matrix of the error term $\omega$. Letting $\Omega = I$ would also provide consistent albeit inefficient estimates. I computed an estimate of $\Omega$ by first obtaining consistent estimates of the

that, following Hansen (1982), the value of N times the objective function provides a overidentification $\chi^2$ test statistic with degrees of freedom given by the number of overidentification restrictions.

## 4.3 Unobserved Job-Match Skills

The treatment of unobserved job-match productivity is straightforward provided that the return to unobserved job-specific skills within a job is the same as the returns to unobserved worker skills.[10] Let the expected log wage of individual i in job j at time t be given by:

$$lnw_{ijt} = \delta^s + X_{ijt}\beta + P_{ijt}\overline{\delta} + m_{ijt-1}[P_{ijt} + \psi(1 - P_{ijt})] + \varepsilon_{ijt} \qquad (8)$$

where

$$m_{ijt-1} = m_{ijt-2} + v_{ijt-1}$$

and the total match productivity $m_{ijt-1}$ can be decomposed as

$$m_{ijt-1} = \alpha_{it-1} + \mu_{ijt-1}$$

i.e. the worker's total productivity in a given match depends on both a part that is transferable across employers ($\alpha_{it-1}$) and a part that is match-specific ($\mu_{ijt-1}$). Exploiting the within-job variation in the data results in not being able to separately identify each component.

parameters using the identity matrix. Then I calculated the empirical covariance matrix of the resulting residuals and re-estimated the models using $\widehat{\Omega}$. For this application, the point estimates were not very sensitive to the choice of the weighting matrix.

[10] This, of course, need not be the case. By definition, job-specific skills have no market value and hence the firm need not give those rents to the worker.

## 4.4 Decomposing the Wage Gap.

Given that explicit incentive contracts are potentially more sensitive to skills we would expect that the average wage difference between performance pay and other workers would reflect both the difference in the average skill level in the two types of jobs as well as the different returns *given* a certain skill level, in addition to the "true" affect ($\overline{\delta}$).[11] More precisely, let the average wage in each pay method be given by

$$\overline{lnw^p} = \delta^p + \overline{X_p}\beta + \overline{\alpha_p}$$

$$\overline{lnw^s} = \delta^s + \overline{X_s}\beta + \psi\overline{\alpha_s}$$

then the average wage gap is given by

$$\overline{lnw^p} - \overline{lnw^s} = \overline{\delta} + (\overline{X}_p - \overline{X}_s)\beta + \overline{\alpha_p} - \psi\overline{\alpha_s}$$

Rearranging this equation, we can express it in terms of the differences in the returns and the differences in skills:

$$\overline{lnw^p} - \overline{lnw^s} = [\overline{\delta} + (1-\psi)\overline{\alpha_p}] + [(\overline{X_p} - \overline{X_s})\beta + \psi(\overline{\alpha_p} - \overline{\alpha_s})] \qquad (9)$$

The first term on the right-hand side of equation (9) represents the effect of performance pay contracts on performance pay workers ("the effect of the treatment on the treated") while the second term indicates the magnitude of the selection bias. $\overline{\delta}$ represents the average effect of performance pay contracts on any given worker drawn from the whole population of workers. Depending on the difference in skills between performance pay workers and

[11] What follows is, of course, not quite consistent with our hypothesized model in that it assumes that the unobserved productivity component is time-invariant. However, this decomposition helps in highlighting the sources of any discrepancy between the average pay of incentive workers and that of salaried/hourly rated workers.

other workers, the effect of incentive contracts on performance pay workers may be larger or smaller. More particularly, if negative selection dominates in the sense that $\overline{\alpha_p} < \overline{\alpha_s}$, then it is possible that the incentive effect $\overline{\delta}$ is positive while the average (raw) wage gap is negative.

# 5 The Data

## 5.1 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. For instance, we know if the respondents were supervising other employees and whether they had received a promotion since the last interview. Unfortunately, we do not know the precise dollar amounts of incentive pay received by workers nor do we know the proportion of their earnings which is due to pay-for-performance.

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."

I restrict the sample to males who were in the labor market on a full-time basis and who are members of the original representative cross-section. The people who were considered as meeting that criterion were (i) those whose primary activity was either working full-time, on a temporary lay-off or looking actively for a job, (ii) those who had worked at least half the year since the last interview and who were working at least 20 hours per week. Individuals excluded from the sample are those who have been in the military at any time, the self-employed and all public sector employees. Also, I exclude observations for which real ($79) hourly earnings are less than $1.00 or greater than $100.00. In addition, I will only be using workers who are observed in all three years. The reason is that I need to account for the endogeneity of pay method change by using variables lagged twice as instruments for the current period's pay method. These restrictions leave me with a sample of 1,220 workers. Some summary statistics are reported in Panel A of Table 1.

## 5.2 Panel Study of Income Dynamics (1981-1992)

The sample consists of 8,407 male heads of households aged 18 to 64 with positive earnings for the period spanning the years 1981-1992.[12] Individuals in the public sector and who worked less than 500 hours are excluded from the analysis. We know whether each worker is paid a piece rate, a commission, an hourly rate or a salary. As we can see, the fraction of workers reporting being paid a piece rate is substantially lower in the PSID. This is true even if we restrict the PSID sample to people of roughly the same age as those in the NLSY. One likely reason for that is the way the questions on pay methods are formulated in the PSID. Workers are first asked whether they are paid an

[12]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 I actually use data covering interview years 1981-1993. Note that I use members of the original cross-section (the full PSID includes a Poverty Subsample).

hourly rate, a salary, or in some other form. Then those that report not being paid either a salary or an hourly rate specify their pay method (piece rate or piece rate + hourly/salary, commissions or commissions + salary, etc.) Thus, contrary to the NLSY where respondents are not "forced" to choose among mutually exclusive categories, the PSID likely makes people report the pay method from which they earn most of their labor income.

## 5.3 Evidence on Selection Effects

If non random selection into pay methods is of any importance, we should at least be able to find some descriptive evidence of it in the differential patterns of earnings changes across methods of pay. The basic Lazear model would suggest that workers who move out of explicit incentive contracts and self-select into salaried jobs should suffer a wage loss because employers expect this self-selected group of workers to be less productive than the overall work force. Conversely, only the more productive ones should move into a piece rate job or a commission job.

Overall, the evidence presented in Table 3 provides some support to the notion that movement in and out of explicit contracts follows the predicted patterns. For example, if we look at job changers (Panel B), moving from a salary/hourly rate to a piece rate or commission contract is associated with a 12.1% wage gain on average, while the reverse move involves large losses (-25.7%). Also, if we look at the average earnings change for workers who move from a piece rate/commission job to a salary/hourly rate job with the same employer, we can see that workers suffer a average loss of about 13%. Interestingly, job stayers who move from a salary/hourly rate to a piece rate suffer a wage decline of 6.3%, which, on the surface, is not quite consistent with the basic model in which only the more productive workers select themselves into incentive contracts.[13]

[13]It is consistent though with the notion that these workers might be able to pool with more productive workers early on in the employment relationship. As they accumulate

The next set of figures show some descriptive evidence on patterns of changes from one pay method to another as well as kernel density estimates of the distribution of log hourly earnings by pay method, net of the effect of all observables.[14]As can be seen from Figure 3, the typical pattern of pay method change is one in which at most two such changes occurs. This is particularly true for job changers. Still, there are workers who seem to go back and forth between pay methods, although one has to be worried about possible misclassification of the pay methods.

The distribution of log hourly earnings by pay methods reveals that not all piece rate workers come from the upper part of the earnings distribution. In fact, if I use only the subset of workers who are observed in only one pay method or the other in all years and estimate the worker-specific intercepts in a log-wage equation, the resulting plots shown in Figure 5A suggests that the selection of workers into piece rate jobs does not appear to be trivial in the sense of being one-sided. Although it should be stressed that this is only indicative, it seems that the ability to adjust the pace of work upward or downward attracts workers of all "ability". This is perhaps clearer when one does the same exercise using only the subsample of workers who are observed at least twice in each pay method (Figure 5B) and for whom I can compute a pair of individual intercepts: one for piece work and the other for salary work. This "separation of types" allowed by piece rates appears to be more consistent with Brown (1990)'s reformulation of the basic Lazear model, at least with the PSID data.[15]

Of course, some important considerations are left out in the foregoing analysis. One is task assignment. Workers may be reassigned by their employer to other jobs which may carry different pay. Also, reassigning workers

tenure and learning about worker productivity takes place, this should become more difficult.

[14]The list of observables includes a quadratic in age, region, year, industry, occupation, and union coverage dummies.

[15]I did not use the NLSY to do the same exercise because of sample size problems.

to jobs in different industry and/or occupation cells may put those workers in an environment in which it may be suitable to use, say, piece rates when in the previous job it was not. In other words, job characteristics, and not only worker characteristics, are also an important determinant of contract form, as is suggested by e.g. Brown (1990) or Garen (1998).

### 5.4 Other Data Related Problems

One important difficulty I face is the fact that I only have "low-frequency" wage observations i.e. I basically have to rely on annual observations to identify the parameters of the model, particularly the return to unobserved skills parameter. It is conceivable that it may be difficult to find support for the model simply because workers are not observed on a more frequent basis as would be the case if I had monthly or even weekly data. Simply put, it seems reasonable to suppose that wages do reflect skills eventually, no matter how people are paid. Thus, having to rely on yearly observations may fail to reveal the greater *short-term* sensitivity of wages to skills in piece rate jobs or, at least, may underestimate it.

In addition, misclassification of pay methods is likely to play a role. More particularly, one would expect reporting errors to attenuate the measured wage effect toward zero, especially when using first-differences.

## 6 Results

If we look at first at the results for the NLSY presented in Table 3, we can see that simply ignoring selection issues by using OLS would lead us to believe that explicit incentive contracts are associated with higher average hourly earnings. This holds true even with first-differences, although the estimates are not very precise. Given that misclassification of pay methods would tend to impart an attenuation bias, the higher coefficient estimated with first differences reflects the fact that the selection process leading workers to work in piece rate jobs may not be such that only higher than average ability individuals choose to work in those jobs.

However, using first-differences along with instrumental variables to allow for learning effects reduces the estimate substantially. Turning to the GMM estimates in column 4 of Table 3, we can see that comparative advantage does appear to be an important determinant of the choice of pay methods. The return to skills is significantly different from one in salaried jobs and the wage effect is essentially reduced to zero. Note, however, that the overidentification test statistic suggests that the overidentification restrictions may not be valid.

Much the same conclusion concerning the effect of comparative advantage emerges when we look at the results obtained by exploiting repeated observations in a given job-match. However, the results also strongly suggest that classification errors within jobs may be a much more severe problem. Simply using instrumental variables estimation along with first-differences results in a wage impact more than twice as large as the one measured with standard differencing.

Turning now the PSID sample, I am able to exploit the larger sample (and longer time dimension) to estimate the models for the subsample of job changers only. Focusing first on the within-worker estimation results, one can see that selection according to comparative advantage clearly seems not to be as important. With the full sample, the return to skills is not significantly different across pay methods. In addition, it appears that attenuation bias plays a major role in relation to learning effects as the results using instrumental variables point toward a much larger wage effect. The same overall conclusions hold when we look at job stayers only. Indeed, perhaps not surprisingly, the attenuation bias is more severe than for the full sample. In addition, the GMM estimate of the incentive effect is virtually the same as when I ignore comparative advantage. This is also true for the overall sample but it appears that the distinction between job stayers and job changers is useful: when one uses only job changers, I find strong evidence that comparative advantage matters and that ignoring it leads to a substantial upward bias in the estimated wage effect.

For comparability purposes, I also estimated the same models using the subsample of workers of the same age as those in the NLSY. To reconcile the differences in the age distribution between the two samples for the workers aged between 23 and 32, I first computed the sample frequencies of each age cells with the NLSY and then I use those frequencies to weight the observations in the PSID. As can be seen from the last columns in panels A and B, the results are remarkably similar to those obtained with the NLSY: comparative advantage really seem to matter early in one's career or in one's job. In both panels, the return to skills is below one and is precisely estimated.

Again, a troublesome feature of the results is the magnitude of the overidentification test statistics. Results obtained when I exclude the DOT job characteristics from the list of instruments were very similar, albeit somewhat less precise. The $\chi^2$ statistics were also closer to the acceptance region, especially in the case of the NLSY.

## 6.1 Summary of Results

Overall, the results suggest that:

- Comparative advantage matters mainly for younger workers or workers who are observed for the first time in a given employment relationship (the job changers). When I turn to exploiting the within-job variation in the data, I find weaker evidence that the return to skills varies across pay methods, although it still holds for younger workers. In other words, there is strong evidence that some partial pooling of workers with different skills occurs early on as the market is in the process of assessing worker productivity. Eventually, though, pay reflects individual skills.

- Estimates using simple fixed-effect models appear to suffer from substantial biases due to the endogeneity of switching pay methods. This endogeneity bias may be caused by either learning effects or measurement error in the pay method dummy. This latter effect seems to cause a strong attenuation bias in models that exploit the within-job variation in the data.

- Assuming that the return to skills is the same across pay methods for workers repeatedly observed in the same job, one can get consistent estimates of the "true" incentive effect by using within-job first-difference models combined with instrumental variable estimation.

- Based on the results using the full PSID, the incentive effect of explicit pay-for-performance contracts is in the neighborhood of 11%.

- As for the results using the within-job variation in the NLSY, it should be pointed out that while the estimated coefficient obtained by GMM is actually negative (column 3 of Panel B), the standard error is quite large. The test of a zero wage impact thus has rather low power.

# 7 Conclusion

This paper has investigated the role played by comparative advantage in directing workers' choice of a pay method. I have found relatively strong support for the notion that pay is more sensitive to skills in jobs where pay is "output-based" in the case of younger workers or workers who are new to their job.

My overall assessment of the results would be that as long as one has a sample of workers who are fairly representative of the overall workforce, as is more likely to be the case with the PSID, then first-difference estimates obtained by exploiting the within-job variation in the data are probably close to the "true" wage impact, provided one controls for the presence of measurement error through the use of instrumental variables. This latter aspect may not be as much of a concern when one has access to an administrative data set, such as the one used by Lazear (1996). This may explain why the estimates presented here are fairly close to those obtained by Lazear. However, it really seems that the combination of learning and self-selection according to comparative advantage that appears to be very important early in one's career should make us a bit more cautious about results obtained with data on younger workers. It may very well be that there is a true incentive effect for those younger individuals, but it is harder to detect.

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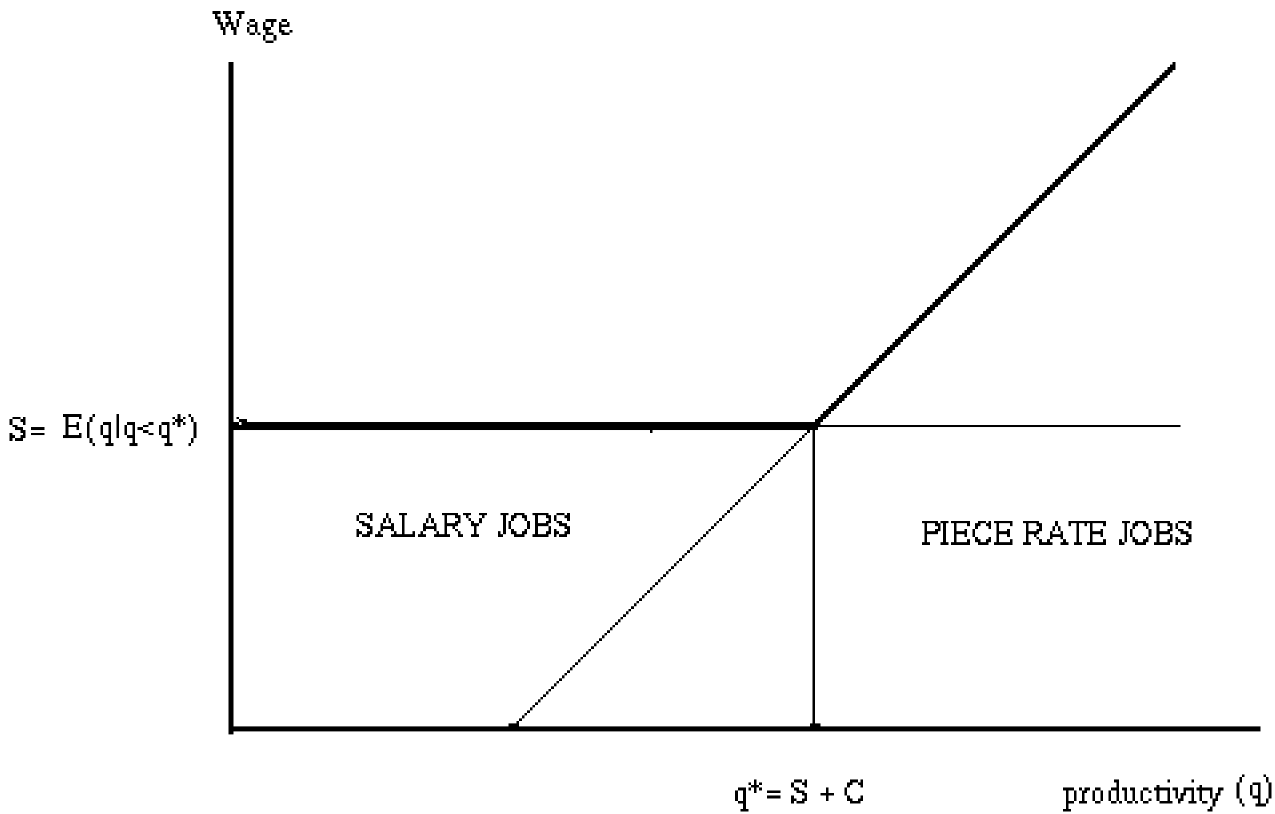


*Figure 1. Optimal Sorting of Workers*

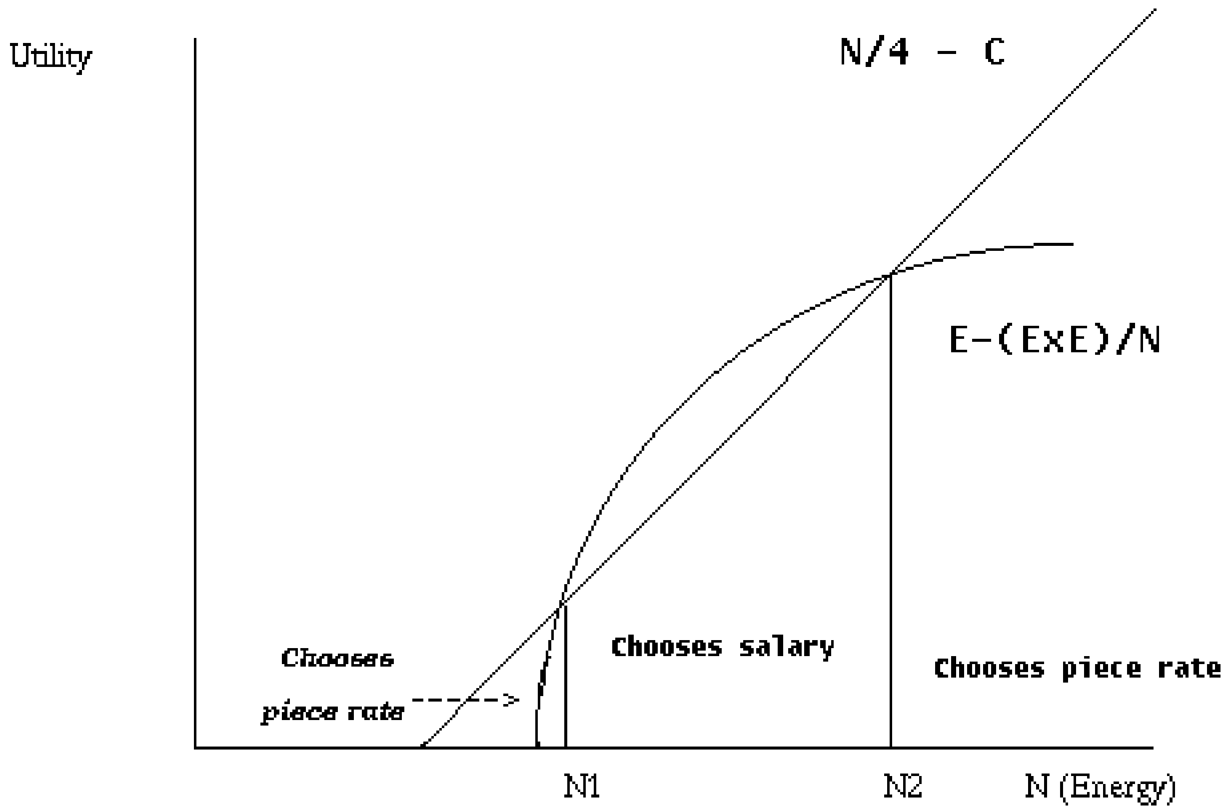


*Figure 2. Cost of Effort and Contract Choice*

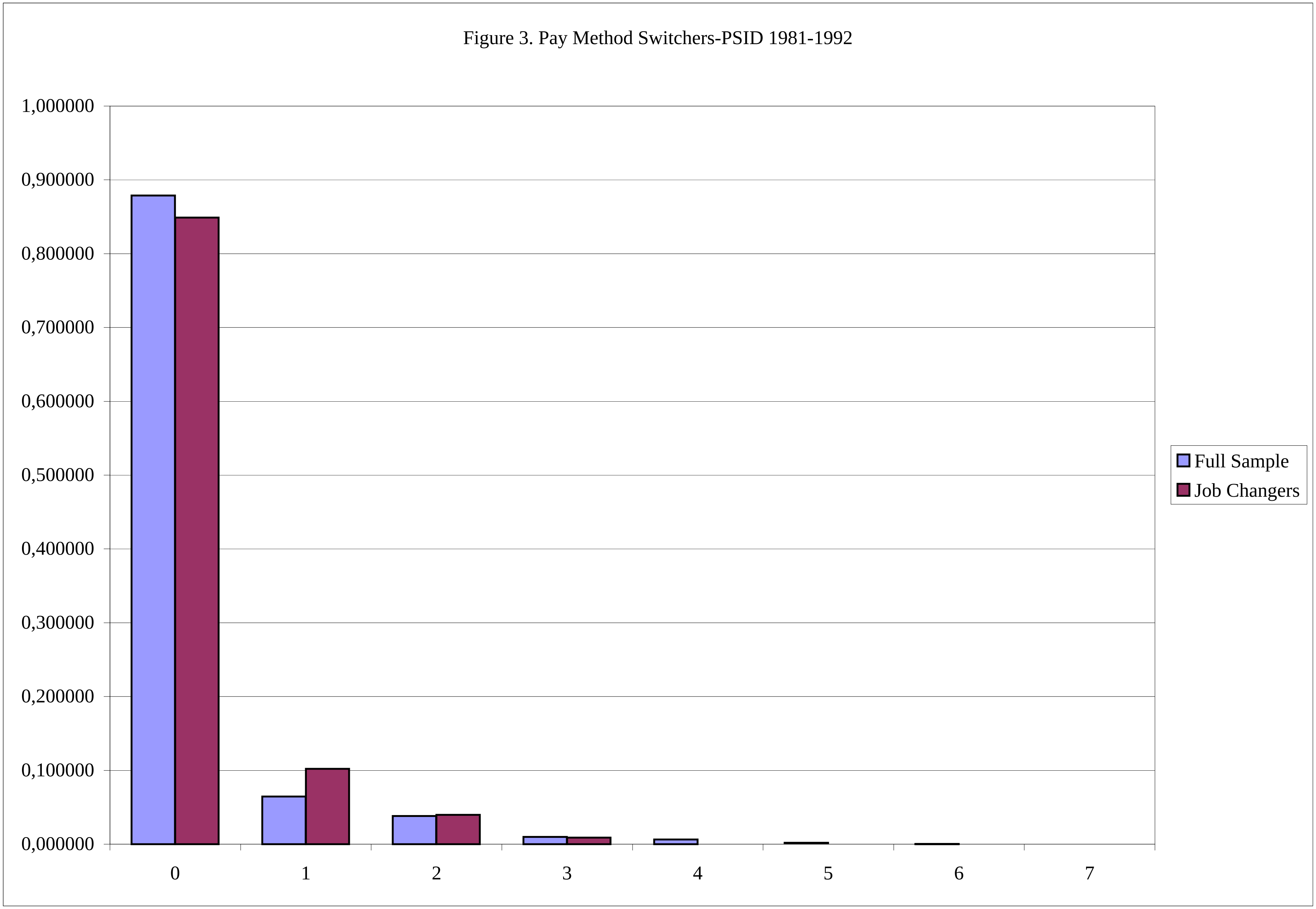
Figure 3. Pay Method Switchers-PSID 1981-1992
1,000000
0,900000
0,800000
0,700000
0,600000
0,500000
0,400000
0,300000
0,200000
0,100000
0,000000
0
1
2
3
4
5
6
7
Full Sample
Job Changers

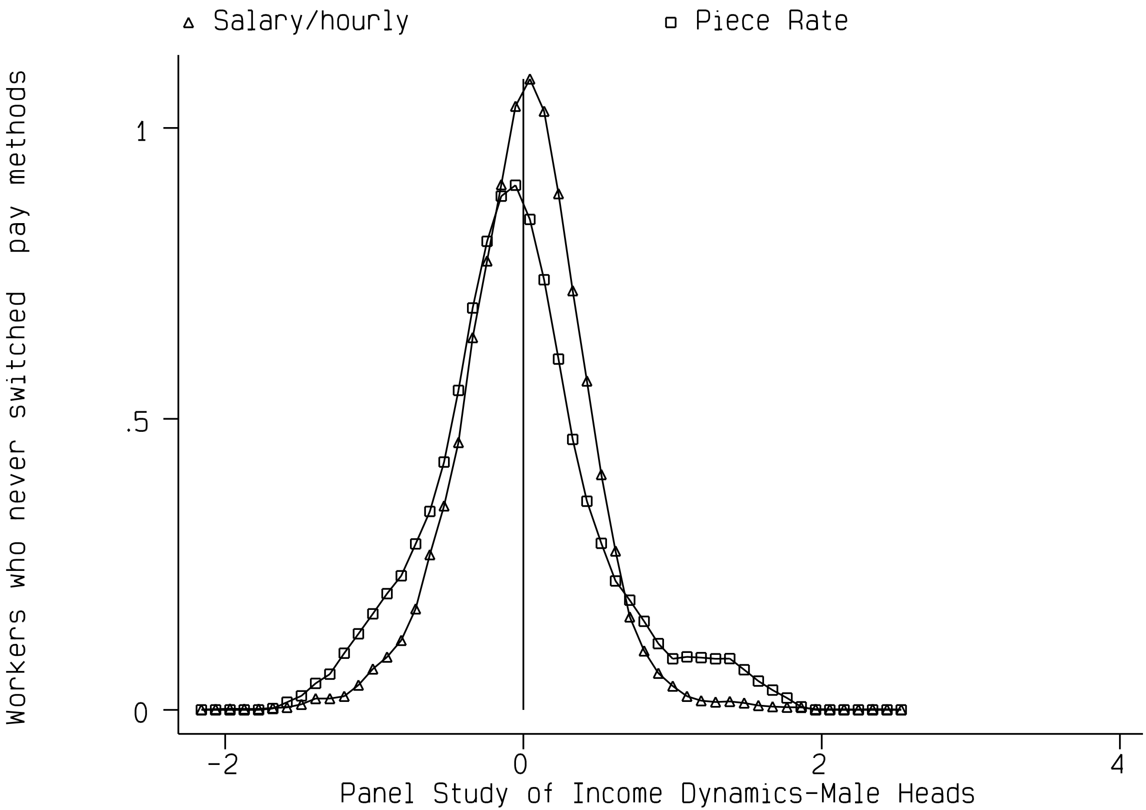


Fig 4a. Distribution of Log Average Hourly Wages

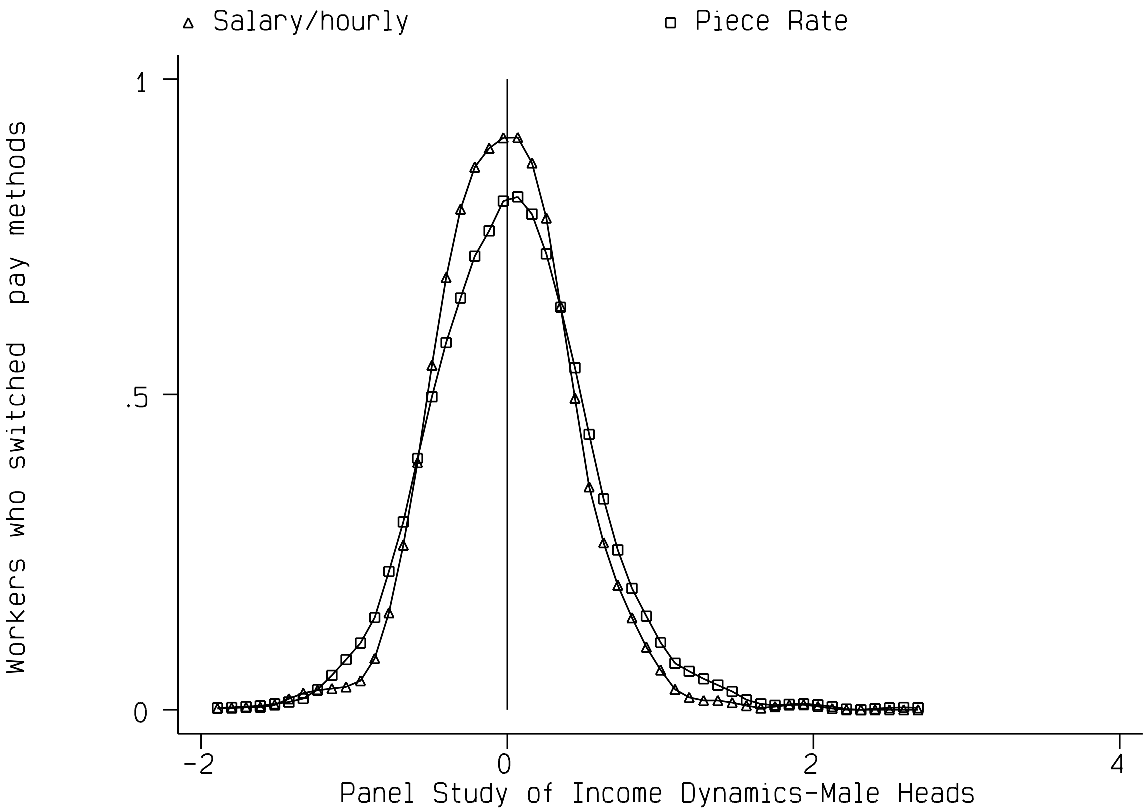


Fig 4b. Distribution of Log Average Hourly Wages


Fig 5a. Distribution of Individual-Specific Effects

Fig 5b. Distribution of Individual-Specific Effects

**Table 1. Sample Means**

**A. National Longitudinal Survey of Youth (1988-1990)**

| | Pay Method | |
|---|---|---|
| **Variable** | **Piece Rate** | **Salary / Hourly** |
| Log Average Hourly Earnings ($79) | 1.87 | 1.75 |
| Age | 28.71 | 28.53 |
| Schooling in Years | 13.41 | 12.93 |
| % White | 0.93 | 0.81 |
| % Unionized | 0.11 | 0.23 |
| N | 128 | 1092 |

**B. Panel Study of Income Dynamics (1981-1992)**

| | Pay Method | |
|---|---|---|
| **Variable** | **Piece Rate** | **Salary / Hourly** |
| Log Average Hourly Earnings ($79) | 2.079 | 2.08 |
| Age | 39.51 | 39.63 |
| Schooling in Years | 11.72 | 11.17 |
| % White | 95.26 | 92.19 |
| % Unionized | 11.62 | 25.58 |
| N | 654 | 7753 |

See text for a discussion on how pay wethods are coded in these two data sets.

**Table 2. Average Change in Log-Earnings by Type of Transition (NLSY)**

**Panel A: Job Stayers Only**

| | | Pay Method in Year T + 1 | |
|---|---|---|---|
| | | Piece Rate or Commission | Salary or Hourly Only |
| **Pay Method in Year T** | P.R. / Commission (# Obs = 177) | 0.061 [0.571] | -0.129 [0.429] |
| | Salary or Hourly Only (# Obs = 1396) | -0.063 [0.031] | 0.020 [0.969] |

**Panel B: Job Changers Only**

| | | Pay Method in Year T + 1 | |
|---|---|---|---|
| | | Piece Rate or Commission | Salary or Hourly Only |
| **Pay Method in Year T** | P.R. / Commission (# Obs = 79) | 0.227 [0.383] | -0.257 [0.617] |
| | Salary or Hourly Only (# Obs = 693) | 0.121 [0.102] | 0.002 [0.898] |

Notes. Each cell entry represents the weighted average change in earnings for workers in Year T+1 (T=1988, 1989) who were paid either one of the pay methods in year T. The number of observations refers to Year T. The numbers in brackets represent the transition rates between pay methods from Year T to Year T+1. I used all available observations that met the sample selection criteria, except for the one requiring that workers be observed in all three years.

**Table 3. Estimates of the Wage Effect of Pay-per-Performance Contracts-NLSY**

**Panel A: Within-Worker Estimation**

| **Variable** | **1. OLS** | **2. First Differences** | **3. First Diffs. + IV** | **4. GMM** |
|---|---|---|---|---|
| Performance Pay Dummy (δ) | 0.0416 (0.0353) | 0.0712 (0.0414) | 0.0377 (0.0385) | 0.0033 (0.2763) |
| Returns to Unobservable Skills (ψ) | – | – | – | 0.7140* (0.0788) |
| Overidentification Test Statistic [Degrees of Freedom] | – | – | 104.51 [73] | 97.76 [73] |
| N | 1220 | 1220 | 1220 | 1220 |

**Panel B: Within-Job Estimation**

| **Variable** | **1. First Differences** | **2. First Diffs. + IV** | **3. GMM** |
|---|---|---|---|
| Performance Pay Dummy (δ) | 0.0461 (0.0394) | 0.0947 (0.0583) | -0.0386 (0.1017) |
| Returns to Unobservable Skills (ψ) | – | – | 0.7852* (0.0421) |
| Overidentification Test Statistic [Degrees of Freedom] | – | 78.37 [73] | 88.56 [73] |
| N | 818 | 818 | 818 |

* Significantly different from 1.
Note. Other covariates include age, age squared, education, region of current residence, year, occupation, industry, union coverage, the local unemployment rate, marital status.

## Table 4. Estimates of the Wage Effect of Pay-per-Performance Contracts-PSID

### Panel A: Within-Worker Estimation

| Variable | 1. OLS | 2. First Differences | 3. First Diffs. + IV | 4. GMM | 5. GMM: 23-32 yr-olds |
|---|---|---|---|---|---|
| Performance Pay Dummy (δ) | 0.0469<br>(0.0202) | 0.0443<br>(0.0172) | 0.0913<br>(0.0362) | 0.0977<br>(0.1161) | -0.0465<br>(0.1603) |
| Returns to Unobservable Skills (ψ) | – | – | – | 1.1785<br>(0.1225) | 0.8386*<br>(0.0756) |
| Overidentification Test Statistic [Degrees of Freedom] | – | – | 146.98<br>[73] | 150.16<br>[73] | 102.63<br>[73] |
| N | 8407 | 8407 | 8407 | 8407 | 1823 |

### Panel B: Within-Job Estimation

| Variable | 1. First Differences | 2. First Diffs. + IV | 3. GMM | 4. GMM: 23-32 yr-olds |
|---|---|---|---|---|
| Performance Pay Dummy (δ) | 0.0279<br>(0.0194) | 0.1094<br>(0.0408) | 0.1107<br>(0.0413) | 0.0115<br>(0.1524) |
| Returns to Unobservable Skills (ψ) | – | – | 0.9556<br>(0.1395) | 0.8433*<br>(0.0632) |
| Overidentification Test Statistic [Degrees of Freedom] | – | 100.68<br>[73] | 100.37<br>[73] | 86.81<br>[73] |
| N | 6444 | 6444 | 6444 | 1259 |

### Panel C: Job Changers Only

| Variable | 1. First Differences | 2. First Diffs. + IV | 3. GMM |
|---|---|---|---|
| Performance Pay Dummy (δ) | 0.1321<br>(0.0570) | 0.1276<br>(0.0535) | 0.0643<br>(0.1554) |
| Returns to Unobservable Skills (ψ) | – | – | 0.6120*<br>(0.1582) |
| Overidentification Test Statistic [Degrees of Freedom] | – | 102.31<br>[73] | 99.59<br>[73] |
| N | 852 | 852 | 852 |

* Significantly different from 1.

Note. Other covariates include age, age squared, education, region of current residence, year, occupation, industry, union coverage, the local unemployment rate, marital status.

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