

Performance pay and earnings: Evidence from personnel records¹

Tuomas Pekkarinen†
Uppsala University

Chris Riddell‡
Queen's University

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Abstract

This paper examines the effects of performance pay on earnings using linked employee-employer panel data from Finland. With this data, we are able to estimate the effects of performance pay contracts in the presence of individual and firm unobserved heterogeneity as well as in tasks of different complexity and for the subsample of workers who change jobs following an establishment closure. Unobservable firm characteristics explain about 40% of the variance in performance pay. We find that performance pay workers earn substantially more than fixed rate workers. The effects persist when only workers who changed firms, and contracts, due to an establishment closure are used for identification. There is also a strong, negative relationship between job complexity and the incentive effects of performance pay. Finally, we exploit several ‘natural experiments’ where there was a compensation regime change in one plant of a given firm, but not in other plants. The plants are highly similar pre-regime change, and had a common trend in earnings pre-regime change. These experiments also yield substantial earnings premiums.

†Department of Economics, Uppsala University, Uppsala, Sweden, 75210. Email: tuomas.pekkarinen@ifau.uu.se

‡School of Policy Studies, Queen's University, Kingston, ON, Canada, K7L 3N6. Email: chris.riddell@queensu.ca

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1. Introduction

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

While this small but growing body of evidence has improved our understanding of the effects of performance contracts, they either focus on a very narrow set of individual firms and tasks, or ignore 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 higher ability or more motivated – 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 over 1990-2000 to estimate the effects of pay for performance contracts on earnings. 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 move firms as a result of an establishment closure. We argue that comparing workers who changed contracts following an establishment closure with those that did not is a more exogenous source of variation to exploit. As well, we find several “natural experiments” where there was a compensation regime change in one plant of a given firm but not in other plants. Finally, the institutional framework of this industry is such that we observe specific details on the complexity of all jobs. This allows us to estimate the effects of performance contracts across tasks of different complexity.

2. Background

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 literature consists of either detailed studies of a single firm, studies that use large household or establishment surveys (usually cross-sectional), or the filings from publicly traded companies (typically only the Fortune 500 or similar small subset). The economics literature has tended to focus on cases where individual contracts are observed – mainly piece rates – while the management literature has tended to focus on establishment-level compensation policies such as profit-sharing.² 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 piece rates – increase productivity and earnings relative to contracts that do not

² One notable exception to this is Banker, Young, and Potter (1996). The authors study the effects of a bonus pay system in a retail trade establishment that implemented the policy in 15 of 34 outlets allowing for a natural experiment design.

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. See Prendergast (1999) for an excellent review.

A key problem with much of the performance pay literature is that compensation policy should likely not be viewed as exogenous. Firms that use performance pay are likely systematically different in unobserved ways that are correlated with observable firm (and worker) characteristics as well as outcomes such as productivity and profitability than firms that do not use performance pay. The same concept is likely true for employees as well – more able or more motivated workers may 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) which do not use performance pay can act as a comparison group for those that do (e.g., Seiler 1984; Brown 1992).

Some studies attempt to deal with the selection problem by collecting longitudinal data on workers from a firm or by using large, household longitudinal surveys such as the NLSY, PSID or BHPS, and then difference 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 on a British Columbia tree planting firm (2004), and the Bandiera, Barankay, and Rasul (2005) study of a U.K. fruit farm. The latter groups of studies 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, in Lazear (2000), the switch to piece rates increased productivity by about 40%, only half of which was attributed to an incentive effect. In Shearer's experiment, productivity gains were about 20%.

While recent econometric studies of individual firms have made a major advancement over the earlier performance pay literature, and provide compelling evidence of both the productivity gains from performance pay and of the role of selection bias, there are some drawbacks to them. First, they come from an incredibly narrow set of occupations. Second, each study is based on only a single firm. But, as noted above, just as we anticipate individual unobserved heterogeneity to be problematic 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 the firm's decision to use performance pay revolve around firm-specific factors such as monitoring costs and the composition of the workforce (for instance, skill heterogeneity).³ Abowd, Creedy and Kramarz (2002) show that failing to account for unobserved firm-specific factors can lead to substantial biases in the estimates on individual-level covariates.⁴ 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.⁵

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

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, in the case of Safelite Glass analyzed by Lazear (2000), the timing of their Chapter 11 bankruptcy filing was such that there is reason to believe 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.

⁴ This is also true for firm-level covariates (such as firm size) that are sometimes available in household surveys.

⁵ For instance, in the NLSY, you observe whether the individual indicates they are paid by piece rates, but you do not observe the precise dollar amount received through performance pay nor the percentage of earnings attributable to a performance pay contract. With the personnel records we possess, the precise amount of performance pay income and share of performance pay hours is observed.

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 low 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 covers an eleven year period of an entire industry (metal manufacturing) of an entire country (Finland). Moreover, the institutional framework for this industry-country is such that we have richer information on employee characteristics than available in most personnel records, and very detailed and reliable information on job complexity. On the other hand, the data does not allow us to observe actual productivity, and thus we must test for productivity effects indirectly through examining earnings.

3. Theoretical considerations

The performance pay contracts in the Finnish metal industry for blue-collar workers are pure piece rates or a mixed piece rate/fixed rate contract, and thus we focus on theories relevant for these types of contracts. The institutional details on payment methods and wage determination in this industry are discussed in detail in section 4. 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 rates 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 their output to pay them, but could also be 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 *iff* $q - M > S$ while others choose the salary firm. Firms paying salaries therefore know that they have, on average, less productive employees and pay a salary based on the expected productivity of that subsample of workers. The key testable implication is that earnings should be higher for piece rates workers than fixed rate workers.⁶

Booth and Frank extend Lazear's model to a richer case where a) monitoring costs can differ across firms, and b) worker output is a function of both effort and ability where effort cannot be monitored and ability has two components: an observable part (subject to a monitoring cost), and an unobservable component. Lazear (2000) makes a similar extension in the case of a single firm (and thus ignores monitoring costs). Both of these theories yield the same conclusion that earnings for piece rate workers will be higher than fixed rate workers, but unlike 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 feature of these models is that, on average, effort need not be higher for piece rate workers because 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 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.

4. Institutional details

Finland is one of the Nordic countries, similar in size to Norway and Denmark with a current population of around 5.2 million. It has been part of the EU since its inception in 1995, adopting

⁶ 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.

the common currency in 1999. Similar to other Nordic countries, there are some fundamental institutional differences between the Finnish labor market and the U.S. labor market. In particular, Finland has a unionization rate of around 70%, and about 90% of the labor force is covered by collective agreement. Further, collective bargaining is more centralized than in the U.S. with high-level employer confederations and central union organizations negotiating country-wide income policy agreements, and then sectoral employer organizations and sectoral trade unions negotiating agreements at the sector level. In some cases, the sectoral agreement may allow some conditions of employment to be negotiated at the local (i.e., workplace) level. Generally, the higher level agreements apply only a very general framework on collective bargaining at the sectoral (or local) level. The wage determination process in metals – the industry examined in this paper – is discussed further below. For a thorough review of the Finnish labor market, labor law and industrial relations system see Ministry of Labor (2003).

Conciliation is mandatory and collective agreements tend to be multiple years in length, making strikes/lockout more uncommon in recent years, but based on the 1996-2005 period Finland still ranks 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 ranked fourth in the entire EU at about 14 Euro. However, wage dispersion is among the lowest levels 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 U.S. In particular, at the end of our sample period in 2000, exports account for around 40% of GDP (vs. around

10% in the U.S.), and metals accounted for over half of overall exports. Along with metals, key industries include electronics and the forestry sector.⁷

Wage determination in the Finnish metal industry

The Finnish metal industry is unionized with the general guidelines on wage determination set out in the 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: 1) how long does it take to learn the job; 2) the degree of responsibility in the job; and 3) the 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 from the complexity points to occupation-related wages. The occupation-related wages can therefore be interpreted as a continuous variable that measures the complexity of the tasks. We observe the occupation-related wages, along with final wages, of each individual and their occupational codes (or more appropriately, their job assignment – 165 categories).

The role of the collective agreement is to set minimum standards for wage determination. A worker in this industry knows the minimum wage he is entitled to for his specific job. The

⁷ Another noteworthy feature of Finland's recent economic history is the severe post 1990 recession, where the unemployment rate increased from around 4-6% in the early 1990s to nearly 20% at the peak of the recession 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.

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 performance bonus, piece rates and reward rates. The spirit of the collective agreement is that the payment method should be determined by the characteristics of the tasks performed by the worker.

On fixed rates, workers are paid by the hour; however, fixed rate contracts have provisions for discretionary bonuses of 2%-17% of the occupation-related wage (i.e., 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 at only 10% of total hours worked.

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

5. 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 metal industry is unique in that highly detailed and reliable information on job complexity is available. Each observation in our data contains the accumulated hours worked and earnings within the last quarter of each calendar year.⁸ 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 5.5.⁹ 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 contains very rich information on the nature of the individual's job (discussed further below), as well as years of education beginning in 1996. Thus, individuals who permanently exit the data prior to 1996 have no education information. The

⁸ The data is compiled quarterly but we were only given access to the last quarter for each year.

⁹ A second sample will be used for the 1993 to 2000 period where we have additional information on the plant. If individual plants within firms have autonomy over compensation policy and other practices unobserved to 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 are 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.

results presented in the paper exclude education, but we replicate all of our analysis for the 1996-2000 years including education and the results are virtually unchanged. We also test the sensitivity of the results to the 1992-1995 period – the recession years as noted in section 4 above – and the results are also unaffected.

A unique feature of the data is that we observe the exact share of hours that an individual works on a given contract. This information reveals that, for men, 39% of workers in this industry always worked on a fixed rate schedule (i.e. 100% of hours on a fixed rate contract), about 1% always worked on piece rates, and 10% always 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); some of the variation comes from individuals changing the mix of time spent on a fixed rate vs. performance pay contract in year t to the mix of contracts in year $t+1$. Unfortunately, the payroll records from the Confederation do not separate the part of reward rate pay that was earned from output versus 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 some 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.¹⁰ Overall, firm dummies account for approximately 40% of the variation in performance pay while observed individual

¹⁰ For brevity we omit these results, but they are available upon request.

and task characteristics account for less than 10%. It would seem important, therefore, that any analysis of the effects of performance pay consider firm effects.

6. Empirical analysis

The empirical analysis consists of 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 ‘natural experiments’ where a compensation regime change was made in one plant of a firm, but not in another.

6.1 Linked employer-employee analysis with full data: Econometric issues

We begin by analyzing the impact of performance pay on earnings using the full data. In particular, our regressions of interest have the general form:

$$Y_{i,t} = \beta P_{i,t} + X_{i,t}\gamma + Z_{j,t}\pi + \sum_{j=1}^J \delta_j F_{it}^j + \alpha_i + \varepsilon_{i,t} \quad (1)$$

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_{it}^j 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 we do not have any time invariant covariates in (1). 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 move 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, Creedy and Kramarz (2002) direct least squares estimator (‘DLS’), and an ‘employee-employer-match’ fixed effects estimator (‘EEMFE’). Abowd, Creedy 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 only useful if one is interested in computing the estimates of δ_j and α_i .¹¹ The latter are unnecessary for our purposes; moreover, their estimation has been subject to some scrutiny (e.g., Andrews, Schank, and Upward 2006).¹² Subject to the assumptions listed 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.¹³

¹¹ Implementing the LSDV estimator is not straightforward given data such as ours: it is an unbalanced panel since firms and workers can 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 into 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.

¹² 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 individual effects are ‘backed-out’ after computing 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. The authors argue that this is the reason why virtually all employee-employer studies find a negative relationship between the unobserved individual and firm effects.

¹³ 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

Having linked employee-employer panel data is only relevant if people move 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 moved 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 (i.e., 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. Similarly, movers with identical characteristics to stayers does not necessarily validate the random mobility assumption. Nevertheless, random mobility will be more convincing if the movers have identical characteristics to the stayers. Table 1 reveals that the movers are very similar to the stayers with respect to individual characteristics; the only systematic difference is firm size. Movers are slightly more likely to be part-time workers, more likely to be on a performance pay contract, and have some minor differences in shift work – but all of these are differences on the order of only 1 to 2.5 percentage points. Average real hourly wages, job complexity, age, industry tenure (and education for 1996 onwards) are virtually identical across the two groups.

6.2 Linked employer-employee analysis with full data: Results

Table 2 present the result from (1) estimated by EEMFE as well as a simple OLS regression that does not include controls for unobserved individual or firm-specific heterogeneity. The

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 (i.e., 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 set 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).

results from the earnings regressions are suggestive of both strong incentive and selection effects – identical to the previous literature. For piece rates, the simple OLS model yields an estimate of .15 for men – or a 15% increase in hourly earnings from a change from a fixed rate contract to a 100% piece rate contract. 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 generally 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 of about 1-2 percentage points higher than those found in the male sample.¹⁴

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

It is worth emphasizing that there are some curiosities in the estimates given the industrial relations framework in this industry. 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

¹⁴ For the other estimates, the age and industry tenure estimates are consistent with the voluminous literature 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 individual 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.

subjective evaluation of the individual's performance. Given that our estimates are similar to previous studies where the comparison contract was truly a fixed wage rate, the results may imply that subjective, performance-based compensation plans with a variable component in the 10% of base range 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 U.S. (Lazear) and Canada (Shearer). Going back to the theory, there appears to be (at least) two explanations of 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.

It is also noteworthy that – throughout our results including those discussed below – the findings are so similar across gender. Some recent studies provide evidence that men outperform women in competitive environments – which may apply to a piece rate setting – as well as physical-oriented environments – which would certainly apply for many of our occupations. For instance, Paarsch and Shearer (2007) find large difference in productivity between men and women in Canadian, piece-rate-based tree-planting. Although, the authors attribute this finding entirely to ability (strength in their setting) as opposed to how genders respond to an incentive. Gneezy, Niederle and Rustichini (2003) provide lab-based experimental evidence from Israel that men outperform women in tournament-pay environments, while they find no gender differences in productivity in piece rate and non-competitive environments. Although, the settings in Gneezy et al. (individuals solving mazes by themselves) and Paarsch and Shearer

(tree-planting over large areas) are likely not relevant for a plant-based manufacturing environment. It may 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 and/or cultural background. For instance, the highly egalitarian nature of the Finnish labor market could be such that there is pressure in firms (either from management or among the workforce) for piece rate production to be equalized across genders (or perhaps the amount of hours allocated to a piece rate contract). As well, the Finnish *Act on Equality Between Men and Women* (which came into effect in 1986) specifies very rigid controls on gender equality along a wide variety of conditions of employment. While this legislation implies that there should be equal access to performance pay contracts – not the output produced – 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.

6.3 Heterogeneity in impacts: Job complexity

We also investigate whether the incentive effect depends on the complexity level 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 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-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 are presented in Table 3.

There does appear to be a strong interaction between performance pay incentive effects and job complexity. The estimated performance pay earnings premiums for piece rates for men fall from the 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 as well. It therefore appears that the earnings premium effect of performance pay declines markedly with job complexity. Moreover, it appears that the role of selection into performance pay contracts increases with complexity as well. As noted above, the ratio of the EEMFE estimate to the OLS estimate gives some indication of the selection effect. Examining 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, in the case of piece rates in the first complexity quartile (essentially the same across sexes), the EEMFE estimate is 70% the magnitude of the OLS estimate while this ratio is 30% in the fourth complexity quartile.

The findings above may support the multi-tasking problem with 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 wage effect, as well a greater role for unobservables in the selection of workers into performance pay contracts – because of perhaps the 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 itself. It may be, for example, that the underlying contracts (i.e, the piece rate itself, which is unobserved) in the more complex

positions are such that employers do not give employees in the complex jobs the same incentive as they do for less complex jobs.

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

Some preliminary evidence on the firms' choice of pay scheme also provides some support for multitasking, 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. For piece rates, 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 are used in small, less complex firms. On the other hand, reward rates exhibit a very different pattern; indeed, based on this very preliminary evidence, it appears that the use of reward rates is based on a very different set of establishment characteristics than piece rates.

6.4 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 Lazear and Booth and Frank emphasize effort and ability as the inputs to individual production where ability is constant. Parent (2007) discusses an alternative view

where 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 would be violated.

There is a straightforward way to test the assumptions of the fixed effects approach. In particular, the methods outlined in section 6.1 assume that the average wage change for those individuals who switch from fixed rates to piece rates is the same as the average wage changes for those who change from piece to fixed rate contracts. The data indicates that the latter assumption is likely violated, especially for piece rates. In particular, for piece rate changes, the average wage change for individuals changing from fixed rate contracts to piece rates is about 5 log points for men and 3.5 log points for women while the wage changes for switches out of piece rates into fixed rates are essentially zero for both sexes. The pattern is similar for reward rates, but the fixed rate to reward rate change is about half the magnitude (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, we have several sources of information available to us that allows for some control over the reason for change in contract. First, we know the reason for a job separation, and thus can examine three sub-samples: stayers (workers who stayed in the same firm between time t and $t+1$), displaced workers (workers who moved firms because of an establishment closure between time t and $t+1$), and 'other movers'. For the stayers sub-sample, identification is driven by a change in contracts between time t and

$t+1$ 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 work sample compares individuals whose contract changed following establishment closure with those workers whose contract did not change after moving 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 presents the results. The estimates for the stayers sample and ‘other changer’ samples are similar and consistent with the earlier estimates with piece rate premiums of 9.2% and 11.9% respectively for men (10.4 and 13.9% for women), and reward rate premiums of 4.9% and 6.4% respectively for men (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 where workers likely have less control over the reason for change in their contract; alternatively stated, the change in contracts for this sub-sample is likely the most exogenous. Indeed, exogeneity in the change of contracts may hold on both sides: first, the change in contract from the closed firm is presumably exogenous, and second while the individual still has some control over their contract in the new job it may be considerably less control than in other moves across firms given the mass nature of the layoff. 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 be associated with 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.

6.5 Evidence from a natural experiment

Our final analysis examines compensation policy regime changes where a given 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 8 year period for all firms and plants, and identified five regime changes. Our only sample restriction is that a plant had at least two years in both regimes.

Not surprisingly, when we examine compensation policy by occupation, some job types in the treatment plant (i.e., regime change plant) did not have their pay scheme changed. We focus only on those occupations that did experience a change, and then use the same occupations in the control plant as a comparison group. In almost all cases the occupations in the treatment group affected by the compensation change also exist in the control group. When common occupations does not hold we restrict the sample to those occupations that exist in both the regime change plant and the control plant. Table 5 shows the evolution of compensation policy for these cases where ‘% of performance pay’ is the fraction of employees in the plant 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 (13320 and 16752) had multiple plants that could have been used as the control group; in neither case was there a control plant that was obviously a better control group and so, for brevity, we only present results that pool the control plants. The results are very similar when we separate the control plants. In one case, firm 13572, there are two treatment plants; there are 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 we have a mix of different types of performance pay policy changes; some firms adopted piece rates, one adopted reward rates while others abolished performance pay.

Figures 1-5 show average real hourly earnings for each experiment while Table 6 quantifies the impact of the compensation 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.¹⁵ 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 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 13320.

¹⁵ The regression results from the common trend tests are available upon request.

We begin with the piece rate cases. Firm 13494 is 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 16752 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 than the previous case, being about 4 years older on average and having 2 more years of experience in the industry, as well as being 40% 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 22537 provides an interesting contrast as piece rates were *abolished* in the treatment plant 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 are 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 diminishes almost entirely with the policy impact estimated at -7.4%.

Firm 13572 and 13320 are reward rate regime changes. We emphasize here that there is, unfortunately, no way of knowing the nature of the reward rate contract – it could be a piece rate labeled as a reward rate or it could consist of no piece rate component at all, but contain a team bonus. In Firm 13572, 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 are identical in terms of job complexity but there are some sizable differences along other dimensions: in particular, the treatment plant is much older (and experienced) and its employees work exclusively on single shifts (relative to only 21% of employees in single shifts in the control plant).¹⁶ Nevertheless, there is a common trend in earnings overall the entire pre-regime period. Again, the effect of regime change is striking as illustrated in Figure 4. After performance pay is introduced, real hourly earnings in the treatment plant surpass the earnings levels of the control plant with an overall effect estimated at 10%. The plants in Firm 13320 are the least comparable pre-treatment of all cases. 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.

¹⁶ On the surface, Firm 13572 may seem anomalous given that hourly earnings are higher in the control plant despite this plant being substantially younger and less experienced. Of course, one possibility is the reward rate contract – indeed, the treatment plant catches up to or surpasses the control plant after performance pay was introduced. The other reason for the earnings differential is likely plant size, which has a strong, positive correlation with hourly earnings. In most cases, the control and treatment plants are of similar size; the firm 13572, the control plant is much larger than the two treatment plants combined.

Overall, these natural experiments offer compelling evidence that the various regime shifts – in particular, 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 allows for a fairly rich profile of the nature of the firm (detailed occupations, job complexity, work arrangements), and still the regime changes – each 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 of why wages fall with the elimination of piece rates and increase with the introduction of piece rates.

7. Conclusions

There is considerable interest in whether performance pay increases productivity. Economic theories of piece rates – where pay is entirely a function of output – predict that piece rate workers will earn more than fixed rated workers because of two mechanisms. First, higher ability workers – who have unobserved characteristics such that they would earn more regardless of payment method – select into piece rate contracts (the selection effect), and 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.

This paper 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 is derived from payroll records. These data cover an eleven year period of an entire industry and, in addition to a rich set of individual characteristics including detailed information on job complexity and occupation, 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 allows us to control for both individual and establishment unobserved heterogeneity.

We find that piece rate workers earn 9-10% more than fixed rate workers and reward rate (quasi-piece rate) workers earn 6-7% more than fixed rate workers, with women earning a performance pay premium of one percentage point higher than men for both contracts. These estimates are about 60% 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 natural experiments where 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 are highly similar with respect to observable characteristics pre-regime change. The results from these experiments are consistent with the rest of the analysis, with estimated policy impacts somewhat larger than those obtained in the full sample.

An intriguing conclusion of this paper is that despite a very different institutional and occupational/industrial setting, we estimate earnings premiums of a very similar magnitude to other studies. Two important next steps in this literature are to a) pursue the heterogeneity in performance pay effects (particularly in the context of a more representative set of firms), and b)

to advance the literature on the determinants of performance pay. The latter issues are, of course, likely related. On the second point, it is worth stressing that there has been very little work done (see McLeod and Parent 1999); ultimately given the firms' choice over the parameters of incentive pay (i.e., 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 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, 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 researchers 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 outcomes¹⁷, this type of research design goes a long ways towards making causal inferences.

¹⁷ This is not to suggest that constructing an 'artificial' experiment is better than natural experiments on single firms such as in Lazear or Banker et al.; 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 – not typically possible in a single-firm case study. Furthermore, while not necessarily precluded in single firm case studies, testing for experimental conditions (i.e., 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 – relative to the *typical* cross-sectional or longitudinal HRM study – the methods developed here provide a much more convincing route towards the study of the causal effects of HRM practices.

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Figure 1: Average real hourly earnings in Firm 13494
(Treatment plant adopted piece rates in 1996 - Control plant used fixed rates)



Figure 2: Average real hourly earnings in Firm 16752
(Treatment plant adopted piece rates in 1997 - Control plant used fixed rates)



Figure 3: Average real hourly earnings for Firm 22537
(Treatment plant eliminated piece rates in 1995 - Control plant on fixed rates)



Figure 4: Average real hourly earnings for Firm 13572
(Treatment plant adopted reward rates in 1998 - Control plant used reward rates)



Figure 5: Average real hourly earnings in Firm 13320
(Treatment plant eliminated reward rates in 1997 - Control plant used fixed rates)



Table 1
Summary statistics

Variable	Men		Women	
	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*	9.59 (1.98)	9.72 (2.11)	7.89 (1.36)	7.76 (1.45)
Reward rate hourly wage*	10.38 (4.38)	10.72 (2.80)	8.67 (1.33)	8.69 (1.39)
Piece rate hourly wage*	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 moved firms (at least once). The * indicates that the variable is only defined for a subset of the full sample.

Table 2
Estimated coefficients from hourly earnings regressions

Variable	Men		Women	
	OLS	EEMFE	OLS	EEMFE
Piece rate share	.147*** (.012)	.089*** (.006)	.160*** (.012)	.103*** (.007)
Reward-rate share	.079*** (.009)	.053*** (.007)	.090*** (.009)	.069*** (.005)
Age	.090*** (.007)	-	.035*** (.006)	-
Age squared	-.010*** (.001)	-.016*** (.001)	-.004*** (.001)	-.009*** (.001)
Industry tenure	.042*** (.004)	-	.055*** (.005)	-
Industry tenure squared	-.008*** (.001)	-.004*** (.002)	-.011*** (.002)	-.012*** (.002)
Log of job complexity	.723*** (.031)	.466*** (.042)	.772*** (.033)	.535*** (.061)
Double shift	.004 (.005)	.006*** (.002)	-.014** (.005)	.000 (.004)
Triple shift	-.013* (.007)	.009** (.004)	-.018** (.008)	-.001 (.005)
Part-time dummy	.005 (.005)	.009** (.005)	-.001 (.005)	-.008*** (.003)
Firm size (*100)	.001*** (.000)	-.001 (.001)	.002*** (.000)	.001 (.001)
Constant	.885*** (.110)	2.33*** (.156)	1.00*** (.106)	1.89*** (.057)
R squared	.63	.91	.65	.91

NOTES: 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. Statistical significance is denoted by *** for the 1% level, ** for the 5% level, and * for the 10% level. All regressions include 10 year dummies and 165 occupational dummies.

Table 3
Estimated coefficients from hourly earnings regressions: Estimation by job complexity quartile

Sample	Men				Women			
	OLS		EEMFE		OLS		EEMFE	
	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 (.011)	.097 (.007)	.111 (.010)	.069 (.010)	.160 (.012)	.095 (.009)	.105 (.009)	.072 (.007)
Complexity quartile 2	.150 (.011)	.079 (.009)	.085 (.009)	.048 (.007)	.177 (.027)	.078 (.012)	.100 (.016)	.052 (.012)
Complexity quartile 3	.145 (.015)	.073 (.009)	.083 (.008)	.046 (.008)	.135 (.017)	.068 (.010)	.089 (.023)	.051 (.010)
Complexity quartile 4	.128 (.017)	.065 (.012)	.042 (.012)	.032 (.011)	.137 (.045)	.052 (.013)	.025* (.074)	.044 (.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 * 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**Estimated coefficients from hourly earnings regressions: Estimation by reason for separation**

Sample	Men		Women	
	Piece rate coefficient	Reward rate coefficient	Piece rate coefficient	Reward rate coefficient
Stayers	.092 (.008)	.049 (.010)	.104 (.010)	.066 (.008)
Establishment closures	.074 (.008)	.034 (.009)	.071 (.013)	.037* (.012)
Other movers	.119 (.011)	.064 (.008)	.139 (.014)	.081 (.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 * 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 5
Compensation regime changes within firms

	1993	1994	1995	1996	1997	1998	1999	2000
Firm 13494 – Fixed rate to piece rate change								
Treatment								
% on piece	0	0	0	1	1	-	-	-
Control								
% on piece	0	0	0	0	0	-	-	-
Firm 16752 – 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 22537 – 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 13572 – 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 13320 – 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

Table 6**Estimated coefficients from hourly earnings regressions: Estimation from within-firm compensation regime changes**

Variable	Firm 13494	Firm 16752	Firm 22537	Firm 13572	Firm 13320
	Fixed to piece	Fixed to piece	Piece to fixed	Fixed to reward	Reward to fixed
Treatment plant	-.145*** (.026)	.054 (.037)	.093*** (.008)	-.058*** (.004)	-.083*** (.007)
Treatment period	.038* (.022)	.003 (.033)	.212*** (.007)	.060*** (.003)	.074*** (.003)
Treatment plant * Treatment period	.152*** (.037)	.083** (.040)	-.074*** (.009)	.112*** (.006)	.015* (.008)
Number of observations	138	310	3541	5932	1424

NOTES: All regressions are estimated by OLS. The dependent variable is the log of real hourly earnings. Robust standard errors are in parentheses. Statistical significance is denoted by *** for the 1% level, ** for the 5% level, and * for the 10% level. All regressions include controls for age (and its square), industry tenure (and its square), job complexity, shift-work, part-time work, and firm size.

Appendix 1

a) Distribution of observations across individuals

Number of observations per individual	Men		Women	
	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

a) Piece rates across firm size and job complexity quintiles

Firm size quintiles	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

Firm size quintiles	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	Firm 13494	
	Control plant	Treatment plant
Age	46.46 (1.32)	44.41 (1.42)
Job complexity	34.83 (.498)	34.63 (.688)
Job tenure	17.19 (1.40)	14.50 (1.34)
Female	0	0
Part-time	.017 (.017)	0
Single shift	.931 (.034)	.727* (.097)
	Firm 16752	
	Control plant	Treatment plant
Age	31.67 (.321)	27.63* (.940)
Job complexity	31.02 (.239)	32.29* (.367)
Industry tenure	5.33 (.896)	2.15* (.283)
Female	.422 (.074)	.050* (.035)
Part-time	.022 (.022)	.050 (.035)
Single shift	.956 (.031)	.700* (.073)
	Firm 22537	
	Control plant	Treatment plant
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)
	Firm 13572	
	Control plant	Treatment plant
Age	31.62 (.180)	41.32* (.456)
Job complexity	33.77 (.038)	33.93 (.103)
Industry tenure	5.00 (.012)	10.07* (.310)
Female	.479 (.010)	.503 (.020)
Part-time	.017 (.003)	.009 (.004)
Single shift	.213 (.008)	.997* (.002)
	Firm 13320	
	Control plant	Treatment plant
Age	42.21 (.315)	39.61* (.980)
Job complexity	36.22 (.720)	34.00* (.174)
Industry tenure	17.37 (.274)	15.58* (.942)
Female	.022 (.006)	0
Part-time	.017 (.005)	.024 (.014)
Single shift	.168 (.015)	.200 (.036)

NOTES: Standard errors are in parentheses. A * denotes that the treatment characteristic is statistically different from that of the control.

