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Tuomas Pekkarinen
Åbo Akademi University and Helsinki School of Economics

Chris Riddell
Queen's University

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Abstract
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Keywords
Performance Pay

PERFORMANCE PAY AND EARNINGS: EVIDENCE FROM PERSONNEL RECORDS

TUOMAS PEKKARINEN and CHRIS RIDDELL*

This paper examines the earnings effects of performance pay using linked employee-employer panel data from Finland's metal industry for 1990–2000. The authors estimate the effects of performance pay contracts in the presence of individual and firm unobserved heterogeneity as well as in tasks of different complexity. Unobservable firm characteristics explain about 40% of the variance in the use of performance pay. Performance pay workers earned substantially more than fixed rate workers, a finding that persists even in analyses that use for identification only those workers who changed firms (and contracts) due to an establishment closure. There is also evidence of a strong, negative relationship between job complexity and the incentive effects of performance pay. Finally, several "quasi-experiments" show that when one plant underwent a compensation regime change but other highly similar plants in the same firm did not, workers in the "treatment" plant gained substantial earnings premiums.

There is considerable interest in the link between performance pay and productivity. Prior to the 1990s, the empirical literature on this issue was quite weak: some studies were based on cross-sectional evidence, others were merely anecdotal. In recent years, several studies from the economics literature, as well as a lesser number from the management literature, have used longitudinal data from a single firm that changed its compensation policy to examine the effect of performance pay controlling for unobserved worker heterogeneity (for example, Lazear 2000). A small group of other studies have performed similar analyses using large, household-based longitudinal surveys (for example, Parent 1999). Another small group of papers have approached the issue by combining structural models with detailed longitudinal data from a single firm (for example, Shearer 2004).

*Tuomas Pekkarinen is Acting Professor, Department of Economics and Statistics, Åbo Akademi University, and Research Fellow, Department if Economics, Helsinki School of Economics, and Chris Riddell (the contact author) is Assistant Professor, School of Policy Studies, Queen's University. The authors thank seminar participants at the LSE, Oxford, MIT (Sloan), the University of Minnesota (Carlson), the University of Toronto, the University of British Columbia, the 2004 EALE meetings (Lisbon), the 2006 SOLE meetings (Boston), and in particular Joshua Angrist, Pablo Casas, Francis Kramarz, Steven Lehrer, Thomas Lemieux, and Paul Oyer for their helpful comments. They also thank the Confederation of Finnish Industry and Employers (Teollisuus ja työnantajat) for permission to use their wage records. Pekkarinen acknowledges the financial support of the Academy of Finland, the Finnish Work Environment Fund, and Yrjö Jahnsson Foundation, and Riddell acknowledges the financial support of the Social Sciences and Humanities Research Council of Canada.

The data are archived at the Labour Institute for Economic Research, Helsinki, and permission for their use is controlled by the Confederation of Finnish Industry and Employers. Researchers interested in these data can find contact information at http://www.labour.fi/english/about/ptabout.htm. The programs used to conduct this analysis are available upon request. Please direct requests to the corresponding author, Chris Riddell, at chris.riddell@queensu.ca. Stata 9 was the statistical package used.

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While this small but growing body of evidence has improved our understanding of the effects of performance contracts, the studies to date have either focused on a very narrow set of individual firms and tasks or ignored establishment-level factors altogether. While there is clearly reason to be concerned about individual-specific unobserved heterogeneity—workers observed to be on performance pay contracts may be more able or more motivated than other workers, on average—there is also reason to be concerned about the endogeneity of compensation policy—firms probably do not randomly decide to implement performance pay. If the firms changed compensation policies as a result of some unobserved changes that took place at the firm level, then previous studies may have failed to identify the incentive effects of performance-based compensation policies.

In this paper, we use unique linked employer-employee panel data from Finland for 1990–2000 to estimate the earnings effects of pay-for-performance contracts. These data contain yearly information on the exact share of earnings from a performance contract and cover the entire population of blue-collar workers in the metal manufacturing sector.

What distinguishes these data from those used in prior studies is that we have variation in payment schemes across both individuals and firms. We are thus able to estimate the effect of performance contracts in the presence of both individual-level and firm-level unobserved heterogeneity. Furthermore, we can identify those workers who were forced to switch firms as a result of an establishment closure. We argue that an analysis comparing workers who changed contracts following an establishment closure with those who did not focuses on a source of variation more exogenous than that examined in most previous studies. As well, we find several "quasi-experiments" in which one plant underwent a compensation regime change but other plants in the same firm did not. Finally, the institutional framework of this industry is such that we observe detailed indications of the complexity of all jobs, allowing us to estimate the effects of performance contracts across tasks of different complexity.

Background

As noted above, the impact of performance pay on individuals and establishment outcomes has received considerable attention from both the economics and management disciplines. Most of the empirical studies either look closely at a single firm, use large household or establishment surveys (usually cross-sectional), or analyze filings from publicly traded companies (typically only the Fortune 500 or a similar small subset). The economics literature has tended to focus on cases in which individual contracts are observed—mainly piece rate contracts—while the management literature has tended to focus on establishment-level compensation policies such as profit-sharing.[1] Both fields also have a large set of executive compensation studies.

The key finding of the economics literature is that contracts that make pay a function of worker output—in particular, contracts that provide for piece rates—increase productivity and earnings relative to contracts that do not make pay a function of output. The evidence of productivity gains is much less compelling in the case of the executive compensation and profit-sharing literatures. A full review of the performance pay literature is beyond the scope of this paper, and so we focus on the piece rate studies in the economics literature, which are the most comparable to our paper.[2]

A key problem with much of the performance pay literature is its operative assumption that compensation policy is exogenous. Firms that use performance pay are likely systematically different from other firms in unobserved ways that are correlated with observable firm (and worker) characteristics as well as with outcomes such as productivity and profitability. Also likely affected by endogenous selection are the characteristics of employees across the two sets of firms: more able or more motivated workers may

[1]One notable exception to this is Banker, Young, and Potter (1996). These authors studied the effects of a bonus pay system in a retail trade establishment that implemented the policy in 15 of 34 outlets.

[2]See Prendergast (1999) for an excellent review.

be more likely to accept (or be offered) a performance-related contract. A good deal of the performance pay literature ignores the selection issue, thereby assuming that firms (or individuals) not using performance pay can act as a comparison group for those using it (for example, Seiler 1984; Brown 1992).

Some studies have attempted to deal with the selection problem by collecting longitudinal data on workers from a firm or by using large longitudinal household surveys such as the NLSY, PSID, or BHPS, and then differencing out unobserved individual-specific heterogeneity. Examples of survey-based longitudinal studies include Parent (1999, 2007) and Booth and Frank (1999). Key examples of the individual firm approach are Lazear's (2000) study of Safelite Glass, Shearer's random assignment study of a British Columbia tree planting firm (2004), and Bandiera, Barankay, and Rasul's (2005) study of a U.K. fruit farm. Studies employing the individual firm approach have the distinct advantage of having information on actual worker productivity; all find very large productivity gains from piece rates relative to fixed wages, but also considerable support for selection bias. For instance, Lazear (2000) found that the switch to piece rates increased productivity by about 40%, only half of which was attributable to an incentive effect. In Shearer's experiment, productivity gains were about 20%.

While recent econometric studies of individual firms represent a major advance over the earlier performance pay literature and have provided compelling evidence of both the productivity gains from performance pay and the role of selection bias, they have some drawbacks. First, they focus on an extremely narrow set of occupations. Second, each study is based on only a single firm. As noted above, however, just as we anticipate individual unobserved heterogeneity to be a problem in an individual-level study of performance pay, compensation policy at the firm level is likely endogenous as well. As discussed in the next section, theories of how firms decide to use performance pay revolve around firm-specific factors such as monitoring costs and the composition of the work force (for instance, skill heterogeneity).[3] Abowd, Creecy, and Kramarz (2002) showed that failing to account for unobserved firm-specific factors can lead to substantial biases in the estimates on individual-level covariates. Survey-based longitudinal studies avoid the narrow occupation issue, but still ignore establishment characteristics, rarely have explicit information on contracts, and are subject to considerable measurement error.

This paper uses an empirical strategy that lies between the household survey approach and the detailed case study approach. In particular, we use unique linked employer-employee panel data to provide new evidence on the effects of performance pay on earnings in the presence of both individual-specific and firm-specific unobserved heterogeneity. These data are derived from payroll records, which, in addition to being comparatively free of measurement error, allow us to observe the exact share of earnings and hours that an employee works on a performance pay contract. There are also three different types of contracts that can be studied. The data cover an entire industry (metal manufacturing) across an entire country (Finland) over an eleven-year period. Moreover, the institutional framework for this industry/country is such that we have richer information on employee characteristics than is available in most personnel records, and very detailed and reliable information on job complexity. On the other hand, the data do not allow us to observe actual productivity, and thus we must test for productivity effects indirectly by examining earnings.

[3]There is one form of firm-specific unobserved heterogeneity that may complicate the discussion of piece rates: the financial situation of the firm. In particular, output-based pay is a relatively easy-to-adopt mechanism (especially in a manufacturing setting) that transfers risk from the firm to potentially risk-averse workers. For instance, the timing of Safelite Glass's Chapter 11 bankruptcy filing (as analyzed by Lazear 2000) suggested that the organization was in financial difficulty before the switch to piece rates. From society's standpoint, there is, therefore, a question as to the welfare costs of output-based pay when such practices may impose large costs on risk-averse workers.

Theoretical Considerations

The performance pay contracts for blue-collar workers in the Finnish metal industry are based on pure piece rates or a mixed piece rate/fixed rate, and thus we focus on theories relevant to these types of contracts. The institutional details on payment methods and wage determination in this industry are discussed in detail in the next section. Lazear's (1986) model is the benchmark for contracts that make pay a function of output, with subsequent reformulations by Brown (1992), Booth and Frank (1999), and Lazear (2000).

Lazear's two-period model is largely concerned with the sorting of workers between piece rate firms and fixed rate firms. Fixed rate workers are paid a salary, S, that is independent of productivity, while piece rate workers are paid based on output (q), but must be monitored, which incurs a cost, M, resulting in a piece rate wage of $q - M$. This is in part because the firm must know employees' output in order to pay them, but could also be partly due to the quality control concerns with piece rate contracts. Workers know their own q's and choose the payment method that yields the highest earnings. Thus, workers will choose the piece rate firm if $q - M > S$, and otherwise choose the salary firm. Firms paying salaries therefore know that their employees are, on average, less productive than employees in piece rate firms, and the salaries they pay factor in this lower expected productivity. The key testable implication is that earnings should be higher for piece rate workers than fixed rate workers.[4]

Booth and Frank extended Lazear's model to a richer case, in which (a) monitoring costs can differ across firms and (b) worker output is a function of both effort and ability, with effort being unmonitorable and ability consisting of two components, one observable (subject to a monitoring cost) and the other not. Lazear (2000) made a similar extension in the case of a single firm (and thus ignored monitoring costs). Both of these theories yielded the same conclusion that earnings for piece rate workers will be higher than those for fixed rate workers, but that—contrary to the implications of Lazear (1986)—part of this earnings effect is due to selection on ability and part is due to an incentive to work harder. An interesting corollary of these models is that effort need not be higher, on average, for piece rate workers than for fixed rate workers, since their higher ability means they do not have to exert as much effort for the same level of output. Effort should increase, however, if a given worker moves from a fixed rate to a piece rate. A further implication of Booth and Frank's model is that monitoring costs affect the firms' decision to use performance-based pay. In general, piece rate jobs will be held by high-ability workers in low-monitoring-cost firms.

Institutional Details

With a current population of around 5.2 million, Finland is similar in size to two other Nordic countries, Norway and Denmark. It has been a member of the European Union since that organization's inception in 1995, and it adopted the common currency in 1999. Like the labor markets of other Nordic countries, the Finnish labor market differs from that of the United States along some fundamental institutional dimensions. In particular, Finland has a unionization rate of around 70%, and about 90% of its labor force is covered by collective agreements. Further, collective bargaining is more centralized in Finland than in the United States, with high-level employer confederations and central union organizations negotiating country-wide income policy agreements, and sectoral employer organizations and sectoral trade unions then negotiating agreements at the sector level. In some cases, the sectoral agreement may allow some conditions of employment to be negotiated at the local (that is, workplace) level. Generally, the higher-level agreements apply only a very general framework to collective bargaining at the sectoral (or local) level. The wage

[4]Lazear's theory is based on a zero-profit condition, and so salary firms have less productive workers, but this is exactly offset by savings on monitoring costs and a lower wage bill.

determination process in metals—the industry examined in this paper—is discussed further below.[5]

Conciliation is mandatory and collective agreements tend to be multiple years in length, making strikes and lockouts less common in recent years than in the past, but over the 1996–2005 period Finland still ranked in the top third of the OECD in strike activity. Working hours in Finland are around the EU member average, while hourly wages for industrial workers are very high—currently (2008) about 14 Euros, the fourth-highest rate in the entire EU. In contrast, the level of wage dispersion in Finland is among the lowest in the OECD, and income taxes are both progressive and high. In general, the Finnish labor market is *far* more egalitarian than the U.S. labor market, which may have implications for how workers respond to incentive pay.

There are other important economic differences between Finland and the United States. In particular, at the end of our sample period in 2000, exports accounted for around 40% of GDP (versus around 10% in the United States), and metals accounted for over half of overall exports. Along with metals, key industries include electronics and the forestry sector.[6]

Wage Determination in the Finnish Metal Industry

The Finnish metal industry is unionized, with the general guidelines on wage determination set out in a collective agreement that is negotiated at a national level between the central employer organization and the trade union. The collective agreement indicates that wages should be determined according to the complexity of the job, and by various individual and firm-specific arrangements.

The collective agreement sets a job-specific minimum hourly wage, which is referred to as the occupation-related wage. These hourly wages are determined according to an evaluation of all jobs in the industry, which is conducted by a group of experts who assign complexity points to each job. The complexity level is based on three criteria: how long it takes to learn the job, the degree of responsibility in the job, and the job's working conditions. The more demanding the job, the more complexity points it is assigned, and the higher the occupation-related wage. There is a one-to-one correspondence between the complexity points and occupation-related wages. The occupation-related wages can therefore be interpreted as a continuous variable that measures the complexity of the tasks. For each individual, we observe the occupation-related wages, final wages, and occupational code (specifically, which of 165 categories of job assignment the job matches).

The role of the collective agreement is to set minimum standards for wage determination. A worker in this industry knows the minimum wage he or she is entitled to for the specific job. The determination of the final wage takes place at the establishment level (or possibly plant level). An individual firm is free to set wages as long as they stay above the minimum levels set by the collective agreement. Moreover, the payment method is decided by the firm.

Payment Schemes in the Finnish Metal Industry

The collective agreement allows firms to choose from three different contracts: fixed rates with a performance bonus, piece rates, and reward rates. The spirit of the collective agreement calls for the payment method to be determined by the characteristics of the tasks the worker performs.

On fixed rates, workers are paid by the hour; however, fixed rate contracts have provisions for discretionary bonuses of 2–17%

[5]For a thorough review of Finland's labor market, labor law, and industrial relations system, see Ministry of Labor (2003).

[6]Another noteworthy feature of Finland's recent economic history is the severe post-1990 recession, during which the unemployment rate increased from around 4–6% in the early 1990s to nearly 20% in 1994. Since then, the economy has improved at a remarkable rate, led largely by the electronics and telecommunications industries; in recent years the unemployment rate has been in the 7% range. As we discuss later in the paper, the results are not sensitive to the exclusion of the recession years.

of the occupation-related wage (that is, the minimum wage for a given job). The bonus is based on the supervisor's evaluation of the employee. The collective agreement indicates that employers are to use the full range of bonus amounts, and to assign these bonuses such that they are distributed symmetrically around the mean of 9.5%. We observe these bonuses in the data, and incorporate them into the earnings of fixed rate workers. For most jobs in this industry, there is considerable variation in fixed rate earnings, with many fixed-rate workers earning in excess of the sum of the occupation-related wage and maximum bonus. This variation reflects firm-specific arrangements.

On piece rates, workers are paid purely based on individual output. The collective agreement indicates that piece rates should be used on clearly specified task assignments, and that payment should be based on output measures such as units, kilograms, or meters produced. As well, total earnings for piece rate workers should not fall below the occupation-related wage, and thus firms should set the specific piece rate amount at a sufficiently high level. In fact, there are no piece rate workers in any year in the data with an actual hourly wage below the occupation-related threshold. On the other hand, there is no ceiling on the piece rate or hours worked. Piece rates are the least common payment scheme in the industry, covering only 10% of total hours worked.

The final type of compensation contract in this industry provides for reward rates, which are a mix of piece rates, fixed rates, and a team-based bonus.

Data

The data come from the records of the Confederation of Finnish Industry and Employers. They contain all payroll records, including earnings and hours worked for all workers who are employed in firms affiliated with the Confederation. In the case of manufacturing in Finland, this covers virtually all firms—hence our focus on the manufacturing sector. We have access to yearly information on the blue-collar population of the metal industry from 1990 to 2000.

As will be discussed in more detail below, the data on job complexity available for the metal industry are notable for their exceptional detail and reliability. Each observation in our data contains the accumulated hours worked and earnings within the last quarter of each calendar year.[7] After eliminating some observations due to missing information, we have a panel of 601,812 employee-year observations representing 120,182 workers from 602 firms. The average number of years of observations per worker is 5.5.[8] Appendix 1 shows the distribution of observations across firms and years. Table 1 presents summary statistics on the key variables.

In addition to the variables listed in Table 1, the data contain rich detail on the nature of the individual's job (discussed further below), and also include years of education beginning in 1996. Thus, for those individuals who permanently exited the data prior to 1996, we have no education information. The results presented in the paper exclude education, but we replicate all of our analysis for the years 1996–2000 including education, and the results are virtually unchanged. We also test the sensitivity of the results to the 1992–95 period—the recession years, as noted in the "Institutional Details" section above—and the results are again unaffected.

A noteworthy feature of the data is that they allow us to observe the exact share of hours that an individual works on a given contract. This information reveals that 39% of male workers in this industry always worked on a fixed rate schedule (that is, 100% of hours on a fixed rate contract), about 1% always worked on piece rates, and 10% always

[7]The data are compiled on a quarterly schedule, but we were only given access to the last quarter for each year.

[8]A second sample will be used for the 1993 to 2000 period, for which we have additional information on the plant. If individual plants within firms had autonomy over compensation policy and other practices unobserved by us, it may be more appropriate to treat the plant as the "firm unit" rather than the firm. For the 1993 to 2000 period, there were a total of 691 plants from 434 firms. Ultimately, using plants instead of establishments made no difference to the results, and so, for brevity, these estimates are omitted.

Table 1. Summary Statistics.

	Men		*Women*	
Variable	*Full Sample*	*Movers*	*Full Sample*	*Movers*
Average Real Hourly Wage	9.92 (1.62)	10.14 (1.67)	8.27 (1.27)	8.19 (1.17)
Fixed Rate Hourly Wage[a]	9.59 (1.98)	9.72 (2.11)	7.89 (1.36)	7.76 (1.45)
Reward Rate Hourly Wage[a]	10.38 (4.38)	10.72 (2.80)	8.67 (1.33)	8.69 (1.39)
Piece Rate Hourly Wage[a]	11.16 (3.96)	11.38 (3.64)	8.65 (1.60)	8.43 (1.51)
Age	38.20 (10.50)	38.69 (9.87)	40.13 (10.73)	40.42 (10.10)
Years of Experience in Industry	12.45 (9.97)	13.06 (9.74)	10.27 (8.45)	11.32 (8.61)
% of Hours Worked on Piece Rate Contract	.104 (.275)	.130 (.306)	.120 (.285)	.152 (.316)
% of Hours Worked on Reward Rate Contract	.347 (.456)	.391 (.458)	.357 (.452)	.347 (.447)
Job Complexity	7.51 (.857)	7.57 (.833)	6.56 (.774)	6.51 (.773)
Single Shift	.618 (.486)	.604 (.489)	.575 (.494)	.574 (.495)
Double Shift	.212 (.409)	.252 (.434)	.233 (.423)	.263 (.440)
Triple Shift	.170 (.376)	.144 (.351)	.192 (.394)	.164 (.370)
Firm Size	982.81 (1287.13)	721.96 (873.02)	789.31 (983.09)	652.44 (684.40)
Part-Time	.039 (.193)	.034 (.211)	.047 (.212)	.046 (.210)
Number of Observations	470,586	35,710	131,226	9,315
Number of Individuals	91,515	14,778	28,667	4,020

Notes: Standard deviations are in parentheses. Wages are reported in 2000 Euros. Movers are the subset of the full sample who switched firms (at least once).

[a]Variable is defined for only a subset of the full sample.

worked on reward rates. The numbers are very similar for women. Thus, for half of the blue-collar metal industry population, the incentive effect of performance pay cannot be estimated, since the counterfactual is not observed. Also of note is that among the 50% of workers who experienced a change in their contract, many worked on different contracts within the same year. This is a unique form of variation that we exploit: some of the variation in contracts comes from individuals changing from a 100% fixed rate in year t to a performance pay contract in year $t + 1$ (and vice-versa); and some comes from individuals changing, between t and $t + 1$, the mix of time spent on a fixed rate versus performance pay contract. Unfortunately, the payroll records from the Confederation do not separate the part of reward rate pay that was output-based from the part that was fixed. The exact share of output-determined (either individual or team) earnings may vary across firms and across tasks.

To shed more light on the variation in performance pay, we conduct a simple analysis of variance. In particular, we regress the piece rate share, as well as the reward rate share, on firm dummies with and without individual and task characteristics.[9] Overall, firm dummies account for approximately 40% of the variation in performance pay, while observed individual and task characteristics account for less than 10%. It would seem important, therefore, for any analysis of the effects of performance pay to consider firm effects.

Empirical Analysis

The empirical analysis is in three parts. We begin by estimating the effect of performance pay on earnings in the presence of both individual and firm unobserved heterogeneity using all the data. Second, we pursue heterogeneity in the effect of performance pay along a potentially important dimension: jobs of different complexity levels. Finally, we address some of the shortcomings of the first set of analyses by exploiting information on the reason for the change in contract: first by using establishment closures, and second by analyzing several "quasi-natural experiments" where a compensation regime change was made in one plant of a firm but not in another.

Linked Employer-Employee Analysis with Full Data: Econometric Issues

We begin by analyzing the impact of performance pay on earnings using the full data. Our regressions of interest have the general form

$$(1) \qquad Y_{it} = \beta P_{it} + X_{it}\gamma + Z_{jt}\pi + \sum_{j=1}^{J} \delta_j F^j_{it} + \alpha_i + \varepsilon_{it},$$

where i is an index for the individual, j for the firm, and t for the year; the dependent variable is the log of the real hourly wage; the share of hours worked on a performance pay contract (two separate variables: piece rates and reward rates), P_{it}, varies across individuals, firms, and time; X_{it} is a vector of observable employee characteristics and Z_{jt} is observable firm characteristics (which is limited to firm size); δ_j represents the firm effect and F^j_{it} is a dummy that equals one if individual i is employed in firm j at time t; α_i is the individual effect; and, finally, ε_{it} is an error term. Note that (1) includes no time-invariant covariates. The firm and individual unobserved heterogeneity are correlated with the other covariates and each other. Of course, we still require the assumption that ε_{it} is strictly exogenous. This assumption is often referred to as "random mobility," and implies that the movement of workers between firms over time is independent of ε_{it}. Workers' decision to switch firms may be a function of the covariates.

Equation (1) can be estimated through three main methods: the least squares dummy variable estimator ("LSDV"), the Abowd, Creecy, and Kramarz (2002) direct least squares estimator ("DLS"), and an "employee-employer-match" fixed effects estimator ("EEMFE"). Abowd, Creecy, and Kramarz provide a discussion of these estimators as well as other econometric issues pertaining to linked employee-employer data. We ignore the LSDV and DLS estimators because implementation is problematic, and useful only if one is interested in computing the estimates of δ_j and α_i.[10] The latter are unnecessary for our purposes; moreover, their estimation has been subject to some critical scrutiny (for example, Andrews, Schank, and Upward 2006).[11] Subject to the assumptions listed

[9]For brevity, we omit these results, but they are available upon request.

[10]Implementing the LSDV estimator is not straightforward given data such as ours: our panel is unbalanced, since firms and workers could enter and exit the data, and there is no regular pattern between the individual and firm dummies. As a result, it is not possible to use the LSDV estimator on firm-differenced (and individual-differenced) data as in the standard panel data case, but rather a set of firm dummies must be included in the individual-differenced data. This leads to the computational issue of inverting a $(k+J) \times (k+J)$ matrix, where k is the number of covariates. In our data, we were unable to estimate (1) for all 11 years using the LSDV estimator, and were also unable to estimate (1) for even a subset of years when including the 164 occupation dummies. We have a variety of results from the LSDV estimator for other specifications, and the LSDV results are identical to the EEMFE results—as should be the case.

[11]As discussed in Andrew, Schank, and Upward (2006), another potential problem with computing estimates of the unobserved individual and firm component is sampling error. This is because the unobserved indi-

above, the EEMFE estimator yields consistent estimates by taking differences within each unique employee-firm combination, and is thus straightforward to implement. The intuition for this estimator is simply that for each unique employee-employer match, δ_j and α_i are removed when subtracting means at the level of the match.[12]

Linked employee-employer panel data are of special value only if people switch firms; without such mobility, the data are identical to regular panel data. Overall, we have a substantial amount of mobility: as seen in Table 1, of the 91,515 men in the data, 14,778 switched firms at least once. The incidence of movement across firms is similar for women. Table 1 also presents summary statistics for the sample of movers. It is important to note that the assumptions made in estimating (1) do not require the movers to be a random sample; what matters is what causes movement. For instance, if movement across firms is driven by the quality of the employee-employer match (that is, the matching of α_i's and δ_j's), then the random mobility assumption likely holds regardless of how the characteristics of movers differ from those of stayers. Conversely, a finding that movers and stayers have identical characteristics would not necessarily validate the random mobility assumption; it would, however, strengthen the *prima facie* case for that assumption. Table 1 reveals that the movers and stayers had very similar individual characteristics; the only systematic difference between them was in firm size. Compared to stayers, movers were slightly more likely to be part-time workers, were more likely to be on a performance pay contract, and differed in minor respects in their pattern of shift work, but these differences were all on the order of only 1–2.5 percentage points. Average real hourly wages, job complexity, age, industry tenure, and (for 1996 onward) education were virtually identical across the two groups.

Linked Employer-Employee Analysis with Full Data: Results

Table 2 presents the results from equation (1) estimated by EEMFE as well as a simple OLS regression that does not include controls for unobserved individual or firm-specific heterogeneity. The results from the earnings regressions are suggestive of both strong incentive and selection effects, a finding consistent with the previous literature. For piece rates, the simple OLS model yields an estimate of .15 for men, indicating that a change from a fixed rate contract to a 100% piece rate contract was associated with a 15% increase in hourly earnings. For reward rates, the OLS estimate is .08. Subject to the fixed effects assumptions discussed above, the incentive effect of piece rates on earnings is about .10. This amounts to 60% of the OLS estimate, implying a selection effect of 40%, a somewhat smaller role for selection than has generally been found in previous studies. The story is the same for reward rates, with the .08 coefficient declining to about .06 when estimated by the EEMFE. For women the results are similar, with performance pay estimates about 1–2 percentage points higher than those found in the male sample.[13]

vidual effects are "backed-out" after computation of the firm effects (which are just the coefficients on the firm dummies), and thus if the coefficient on a given firm dummy is, for example, overstated, the individual effects will be understated and vice-versa. Andrew, Schank, and Upward argued that this is the reason why virtually all employee-employer studies find a negative relationship between the unobserved individual and firm effects.

[12]This is a simple and very useful procedure. To illustrate, imagine a 12-year panel with the following three types of individuals: (a) stays with the same firm for all 12 years; (b) works at two firms for 6 years each; (c) works at three firms for 4 years each. In (a), the firm identifier is the same for all years and thus the individual is counted as one match (and so is treated in the same way as in standard longitudinal data). In (b) there are two sets of observations (or two distinct employer-employee "matches"), 6 years with one firm identifier and 6 years with another. In (c) there are three distinct matches. To implement the EEMFE estimator, you simply mean-difference the data at the level of the match (that is, within each individual-firm matched set of observations instead of within each individual). Assuming there is mobility across firms, this means that you have more sets of observations that are mean-differenced than in standard panel data; in our case, 113,919 male-firm matches (instead of 91,515 men) and 34,772 female-firm matches (instead of 28,667 women).

[13]For the other estimates, the age and industry tenure estimates are consistent with the voluminous literature

What are the productivity gains associated with these earnings premiums? We do not have the data to provide any insights into this question, but Lazear (2000) found that a 20% incentive effect on productivity translated into a 9.6% hourly wage premium. Based on hourly earnings, our piece rate results are almost identical to Lazear's in a very different setting.

Our estimates reveal at least two patterns that, given the industrial relations framework in this industry, are curious enough to warrant mention. First, recall that the comparison fixed rate contract includes a substantial bonus component, which ranges from 2% to 17%, with a mean of 9.5%. The collective agreement for this industry specifies that these bonuses should be based on the supervisor's subjective evaluation of the individual's performance. Given that our estimates are similar to those from previous studies in which the comparison contract was truly a fixed wage rate contract, the results may imply that subjective, performance-based compensation plans with a variable component that accounts, on average, for about 10% of base pay have little effect on productivity. Second, recall that every job (based on complexity points) has its own specific minimum wage that is binding regardless of pay scheme. It may be reasonable to assume, therefore, that workers would be more willing to incur the risk of a performance pay contract than in other institutional settings. Yet, as noted above, the selection effect—as indicated by the ratio of the EEMFE to OLS estimates—is similar to that estimated in the United States (Lazear) and Canada (Shearer). Going back to the theory, there appear to be (at least) two possible explanations for this oddity: (a) that risk aversion is not as important as typically believed, and (b) that workers actually know their productivity quite well, and that it is fairly constant.

Also noteworthy is the similarity of the patterns found for men and women throughout our results, including the results discussed below. Some recent studies provide evidence that men outperform women in competitive environments—which may apply to a piece rate setting—as well as in work environments demanding extensive physical labor—which certainly describes many of our occupations. For instance, Paarsch and Shearer (2007) found large productivity differences between men and women in piece-rate-based tree-planting in Canada. (The authors, however, attributed this finding entirely to ability—which, in the setting they examined, importantly included physical strength—rather than to gender differences in responses to an incentive.) Gneezy, Niederle, and Rustichini (2003) provided lab-based experimental evidence from Israel that men outperformed women in tournament-pay environments, but found no gender differences in productivity in piece rate and non-competitive environments. Granted that the work settings in Gneezy et al. and Paarsch and Shearer (respectively, maze-solving by individuals, and tree-planting over large areas) may have little in common with a plant-based manufacturing environment, it could be that individuals do not view piece rate environments as competitive, in which case our findings are consistent with these recent studies. As a preview to the results below, we note, however, that we find identical piece rate premiums across gender even in low-complexity tasks—which tend to include more physical activities.

One possible explanation for our findings is the institutional or cultural background. For instance, the highly egalitarian nature

on wage determination. The shift-work dummies suggest that individuals who work on non-standard shifts may have poorer unobservables, as the negative coefficients vanish for both genders when fixed effects is used for estimation; in fact, the shift-work coefficients become positive for men, which would be consistent with a compensating wage for shift work. The firm size premium under OLS is reduced substantially for both genders, and becomes negative for men under fixed effects (consistent with the literature on firm size wage premiums). The job complexity premium is dramatically lower for both genders when fixed effects models are used. This suggests part of the OLS estimate is due to unobservables; in particular, firms likely match better workers to more complex tasks (note that the complexity estimate is less than one because virtually all individuals are paid above the occupation-specific minimum wage). Finally, for part-time work, there is an inconsistency between men and women, with a premium estimated for men, which increases under fixed effects, but a wage penalty for women.

Table 2. Estimated Coefficients from Hourly Earnings Regressions.

	Men		*Women*	
Variable	*OLS*	*EEMFE*	*OLS*	*EEMFE*
Piece Rate Share	.147***	.089***	.160***	.103***
	(.012)	(.006)	(.012)	(.007)
Reward Rate Share	.079***	.053***	.090***	.069***
	(.009)	(.007)	(.009)	(.005)
Age	.090***	—	.035***	—
	(.007)		(.006)	
Age Squared	–.010***	–.016***	–.004***	–.009***
	(.001)	(.001)	(.001)	(.001)
Industry Tenure	.042***	—	.055***	—
	(.004)		(.005)	
Industry Tenure Squared	–.008***	–.004***	–.011***	–.012***
	(.001)	(.002)	(.002)	(.002)
Log of Job Complexity	.723***	.466***	.772***	.535***
	(.031)	(.042)	(.033)	(.061)
Double Shift	.004	.006***	–.014**	.000
	(.005)	(.002)	(.005)	(.004)
Triple Shift	–.013*	.009**	–.018**	–.001
	(.007)	(.004)	(.008)	(.005)
Part-Time Dummy	.005	.009**	–.001	–.008***
	(.005)	(.005)	(.005)	(.003)
Firm Size (∗100)	.001***	–.001	.002***	.001
	(.000)	(.001)	(.000)	(.001)
Constant	.885***	2.33***	1.00***	1.89***
	(.110)	(.156)	(.106)	(.057)
R Squared	.63	.91	.65	.91

Notes: The dependent variable is the log of real hourly earnings. Standard errors are in parentheses, and are adjusted for clustering at the firm level. The number of observations is 470,586 for men and 131,226 for women. The number of firms is 602; the number of individuals is 91,515 and 28,667 for men and women, respectively; and the number of employee-employer matched dummies for the EEMFE estimator is 113,909 for men and 34,772 for women. All regressions include 10 year dummies and 165 occupational dummies.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

of the Finnish labor market could be such that there is pressure in firms (coming either from management or from among the work force) for piece rate production (or perhaps the hours allocated to a piece rate contract) to be equalized across genders. As well, the Finnish *Act on Equality between Men and Women,* which became effective in 1986, specifies very rigid controls to ensure gender equality along a wide variety of conditions of employment. While this legislation implies that there should be equal access to performance pay contracts rather than that equal productive output should somehow be imposed, such legislation may also indirectly provide pressure to equalize earnings. Unfortunately, without information on the actual piece rate itself and output, we cannot draw more definitive conclusions.

Heterogeneity in Impact: Job Complexity

We also investigate whether the incentive effect depends on the complexity of the individual's job. Economic theories of compensation design tend to focus on monitoring costs—piece rates tend not to be used in complex jobs because monitoring output in such jobs is too difficult or too costly. Another possibility that has not been thoroughly tested is that it is more difficult for workers to respond to a simple output-

Table 3. Estimated Coefficients from Hourly Earnings Regressions: Estimation by Job Complexity Quartile.

	Men				*Women*			
	OLS		*EEMFE*		*OLS*		*EEMFE*	
Quartile	*Piece Rate Coefficient*	*Reward Rate Coefficient*	*Piece Rate Coefficient*	*Reward Rate Coefficient*	*Piece Rate Coefficient*	*Reward Rate Coefficient*	*Piece Rate Coefficient*	*Reward Rate Coefficient*
Complexity Quartile 1	.158	.097	.111	.069	.160	.095	.105	.072
	(.011)	(.007)	(.010)	(.010)	(.012)	(.009)	(.009)	(.007)
Complexity Quartile 2	.150	.079	.085	.048	.177	.078	.100	.052
	(.011)	(.009)	(.009)	(.007)	(.027)	(.012)	(.016)	(.012)
Complexity Quartile 3	.145	.073	.083	.046	.135	.068	.089	.051
	(.015)	(.009)	(.008)	(.008)	(.017)	(.010)	(.023)	(.010)
Complexity Quartile 4	.128	.065	.042	.032	.137	.052	.025†	.044
	(.017)	(.012)	(.012)	(.011)	(.045)	(.013)	(.074)	(.014)

Notes: The dependent variable is the log of real hourly earnings. Standard errors are in parentheses, and are adjusted for clustering at the firm level. All estimated coefficients except that flagged with a dagger are statistically significant at the 1% level. All regressions include controls for age (and its square), industry tenure (and its square), job complexity, shift-work, part-time work, firm size, year dummies, and occupation dummies..

based performance measure when their job is complex and involves multiple tasks. To explore the interaction between pay methods and job complexity, we estimate our earnings regressions by complexity quartiles.

The results (Table 3) do show an apparently strong interaction between performance pay incentive effects and job complexity. The estimated performance pay earnings premiums for piece rates for men fall from around 11% in the bottom complexity quartile to 8% in the middle two quartiles, and then down to 4% in the top complexity quartile. A very similar decline is seen for reward rates. The trend for women is generally the same. The earnings premium effect of performance pay thus appears to decline markedly with job complexity. Moreover, the role of selection into performance pay contracts seems to increase with complexity as well. As noted above, the ratio of the EEMFE estimate to the OLS estimate gives some indication of the selection effect. An examination of this ratio by complexity level reveals that the ratio of the EEMFE estimate to the OLS estimate decreases markedly as complexity increases (except for reward rates for women). For instance, the EEMFE: OLS estimate ratio is 70% for piece rates in the first complexity quartile (essentially the same across sexes), versus 30% in the fourth complexity quartile.

The findings above may support the argument that jobs with high levels of multi-tasking pose problems for incentive pay contract design. For instance, if it is more difficult to respond to a simple performance measure (such as a piece rate) in a more complex job, we might expect a lower productivity effect and hence a lower wage effect, as well as a greater role for unobservables in the selection of workers into performance pay contracts—perhaps because of greater risk in being in a complex job on a piece rate contract, or because of the (unobserved) characteristics required to perform well in such jobs under performance pay. Unfortunately, we are limited in what we can say about the role of multitasking, as we do not observe the actual piece rate. It may be, for example, that the underlying contracts (that is, the piece rates themselves, which are unobserved) in the more complex positions are such that employers do not give employees in complex jobs the same incentive they give employees in less complex jobs.

A possible implication of the results by complexity is that the previous findings of Lazear (2000) for windshield installers and Shearer (2004) for tree-planters may overstate the productivity gains that a more typical establishment could expect to achieve by implementing performance pay.

Preliminary evidence on firms' choice of pay scheme also provides some support for the hypothesis that the degree of multitasking in a job plays a part in this decision, at least in the case of piece rates. Appendix 2 shows mean use of performance pay across firm size and complexity quintiles for piece rates and reward rates separately. Firm size provides a measure of monitoring costs. There is a clear negative correlation between job complexity and the use of piece rates. This is also true for the firm size–piece rate relationship. In general, piece rates were used in small, less complex firms. Reward rates exhibit a quite different pattern; indeed, based on this highly preliminary evidence, it appears that the use of reward rates was based on a set of establishment characteristics very different from those associated with piece rates.

Evidence from Establishment Closures

While the fixed effects estimator allows us to control for unobserved individual-level and establishment-level heterogeneity, strong assumptions are still made. We have discussed the random mobility assumption. Another restrictive assumption is the time-invariant nature of the fixed effects, particularly for α_i. One criticism of fixed effects methods applied to compensation policy and earnings is that learning processes may change with the contract. Theories of performance pay such as those developed by Lazear and by Booth and Frank emphasize effort and ability as the inputs to individual production where ability is constant. Parent (2007) discussed an alternative view in which effort and skill—which consists of two components: observed skills such as formal training/education, and unobserved skills—are the inputs to individual output. If firms (and possibly workers themselves) learn about an individual's (initially) unobserved skills over time and change the pay method based on this learning process, the time-invariant assumption of individual-specific unobserved heterogeneity will be violated.

There is a straightforward way to test the assumptions of the fixed effects approach. In particular, the methods outlined above (under "Econometric Issues") assume that the average wage change for those individuals who switch from fixed rates to piece rates is the same as the average wage change for those who change from piece rate to fixed rate contracts. The data indicate that the latter assumption is likely violated, especially for piece rates. In particular, a move from fixed to piece rates is associated with an average wage change of about 5 log points for men and 3.5 log points for women, while a change from piece to fixed rates is associated with essentially no wage change for either sex. The pattern is similar for reward rates, but the wage gain associated with a change from fixed to reward rates is about half that associated with the change from fixed to piece rates (and even less for women). Thus, for reward rates (especially for women), a fixed effects approach may be palatable, but overall the assumptions of fixed effects models are likely violated in this setting.

The underlying problem with fixed effects estimators in this context is that we do not observe the reason for the change in compensation scheme. Fortunately, several available sources of information allow for some control over the reason for change in contract. First, we know the reason for a job separation, and thus can examine three subsamples: stayers (workers who stayed in the same firm between time t and time $t+1$), displaced workers (workers who switched firms because of an establishment closure between time t and time $t+1$), and "other movers." For the stayers subsample, identification is driven by a change in contracts between time t and time $t+1$ within the same firm. For the other samples, the performance pay estimates are identified only by a change in contracts across firms. In particular, the displaced worker sample compares individuals whose contract changed following establishment closure with those whose contract did not change after moving between firms following layoff. We emphasize that the displaced worker sample only uses establishment closures; individuals involved in layoffs where the firm did not shut down are included in the "other mover" sample.

Table 4. Estimated Coefficients from Hourly Earnings Regressions: Estimation by Reason for Separation.

	Men		Women	
Sample	Piece Rate Coefficient	Reward Rate Coefficient	Piece Rate Coefficient	Reward Rate Coefficient
Stayers	.092	.049	.104	.066
	(.008)	(.010)	(.010)	(.008)
Establishment Closures	.074	.034	.071	.037†
	(.008)	(.009)	(.013)	(.012)
Other Movers	.119	.064	.139	.081
	(.011)	(.008)	(.014)	(.010)

Notes: All regressions are estimated by first-differences. The dependent variable is the log of real hourly earnings. Standard errors are in parentheses, and are adjusted for clustering at the firm level. All estimated coefficients except that flagged with a dagger are statistically significant at the 1% level. All regressions include controls for age (and its square), industry tenure (and its square), job complexity, shift-work, part-time work, firm size, year dummies, and occupation dummies.

Table 4 presents the results. The estimates for the stayers sample and "other changer" samples are similar and consistent with the earlier estimates: for stayers and "other changers," the piece rate premiums were, respectively, 9.2% and 11.9% for men and 10.4% and 13.9% for women, and the reward rate premiums were 4.9% and 6.4% for men and 6.6% and 8.1% for women. However, the estimates from the sample of displaced workers are much lower, at 7.4% for piece rates and 3.4% for reward rates in the male sample (and a statistically insignificant effect for women for reward rates—although the sample size is very small in this case).

The displaced worker sample is the one case in which workers likely have less control over the reason for change in their contract; alternatively stated, the change in contracts for this subsample is likely the most exogenous. Indeed, exogeneity in the change of contracts may hold on both sides: first, the change in contract arising from a firm closure is presumably exogenous; and second, given the mass nature of the layoff, the individual likely has much less control over the contract in the new job than he or she has in other moves across firms. Arguments in favor of learning or comparative advantage in compensation policy (such as Parent 2007) would likely only apply to stayers and possibly voluntary movers. If the learning notion is correct, then stayers who change contracts should be the individuals for whom performance pay "works," and thus should obtain larger earnings premiums. The learning argument could also hold for voluntary movers if they change from a fixed-rate firm to a piece-rate firm specifically because of this learning process. Overall, there is some evidence that the performance pay incentive effects are lower for individuals who changed contracts for reasons that are more likely to be exogenous to potentially time-varying unobserved factors. Nevertheless, the earnings premiums even for the displaced worker sample are substantial, particularly for piece rates.

Quasi-Experimental Evidence

Our final analysis examines cases in which a firm made a change in pay method in one plant, but not in another. Unique plant identifiers only became available in 1993, and thus this analysis covers the 1993 to 2000 period. We examined compensation policy over this eight-year period for all firms and plants, and identified five regime changes. Our only sample restriction is that a plant had to have been under each of the two regimes for at least two years.

Not surprisingly, when we examine compensation policy by occupation, we find that for some job types in the treatment plant (that is, the regime change plant) there was

Table 5. Compensation Regime Changes within Firms.

	1993	*1994*	*1995*	*1996*	*1997*	*1998*	*1999*	*2000*
				Firm 1—Fixed Rate to Piece Rate Change				
Treatment								
% on Piece	0	0	0	1	1	—	—	—
Control								
% on Piece	0	0	0	0	0	—	—	—
				Firm 2—Fixed Rate to Piece Rate Change				
Treatment								
% on Piece	0	0	0	0	.32	.31	.31	.23
Control								
% on Piece	0	0	0	0	0	0	0	0
				Firm 3—Piece Rate to Fixed Rate Change				
Treatment								
% on Piece	.90	.91	0	0	0	0	0	0
Control								
% on Piece	0	0	0	0	0	0	0	0
				Firm 4—Fixed Rate to Reward Rate Change				
Treatment								
% on Reward	0	0	0	0	0	1	1	1
Control								
% on Reward	1	1	1	1	1	1	1	1
				Firm 5—Reward Rate to Fixed Rate Change				
Treatment								
% on Reward	.75	.79	1	0	0	0	0	0
Control								
% on Reward	1	1	1	1	1	1	1	1

no change in pay scheme. We focus only on those occupations that did experience a change, and then use the same occupations in the control plant as in a comparison group. In almost all cases the occupations in the treatment group affected by the compensation change also existed in the control group. In the minority of cases in which they did not, we restrict the sample to occupations that existed in both the regime change plant and the control plant. Table 5 shows the evolution of compensation policy for these cases, with "percent of performance pay" denoting the fraction of employees in the plant who were working on a performance pay contract. The plant that had its compensation scheme changed is denoted the treatment plant, while another plant in the same firm that did not experience a change in compensation policy is denoted the control plant.

Two of these firms, identified as firms 2 and 5, had multiple plants that could have been used as the control group; in neither case was there a control plant that clearly represented the best control group, and so, for brevity, we present only the results that pool the control plants. The results are very similar when we separate the control plants. In one case, firm 4, there are two treatment plants; there were no statistically significant differences pre-regime changes between these two plants, and thus we also pool them. Again, the results are virtually unchanged if we examine them separately. One interesting feature of these firms is that they give us a mix of different types of performance pay policy changes: some firms adopted piece rates, one adopted reward rates, and others abolished performance pay.

Figures 1–5 show average real hourly earnings for each experiment, while Table 6 quantifies the impact of the compensation

Figure 1. Average Real Hourly Earnings in Firm 1.
(treatment plant adopted piece rates in 1996; control plant used fixed rates)

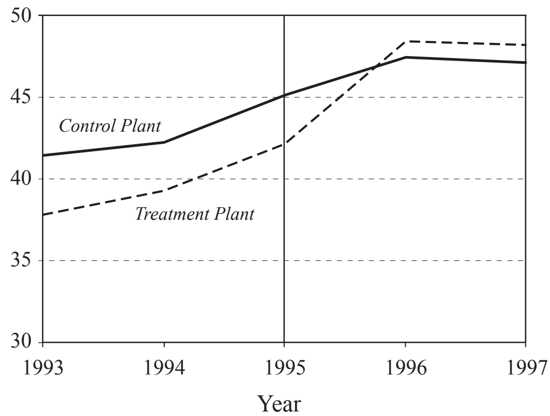


Figure 2. Average Real Hourly Earnings in Firm 2.
(treatment plant adopted piece rates in 1997; control plant used fixed rates)

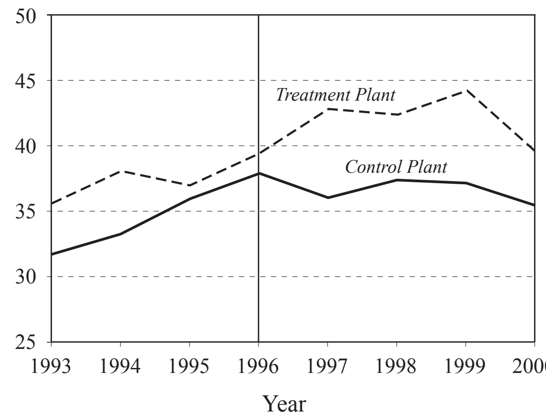


Figure 3. Average Real Hourly Earnings in Firm 3.
(treatment plant eliminated piece rates in 1995; control plant on fixed rates)

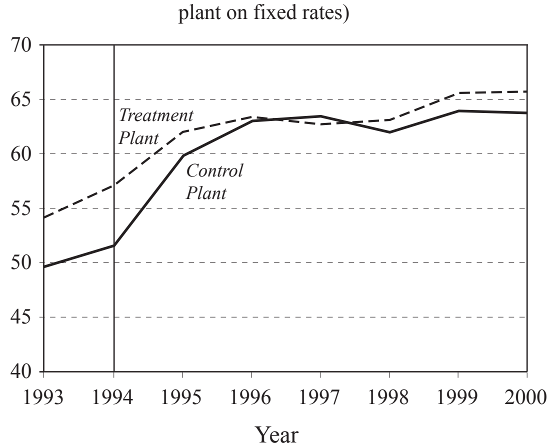


Figure 4. Average Real Hourly Earnings in Firm 4.
(treatment plant adopted reward rates in 1998; control plant used reward rates the entire period)

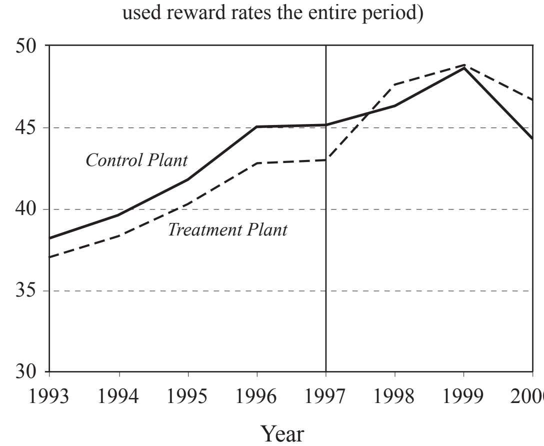
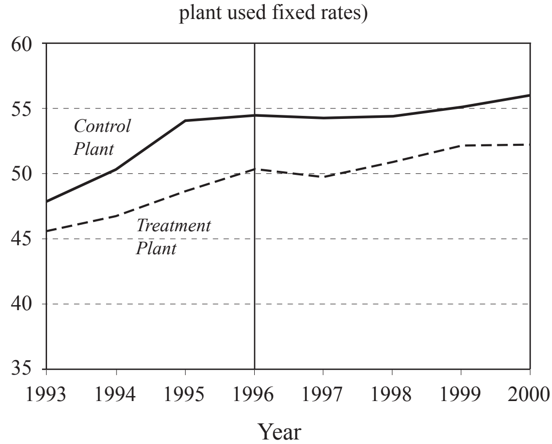


Figure 5. Average Real Hourly Earnings in Firm 5.
(treatment plant eliminated reward rates in 1997; control plant used fixed rates)



regime change. Below, we briefly review the findings in each experiment, with particular attention to whether the experimental conditions of common support and common trend hold pre-regime change. Appendix 3 presents summary statistics on the pre-regime change characteristics for each treatment-control plant combination. We explicitly tested for a common trend in earnings prior to the regime change, which is based on earnings regressions with a treatment dummy, year dummies, and interactions between the two over the pre-treatment period only. Common trend holds if the coefficients on the interaction terms are not statistically different from zero.[14] Common support can be seen in Appendix 3, and indicates whether there is a statistically significant difference in pre-treatment period characteristics between the control plant and the regime change plant. Overall, while in most cases there are some important differences in observable characteristics between the control plant and the treatment plant pre-regime change, the plants are highly similar, and common trend holds in virtually all year-to-year transitions except for Firm 5.

We begin with the piece rate cases. Firm 1 was relatively small and only changed the compensation policy of three occupations; the control plant did not make such a change for these same occupations. Common support holds in almost all cases, although the sample size is small. Nevertheless, Appendix 3 reveals a high degree of similarity pre-regime change. The gap in hourly earnings before the change in pay scheme is likely due to the treatment plant being in a rural location, which is associated with a large earnings penalty in the industry as a whole. Common trend holds, with an almost identical earnings path pre-regime change. As seen in Figure 1, the effect of the regime change was striking. After the treatment plant switched to piece rates, average real hourly earnings increased by 15% relative to the control plant pre-treatment.

Firm 2 also adopted piece rates in the treatment plant for a small number of occupations, but only one-third of employees within these affected occupations were switched. The control plant is much less comparable to the treatment plant in this firm than in firm 1. In particular, the workers in the control plant were about 4 years older on average, and had 2 more years of experience in the industry, than those in the treatment plant, and 40% of them were female, compared to only 5% in the treatment plant. Nevertheless, common trend holds for two of three years pre-regime change, as seen in Figure 2, and job complexity differed by only a single point. Note that the overall complexity point range for the blue-collar metal industry is 26.5 to 41.1 points. The results reveal an average effect of 8%, lower than in the case above, but of course fewer workers were switched to piece rates.

Firm 3 provides an interesting contrast, as piece rates were *abolished* in the treatment plant in this firm, while the control plant never used piece rates. About 90% of employees in affected occupations in the treatment plant were switched. While there is a substantial difference in work arrangements (single shifts are twice as likely in the treatment plant), the two plants were highly similar pre–regime change, with no statistically significant differences in age, tenure, job complexity, or gender composition (and a minor difference in part-time work), and a common trend in earnings. The effect of the regime change was striking, as illustrated in Figure 3. After piece rates were eliminated in the treatment plant, the large gap in earnings almost entirely closed, with the policy impact estimated at –7.4%.

Firms 4 and 5 are examples of reward rate regime changes. We emphasize here that there is, unfortunately, no way to know the nature of the reward rate contract—it could have provided for a piece rate labeled as a reward rate or it could have mandated no piece rate component at all, but a team bonus instead.

In Firm 4, reward rates were adopted in two different treatment plants while the control plant used reward rates over the entire period. As noted above, we pool the treatment plants. The treatment and control plants were identical in terms of job complex-

[14]The regression results from the common trend tests are available upon request.

Table 6. Estimated Coefficients from Hourly Earnings Regressions: Estimation from Within-Firm Compensation Regime Changes.

Variable	*Firm 1* *Fixed to Piece*	*Firm 2* *Fixed to Piece*	*Firm 3* *Piece to Fixed*	*Firm 4* *Fixed to Reward*	*Firm 5* *Reward to Fixed*
Treatment Plant	–.145***	.054	.093***	–.058***	–.083***
	(.026)	(.037)	(.008)	(.004)	(.007)
Treatment Period	.038*	.003	.212***	.060***	.074***
	(.022)	(.033)	(.007)	(.003)	(.003)
Treatment Plant * Treatment Period	.152***	.083**	–.074***	.112***	.015*
	(.037)	(.040)	(.009)	(.006)	(.008)
Number of Observations	138	310	3,541	5,932	1,424

Notes: All regressions are estimated by OLS. The dependent variable is the log of real hourly earnings. Robust standard errors are in parentheses. All regressions include controls for age (and its square), industry tenure (and its square), job complexity, shift-work, part-time work, and firm size.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

ity, but there were some sizable differences along other dimensions: in particular, the treatment plant employees were much older (and experienced), and they worked exclusively on single shifts (compared to only 21% of employees in single shifts in the control plant).[15] Nevertheless, there is a common trend in earnings overall in the entire pre-regime period. Again, the effect of regime change is striking, as illustrated in Figure 4. After performance pay was introduced, real hourly earnings in the treatment plant surpassed those in the control plant, with an overall effect estimated at 10%.

The plants in Firm 5 are the least comparable pre-treatment of all cases. Indeed, this is the only case in which the common trend test fails. In this case, reward rates were used in both plants, but then abolished in the treatment plant. As seen in Figure 5, very little happened to earnings—the compensation regime change appears to have had no effect.

Overall, these quasi-experiments offer compelling evidence that the various regime shifts, especially the piece rate regime shifts, had a causal effect on earnings. We emphasize that this does not necessarily mean that compensation policy caused the change in earnings; there may have been other unobserved changes in addition to the new compensation policy. That said, the data allow for a fairly rich profile of the firm (detailed occupations, job complexity, work arrangements), and still the regime changes—each of which involved a dramatic change in contracts—were associated with substantial earnings effects. Finally, we also emphasize that while these earnings changes that apparently occurred in response to the change in contracts are consistent with productivity effects as illustrated in the theories of Lazear (1986, 2000) and Booth and Frank (1999), we have no data on productivity, and thus there may be other explanations for why wages fell with the elimination of piece rates and increased with the introduction of piece rates.

Conclusions

There is considerable interest in whether performance pay increases productivity.

[15]On the surface, Firm 4 may seem anomalous, given that hourly earnings were higher in the control plant despite this plant having substantially younger and less experienced workers than the two treatment plants did. Of course, one possibility is that the reward rate contract is responsible for this pattern; indeed, the treatment plants caught up to or surpassed the control plant after performance pay was introduced. Another likely source of the earnings differential is plant size, which has a strong, positive correlation with hourly earnings. In most cases, the control and treatment plants were of similar size; in firm 4, the control plant was much larger than the two treatment plants combined.

Economic theories of piece rates—where pay is entirely a function of output—predict that piece rate workers will earn more than fixed rate workers because of two mechanisms. First, higher-ability workers—whose unobserved characteristics are such that they would earn more regardless of payment method—select into piece rate contracts (the selection effect). Second, piece rates induce higher levels of effort (the incentive effect). While previous theories have noted the role of the firm in choosing its compensation policy, previous empirical tests of incentive effects have either ignored establishment characteristics, or focused on a single firm.

In this paper we have estimated the effects of performance pay contracts—both a pure piece rate contract and a quasi piece rate contract—using linked employee-employer panel data from Finland, which are derived from payroll records. These data, covering an entire industry over an eleven-year period, not only include a rich set of individual characteristics, including detailed information on job complexity and occupation, but also have allowed us to observe the exact share of hours worked on a given contract in any given year. Finally, the linked employee-employer nature of the data has allowed us to control for both individual and establishment unobserved heterogeneity.

We find that piece rate workers earned 9–10% more than fixed rate workers, and reward rate (quasi–piece rate) workers earned 6–7% more than fixed rate workers, with women earning a performance pay premium one percentage point higher than men for both types of contracts. These estimates are about 60% of the magnitude of simple OLS estimates that control only for observable characteristics. The incentive effect declines markedly with job complexity, from 11% in the lowest complexity quartile to 4% in the highest complexity quartile. Estimates from displaced workers, where identification is driven by comparing workers who changed contracts following an establishment closure with workers who did not change contracts after displacement, reveal incentive effects about two percentage points lower.

We also exploit several quasi-experiments wherein a firm made a mass change in compensation policy within one plant, but not in another. In all cases but one, the treatment and control plants had a common trend in earnings before the regime cases, and overall the plants were highly similar with respect to observable characteristics pre-regime change. The results from these experiments are consistent with our findings from the rest of the analysis, with estimated policy effects somewhat larger than those obtained in the full sample.

It is intriguing that the earnings premiums we have estimated are very similar in magnitude to those reported in other studies that have examined occupational/industrial settings very different from Finland's. Two important next steps for researchers are to (a) investigate the reasons for the heterogeneity in performance pay effects (particularly in the context of a more representative set of firms) and (b) advance the literature on the determinants of performance pay. The latter issues are, of course, likely related. Regarding the determinants of performance pay, it is worth stressing that this is a nearly unexplored question (see McLeod and Parent 1999); ultimately, given the firms' choice over the parameters of incentive pay (that is, the underlying piece rate or the performance measure and threshold criteria in a bonus plan), we likely need to understand the decision to use performance pay in order to fully understand the effectiveness of performance pay.

Finally, this paper provides what we believe to be a novel methodological approach to linked employee-employer panel data generally, and to the study of the effectiveness of human resource management practices specifically. In particular, with linked employee-employer panel data the researcher can construct quasi-experiments either by using establishment closures or, even better, by exploiting within-firm changes across plants (or offices, or branches). Without plant-level information, one could also construct a control group from other firms (for instance, by using matching procedures). The linked employee-employer nature of the data allows one to identify true *regime changes* in human resource policy. Moreover, a key feature of this research design is that, with a sufficiently long panel, the researcher can test for the experimental conditions of common support and common trend. Relative to most of the literature on the effect of human resource management practices on establishment outcomes,[16] this type of research design goes a long way toward permitting causal inferences.

[16]This is not to suggest that constructing an "artificial" experiment is better than natural experiments on single firms such as in Lazear (2000) or Banker et al. (1996); indeed, one loses important institutional knowledge with our method. That said, there are important benefits to examining a variety of regime changes such as both the adoption of piece rates and elimination of piece rates, and such analyses are not typically possible in a single-firm case study. Furthermore, while not necessarily precluded in single firm case studies, testing for experimental conditions (that is, common support, common trend) may be, in most cases, more realistic with LEEP data than in the single firm natural experiment. The key point, however, is that the methods developed here provide a much more convincing route toward the study of the causal effects of HRM practices than does the *typical* cross-sectional or longitudinal HRM study.

Appendix 1

A. Distribution of Observations across Individuals

	Men		Women	
Number of Observations per Individual	*Number of Individuals*	*Percent of Individuals*	*Number of Individuals*	*Percent of Individuals*
1	21,486	23.48	7,544	26.32
2	12,189	13.32	4,239	14.79
3	9,142	9.99	3,053	10.65
4	5,941	6.49	2,022	7.05
5	5,263	5.75	1,670	5.83
6	5,237	5.72	1,738	6.06
7	4,132	4.52	1,385	4.83
8	3,326	3.63	1,242	4.33
9	3,323	3.63	1,209	4.22
10	5,880	6.43	1,548	5.40
11	15,596	17.04	3,017	10.52
Total	91,515	100.00	28,667	100.00

B. Distribution of Observations across Firms

Number of Observations per Firm	*Number of Firms*	*Percent of Firms*
1	69	11.46
2	84	13.95
3	82	13.62
4	27	4.49
5	43	7.14
6	31	5.15
7	31	5.15
8	24	3.99
9	23	3.82
10	36	5.98
11	152	25.25
Total	602	100.00

Appendix 2

Firm Size Quintiles	*Complexity Quintiles*				
A. Piece Rates across Firm Size and Job Complexity Quintiles					
	1	2	3	4	5
1	.17	.16	.16	.14	.08
2	.19	.14	.14	.13	.07
3	.22	.11	.12	.21	.10
4	.09	.03	.08	.11	.05
5	.02	.01	.01	.04	.03
B. Reward Rates across Firm Size and Job Complexity Quintiles					
	1	2	3	4	5
1	.18	.21	.17	.19	.15
2	.27	.33	.26	.33	.28
3	.30	.36	.36	.39	.38
4	.38	.42	.48	.42	.29
5	.46	.50	.54	.49	.62

Appendix 3
Descriptive Statistics for Regime Changes

Pre-Regime Change Characteristic	*Control Plant*	*Treatment Plant*	*Pre-Regime Change Characteristic*	*Control Plant*	*Treatment Plant*
Firm 1			***Firm 4***		
Age	46.46 (1.32)	44.41 (1.42)	Age	31.62 (.180)	41.32* (.456)
Job Complexity	34.83 (.498)	34.63 (.688)	Job Complexity	33.77 (.038)	33.93 (.103)
Job Tenure	17.19 (1.40)	14.50 (1.34)	Industry Tenure	5.00 (.012)	10.07* (.310)
Female	0	0	Female	.479 (.010)	.503 (.020)
Part-Time	.017 (.017)	0	Part-Time	.017 (.003)	.009 (.004)
Single Shift	.931 (.034)	.727* (.097)	Single Shift	.213 (.008)	.997* (.002)
Firm 2			***Firm 5***		
Age	31.67 (.321)	27.63* (.940)	Age	42.21 (.315)	39.61* (.980)
Job Complexity	31.02 (.239)	32.29* (.367)	Job Complexity	36.22 (.720)	34.00* (.174)
Industry Tenure	5.33 (.896)	2.15* (.283)	Industry Tenure	17.37 (.274)	15.58* (.942)
Female	.422 (.074)	.050* (.035)	Female	.022 (.006)	0
Part-Time	.022 (.022)	.050 (.035)	Part-Time	.017 (.005)	.024 (.014)
Single Shift	.956 (.031)	.700* (.073)	Single Shift	.168 (.015)	.200 (.036)
Firm 3					
Age	35.62 (.575)	36.78 (.498)			
Job Complexity	35.66 (.133)	35.92 (.132)			
Industry Tenure	12.72 (.609)	12.39 (.441)			
Female	.053 (.013)	.033 (.009)			
Part-Time	.059 (.013)	.016* (.006)			
Single Shift	.388 (.027)	.617* (.024)			

Notes: Standard errors are in parentheses. An asterisk indicates that the treatment characteristic is statistically different from that of the control.

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