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The effects of wage flexibility and the role of institutions: incentive, sorting and rent sharing.

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ABSTRACT

This paper focuses on the effects of a shift in the firm pay strategy from a fixed wage to a flexible pay scheme on firm's performance. Theory predicts that the introduction of wage flexibility might produce both incentive and sorting effects: in presence of heterogeneous workers, the introduction of output-based pay schemes should increase average productivity, reduce quits among the most productive workers and induce the highest ability workers to apply for a job from outside. Extending the model with the introduction of local unions bargaining over the variable pay scheme provides some testable predictions on the relation between union power, workforce composition and the effects of flexible wage schemes. Empirical evidence on a representative sample of Italian metalworking firms shows that both productivity and wages are positively influenced by the introduction of a flexible wage scheme, while no significant effects emerge on aggregate labour turnover rates. On average, the introduction of wage flexibility increases productivity and wages by, respectively, 5-15% and 3-5% in both the short (i.e., one year since introduction) and medium run (i.e., three years since introduction). These effects vary with both business conditions and union power. Productivity gains are in fact more relevant in low unionized firms, while the effect on wages is less clear cut, probably because highly unionized firms can anyway bargain higher (fixed) wages at the local level.

Key-words: wage flexibility, firm performance, local unions, treatment evaluation
JEL Codes: J33, J38, J51

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Abstract

This paper focuses on the effects of a shift in the firm pay strategy from a fixed wage to a flexible pay scheme on firm's performance. Theory predicts that the introduction of wage flexibility might produce both incentive and sorting effects: in presence of heterogeneous workers, the introduction of output-based pay schemes should increase average productivity, reduce quits among the most productive workers and induce the highest ability workers to apply for a job from outside. Extending the model with the introduction of local unions bargaining over the variable pay scheme provides some testable predictions on the relation between union power, workforce composition and the effects of flexible wage schemes. Empirical evidence on a representative sample of Italian metalworking firms shows that both productivity and wages are positively influenced by the introduction of a flexible wage scheme, while no significant effects emerge on aggregate labour turnover rates. On average, the introduction of wage flexibility increases productivity and wages by, respectively, 5-15% and 3-5% in both the short (i.e., one year since introduction) and medium run (i.e., three years since introduction). These effects vary with both business conditions and union power. Productivity gains are in fact more relevant in low unionized firms, while the effect on wages is less clear cut, probably because highly unionized firms can anyway bargain higher (fixed) wages at the local level. These results clearly suggest that the public support of performance-related-pay schemes may be offset by adverse business conditions or by quite strong local unions.

Key-words: wage flexibility, firm performance, local unions, treatment evaluation

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1. Introduction

Wage flexibility is a central topic in economics, mainly in literature related to the provision of incentives at the firm level. A number of theoretical models have been developed to explain how firms should design compensation schemes to induce workers to operate in firm's interest (i.e., to put an amount of effort in their job such that to maximize firm's profits)[1]. Performance-related schemes (such as piece rate, productivity-related premiums, profit and gain sharing, etc.) are often used to make workers more productive and labour cost more sensitive to business conditions.

In Italy this issue is particularly crucial, also in light of recent institutional changes emphasizing the role of flexible wage schemes both at the macro and micro level: making wages more sensitive to individual and collective performance should help to keep inflation under control (in light of the strict Maastricht targets), at the same time allowing more competitive/profitable firms to share eventual extra-rents with their workers (Blinder, 1990).

Wage flexibility becomes a strategic tool to react to changing business conditions mainly when, as in the Italian case, it is relatively difficult to adjust employment levels (at least in larger and unionized firms) due to the presence of quite rigid employment protection regulation.

More recent literature has pointed out that wage flexibility might produce further effects other than making employees more productive. More specifically, in presence of heterogeneous workers, the introduction of output-based pay schemes should produce also relevant sorting effects, reducing quits among the most productive workers and inducing the higher ability workers to apply for a job from outside (Lazear, 2000).

Despite of the sophistication reached by economic theory in this field, empirical analysis is still at an initial stage, mainly due to the lack of suitable data to test theoretical predictions.

Following the seminal work by Lazear (1996 and 2000), the aim of this research is to provide some further empirical evidence on the effects of wage flexibility on firm performance on the basis of a unique and rich panel data set of Italian metalworking firms.

This work differ from Lazear's mainly for two aspects: first, empirical analysis is based on a relatively large sample of firms (rather than a single company), thus making the results more easily extendable to other samples/sectors; second, given the role played by labour market institutions (such as unions) in the Italian context, I study to what extent the presence of institutions interact with the introduction of wage flexibility in affecting outcome variables.

In light of these considerations, we start considering the main features of the Italian institutional setting and the most recent changes in this perspective (section 2). In section 3 we discuss and extend the basic theoretical model, taking into account how the original testable predictions can change if we consider the role of unions in local bargaining.

[1] For a recent survey, see Prendergast, 1999.

Section 4 presents the main results of existing empirical literature, while econometric issues are discussed in section 5, paying attention to the main features of different methods available to evaluate the effect of a certain treatment (in our case, the introduction of wage flexibility) on a set of outcome variables. Section 6 presents the data and the sample used in the empirical analysis. Some preliminary empirical evidence is reported in section 7, while section 8 is devoted to the discussion of the main econometric results. Concluding remarks and a further interpretation of the results in a policy perspective are discussed in the last section.

2. The institutional context

In 1993 the Italian government, national trade unions and employer associations signed an income policy agreement aimed at containing the inflation rate (in light of the EU Maastricht targets) through a two-stage bargaining system: national bargaining (by economic sector) and local contracts. The first had to preserve the purchasing power of wages and salaries (incorporating expected inflation rate in wage increases and eventually adjusting wages in a subsequent bargaining round if the actual inflation rate was higher than expected), while local bargaining (either at the regional or firm level) had to allow eventual rent sharing through performance-related pay schemes rather than fixed (usually irreversible) premiums. The 1993 Agreement stressed the need to make wages more flexible in order to avoid the wages-prices spiral that characterized the Italian economy in the Eighties (when inflation rates reached the double digits) and to prevent further unemployment increases by allowing negative macroeconomic shocks to be partially absorbed through wages adjustments.

The Italian metalworking sector stressed further the role of flexible pay schemes with the 1994 industry contract, which explicitly referred to the 1993 Agreement recommendations in terms of flexible wage increases at the local level.

In light of the uncertainty of the situation between 1993 and 1994, metalworking firms preferred not to sign new local contracts (waiting for the recommendations of the national contract). Local bargaining started again in 1995-97 (Rossi, 1997; Monitorlavoro, 1998), when most of the medium-large metalworking firms adopted some forms of flexible compensation schemes (the so called "*premio di risultato*")[2].

According to the results of an ad-hoc survey carried out by the national statistics office on a representative sample of around 8000 firms with at least 10 employees both in the manufacturing and service sectors, in 1995-96 firm-level bargaining involved around 10% of the surveyed firms and 40% of total workers. The majority of the workers covered by company-level collective bargaining was employed in manufacturing firms (73.4%), rather than in services (26.6%). Wage issues were at the first place among bargaining topics in most of the agreements signed. Variable pay schemes were bargained in 40% of total firms, involving almost 60% of total employment. As far as the metalworking sector is concerned, local bargaining took

[2] Previous studies have already pointed out how the 1993 Agreement produced significant changes in both the types of wage premiums and the role of union organizations within the bargaining process at the local level (Origo, 2000).

place in one out of five metalworking firms, employing around 55% of total workers in that sector. Almost all of them bargained over wages and one out of three introduced some form of performance-related pay schemes.

With reference to performance indicators, the 1993 tripartite Agreement emphasizes the need to strictly link the variable pay scheme to specific projects, agreed between the bargaining parties, aimed at increasing productivity, quality and other elements of firm competitiveness, as well as to indicators of the overall firm economic performance. According to the Istat survey and direct text analysis of firm contracts[3], on average the actual amount of the premium depends on the trend of at most three parameters, measuring in some way both firm's productivity and profitability. Financial indicators alone are used by 20% of the firms, but the most common single indicator is still productivity (25% of the firms in the Istat survey). Furthermore, it has become more common to use quality indicators, measuring either internal (reduction of production failures, correspondence to official quality standards, etc) or external quality (customer satisfaction).

Usually, the variable pay scheme substitutes traditional fixed wage premiums and it may only add to minimum wage levels set by collective agreements. In this sense, wage flexibility is only upward, but the wage premium might be zero if performance targets are not met[4]. The amount of the premium is usually the same for all the workers involved. When it differs, the amount paid is proportional to either the wage associated to each occupational level[5] or to an indicator of individual absenteeism.

3. The model

In order to describe how the introduction of wage flexibility can affect firm performance, let's consider a principal-agent model with heterogeneous workers in which the firm wants to switch from a fixed wage to an output-based pay scheme.

In this context, workers' preferences are function of both the wage level (W) and individual effort (X):

$$U = U(W, X) \quad with\, U'_W > 0, U'_X < 0 \qquad [1]$$

The output level *e* produced by each worker can be considered as a function of her effort (X) and her ability (A):

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[3] For an extensive review of these studies, see Del Boca and Origo (2003).

[4] Istat survey shows that failure in meeting the performance targets usually implies a proportional reduction of total payment (44.6% of total workers). The premium can actually be zero for 42.6% of the workers involved. A minimum fixed payment is anyway guaranteed for the remaining 12.8%.

[5] Metalworking workers are classified into two categories (blue and white collar workers) and eight occupational levels (the so called "*livelli di inquadramento*") broadly defined in the national labor contract of the metalworking sector. The basic pay ("*minimo tabellare*") is parameterized on these levels. The same kind of normalization is sometimes used to determine the actual amount of the variable premium.

$$e = f(X, A) \quad with\ f_X^{'} > 0, f_A^{'} > 0 \qquad [2]$$

e is observable by the firm, while X and A are not. Furthermore, the level of effort X is negatively related with the level of ability A: higher (lower) ability workers have then to exert less (more) effort to produce a given level of output[6].

Let us also assume that, for any given pair of output and wage (e_o, W), we can define the utility of the minimum ability worker willing to accept the job (instead of consuming leisure) as:

$$U = U(W, X) = U(W, X(A_0)) = U(0,0) \qquad [3]$$

where the last term measures the utility associated with leisure.

It follows that all workers with ability level greater than A_0 will earn rents (or extra-utility) from working (with respect to the minimum ability worker whose ability is A_0) if they have to produce e_0, since they can produce it with less effort than the least able worker. Furthermore, if alternative jobs (firms) are available, those willing to work at a certain firm must not have work alternatives with higher rents. Defining as R(A) the rent associated with an alternative job (firm), the highest ability worker will not leave the current firm as long as her extra-utility equals the rent she could earn elsewhere:

$$U(W, X(A_h)) = R(A_h) \qquad [4]$$

If [3] and [4] hold, then those working at the current firm will have ability greater than A_0 but lower than A_h.

To both induce the least able individuals to work and the most able ones to stay, profit maximization is constrained by the above two conditions.

Firm profits depend on total output, which is the aggregate result of the level of effort exerted by each employed worker[7].

In this framework, it can be shown that a profit maximizing firm faces a trade-off in choosing the optimal level of a fixed wage W coupled with a minimum output requirement e_0. In theory, there is not an optimal choice of (e_0, W) for any specific type of worker. Regardless their ability level, all the workers will produce e_0 earning W but, in terms of disutility from the effort required, e_0 is too high for the A_0 type workers and too low for the A_h type ones. Given a certain level of output, the firm wants to choose

[6] This can be easily proved just applying the implicit function theorem to equation [2].

[7] Assuming that workers are distributed according to a probability function g(A), firm's profits can be expressed as: $\Pi = \int_{A_0}^{A_h} (e - W) g(A) dA$

W as low as possible, but this will affect the number (and the level of ability) of the workers willing to work at this firm. On the other hand, given a certain level of W, the firm wants to set the level of the output standard as high as possible, but the higher the output standard, the lower is the number of workers willing to accept the job (Lazear, 1996).

One way to overcome this problem is to switch to a performance related pay scheme.

As a specific case of flexible wage rate, let's consider a linear piece rate scheme, according to which workers are paid in proportion of the amount of output they produce, net of a minimum level of production that has to be guaranteed: W(e) = be-K, where b is a fixed parameter and K is the implicit charge for the job. Instead of assuming a complete switch from a fixed to a flexible wage, it is more realistic that the firm will continue to pay a fixed wage (coupled with the minimum effort level) to those who would earn less than that under the piece rate:

$$W = \max\left[\overline{W}, be - K\right] \qquad [5]$$

Under the flexible wage scheme, assuming that workers' preferences can be represented by the following specific utility function:

$$U(W, e) = W - \frac{C(e_i)}{A_i} \qquad [6]$$

workers' maximization problem becomes:

$$\underset{e_i}{Max}\left[be_i - K - \frac{C(e_i)}{A_i}\right] \qquad i = 0,...h \qquad [7]$$

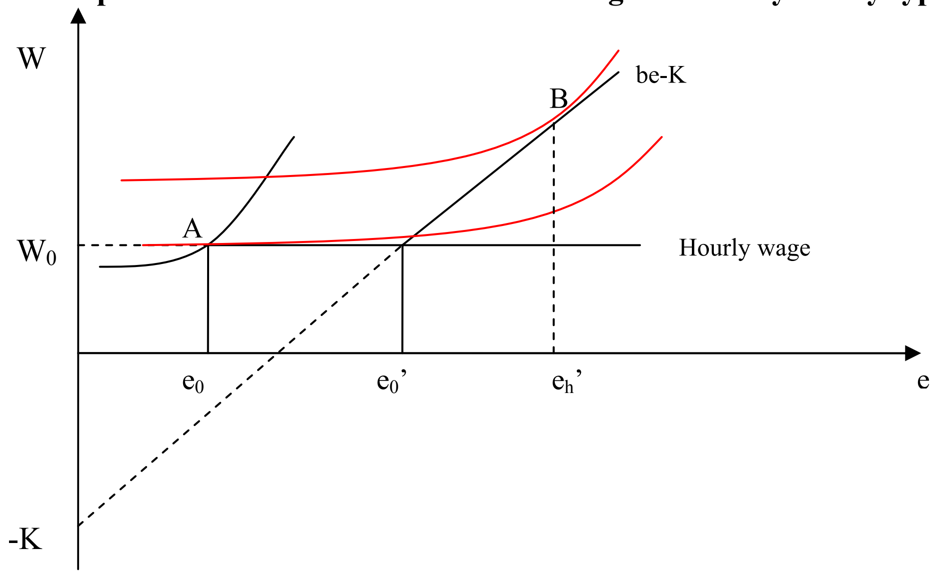
whose First Order Conditions lead to:

$$b = \frac{C'(e_i *)}{A_i} \qquad i = 0,...h \qquad [8]$$

The firm will extract the highest effort by each ability type by setting b equal to 1.

Under the new compensation scheme, for some low ability workers it will be still convenient to produce e_0 and get the fixed W (point A in figure 1). However, higher ability workers might find more convenient to be paid according to their performance (point B in figure 1)[8].

Figure 1 – Optimal effort choice under flexible wage scheme by ability type



This result leads to the following testable predictions[9]:

Proposition 1 (incentive effect): Under the flexible wage scheme average productivity is higher than under the fixed wage scheme as long as some workers decide to produce more than e_0 under the flexible wage scheme.

Proposition 2 (sorting effect): Under the flexible wage scheme average ability is higher than under the fixed wage scheme as long as some workers will be attracted from outside by the new pay policy.

From proposition 1 and 2 and from the fact that wages are directly linked to individual productivity under the flexible wage scheme, it also follows that average wages will increase with the new wage policy if either average productivity or average ability is higher.

Proposition 3 (inequality effect): Under the flexible wage scheme variance of worker ability and productivity within the firm is higher than under the fixed wage scheme as long as some workers will accept the fixed wage and some workers will choose to work enough to be paid according to their performance.

[8] The marginal rate of substitution between wage and output is in fact lower for high ability workers (since they are characterized by a relatively low cost of production) and hence their indifference curve are flatter.

[9] For a sketch of the proof, see Lazear (2000).

3.1. An extension with institutions

The model discussed above relies on the assumption that the management can freely choose how and when to eventually change the compensation scheme. This assumption can be a good description of what happens in a low unionized labour market as the American one (an it perfectly fits the case study reported in Lazear, 2000). However, in contexts where unions are more powerful (as it is in some EU Countries, including Italy; see Corneo and Lucifora, 1997; Checchi and Lucifora; 2002), it is more realistic to assume that any substantial change in employment or wage conditions is bargained between the firm and some local union representatives.

Let's consider the flexible wage scheme described in [5] and assume that the firm can still freely choose b, but it has to first bargain over K (i.e., the minimum level of effort – or the implicit charge of the job) with a local union.

Union cares of the utility of the median worker and union's objective function can then be written as:

$$U(W_m, e_m) = be_m - K - \frac{C(e_m)}{A_m} \qquad [9]$$

where e_m is the output produced by the median ("m") ability worker.

We can represent the bargaining process as a traditional Nash problem:

$$\underset{K}{Max}\left[U - \overline{U}\right]^{\gamma}\left[\Pi - \overline{\Pi}\right]^{1-\gamma} \qquad [10]$$

where $\overline{U}$ and $\overline{\Pi}$ represent the fallbacks of, respectively, the union and the management. For simplicity, if bargaining fails, workers' utility will equal the fixed wage (eventually paid by another firm), while firm's profit will be zero.

Regardless of the value of K bargained, since the latter does not affect the marginal choice of effort, the firm will always fix b equal to 1.

Taking into account the assumptions discussed above and plugging b*=1, the Nash bargaining problem reduces to[10]:

$$\underset{K}{Max}\left[e_m - K - \frac{C(e_m)}{A_m} - \frac{C(e_0)}{A_0}\right]^{\gamma}\left[\int_{A_0}^{A_h} e - (e - K)\right]^{1-\gamma} \qquad [11]$$

whose First Order Condition leads to:

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[10] Under the fixed wage scheme, the optimal wage level is that inducing the least able individual to work and to produce e_0. Given the utility function in [6], this leads to $W^*(\text{fixed})=C(e_0)/A_0$

$$K^* = (1-\gamma)\left[e_m - \frac{C(e_m)}{A_m} - \frac{C(e_0)}{A_0} \right] \qquad [12]$$

This result allows to obtain the following testable predictions:

Proposition 4: K* (and hence the incidence of flexible wage schemes) is lower (higher) when the union power is higher (lower)

Proof: From the optimal condition [12]: $\frac{\partial K^*}{\partial \gamma} = -\left[e_m - \frac{C(e_m)}{A_m} - \frac{C(e_0)}{A_0} \right]$<0 (since the term in brackets is positive when the median employee decides to work under the flexible wage scheme instead of accepting the fixed wage).

Proposition 5: K* increases with the ability level of the median worker.

Proof: From the optimal condition [12]: $\frac{\partial \mathrm{K}^*}{\partial \mathrm{A}_\mathrm{m}} = (1-\gamma)\frac{C(e_m)}{A_m^2} > 0$ (since both terms are positive).

According to the last two propositions, unions tend to opposite the introduction of wage flexibility but their actual resistance (and hence the subsequent impact of wage flexibility described in propositions 1-3) depends on both union power and the composition of the workforce. The effects of wage flexibility should then be lower in highly unionized firms with low skilled workers, while a flexible wage scheme should be more effective in low unionized firms with a high skilled workforce.

4. Previous empirical results

Most empirical research on incentives concentrated on the effects of payment schemes on both productivity and wages (Ashenfelter and Pencavel, 1976; Goldin, 1986; Brown, 1992). As far as performance-related pay schemes are concerned, most of the empirical evidence found a positive relation between them and the outcome variables considered (Weitzman and Kruse, 1990; Fernie and Metcalf, 1996; Paarsch et al., 1996). In the case of Italy, an increasing number of contributions has been studying both the factors determining the adoption of flexible wage schemes and the effects of the latter on productivity, profitability and earnings (for an extensive review, see Biagioli and Del Boca, 1999; Del Boca et al., 1999; Del Boca and Origo, 2003), usually confirming the positive relation found in other countries.

Most of these studies are based on cross-section data (thus focusing on the use of wage flexibility rather than a switch in the pay policy) and they limit their attention to incentive effects, neglecting both sorting and inequality effects.

A full empirical test of the discussed theoretical model without output variability and institutions is carried out by Lazear (1996 and 2000) on very detailed data on a large US auto-glass company. In 1994-95, following a significant management change, the company progressively revised its wage policy, shifting from hourly fixed wages to piece-rate pay. Monthly data on more than 3000 employees observed over a 19 months period (both prior and after the wage scheme switch) shows that the introduction of the flexible wage scheme made output per worker to increase by 44%, mostly due to incentive effects (22%). Sorting seems relevant mainly through the hiring process, since most of the new workers come from the highest ability groups. Positive effects are also registered in terms of wages, which on average increased by 10% after the pay switch. Almost all the observed workers (more than 90%) actually experienced a pay increase in the period considered.

This piece of evidence is fully consistent with the model discussed above. However, the results are referred to a unique company and hence their extension to the population might be questionable. Furthermore, since in that company the pay switch followed a relevant management change, it is quite difficult to determine whether the observed improvements in the firm performance are only due to the changes in the pay policy or, rather, they were also determined by the new overall firm strategies and management.

We shall then try to provide further test to the model by using a larger panel of Italian firms, paying particular attention to the possible effects of a quite different institutional context.

5. Econometric issues

The aim of the empirical analysis is to evaluate the effect of the introduction of wage flexibility on some outcome variables related to firm performance. This is a traditional problem of treatment evaluation without random experiments, since firms can freely choose whether and when to introduce flexible wage schemes (i.e., the treatment).

Let's consider a population of N firms and suppose that each i-th firm can take part to a certain treatment D. D_i is equal to 1 if the i-th firm receives the treatment, 0 otherwise. Let Y be the outcome variable on which we want to evaluate the effect of the treatment D. If we are interested in evaluating the Average effect of the Treatment on the Treated (ATT) as follows:

$$ATT = E\{\Delta_i \mid D_i = 1\} = E\{Y_i(1) - Y_i(0) \mid D_i = 1\} \qquad [13]$$

only $Y_i(1) \mid D_i = 1$ is observed, while $Y_i(0) \mid D_i = 1$ is by definition not identified by the observed data. We have then to estimate the counterfactual situation for each treated firm (i.e., what would have been the observed outcome for the same firms if they had not been treated).

There are a number of methods to estimate the counterfactuals. Regardless of the method used, if the treated firms are not randomly drawn from the initial population, to correctly evaluate the net impact of the treatment on the outcome of interest we need to observe the (treated) firms somewhen before the treatment and to measure the outcome variable(s) somewhen since the treatment. For instance, if the i-th firm is treated at t, we need to know the main characteristics of that firm (including the pre-treatment value of the outcome) at least at $t-\tau$ and to observe the value of the outcome at least at $t+\sigma$.

The choice of the pre-treatment period τ should guarantee that outcome variables were not affected by any (anticipated) effect of the treatment. Whenever it is reasonable to assume that the treatment could not produce any effects before it's actual implementation, it is intuitively convenient to choose τ as close as possible to t, because the social and economic environment is more similar.

The choice of the post-treatment period σ depends on both the type of treatment and the type of effects we are interested in evaluating. If we believe that the treatment should immediately affect the outcome variables or we are interested only in its effects in the short run, then σ should be pretty close to t. On the contrary, if it is plausible that it takes some time before the treatment becomes effective or we want to capture long run effects, than σ should be chosen further in the future.

In practice, the choice of both τ and σ is often heavily influenced by the features of the data available. In most cases, because of sample size consideration, both τ and σ are in fact set equal to 1. The availability of good (longer) panel data (or, with some further assumptions, repeated cross sections) can provide more degrees of freedom in the choice of the pre-treatment and the post-treatment periods.

5.1. Choice of the comparison group: identification and estimation methods

In presence of non-experimental data, the missing data problem discussed above is not the only issue that should concern the evaluator. When the potential beneficiaries are not randomly assigned to either the treatment or the control group, firms can self-select into the treatment according to either their observable or unobservable characteristics.

Estimation methods with non-experimental data have then to take into account also the problem of self-selection (Heckman, 1979) and their appropriateness depends on three main factors: the type and the quality of data available, the underlying model and the parameter of interest. In general, panel or repeated cross-section data require less stringent assumptions due to the relative richness of information: there is then a clear trade-off between the available information and the restrictions needed to obtain a reliable estimator (Blundell and Costa Dias, 2002).

In the empirical literature on program evaluation three estimators have been traditionally used: Mean Comparison (MC, also known as cross-section estimator), Before-After (BA) estimator and Difference-in Differences (DD) estimator. As pointed out by Heckman et al (1999), these three estimators represent the trilogy of conventional non-experimental evaluation methods and they differ for the type of assumptions on which they rely to identify the control group[11] and the type of data required.

[11] The MC estimator relies on the assumption that on average firms that are not treated have the same no treatment outcome as those that are actually treated; the BA assumes instead that the eventual change in

All these estimators can be calculated also conditional on a vector X of observable characteristics. Conditioning on X or additional instrumental variables should in fact make it more likely that the relevant identifying assumptions will be met. However, there might be some cases in which conditioning might worsen the evaluation bias. The latter can be decomposed into three main components: the bias due to differing supports of X for the treated and the untreated, the bias due to different distribution of X in the two groups and the bias due to unobserved heterogeneity. If, for example, the distribution of X is different between the treated and the controls, conditioning on X may compress systematic differences in outcomes between the two groups. On the other hand, if the bias is mainly due to differences in the unobservables, conditioning may accentuate differences between the two groups (Heckman et al, 1997).

Non parametric methods, in particular matching estimators, help to partly solve some of these problems. The baseline of this family of estimators is to re-establish "at desk" the conditions of a randomized experiment even if no randomized control group is initially available. Matching estimators aim to construct the correct counterfactual sample by pairing each participant with members of the non-treated group.

The most common matching estimator is the propensity score matching estimator proposed by Rosenbaum and Rubin (1983), who showed that, conditional on the propensity score P(X), the treatment and the observables are independent (balancing property):

$$D \perp X \mid P(X) \qquad [14]$$

Identification relies on the so called Conditional Independence Assumption or Unconfoundness: conditional on the set of observable X (or P(X)), the non-treatment outcome is unrelated to the treatment:

$$Y(1), Y(0) \perp D \mid P(X) \qquad [15]$$

The treated and the untreated can then be matched on the basis of their propensity score P(X). Since the estimated P(X) is a continuous variable and matching on exactly the same value of P(X) is practically unfeasible, several matching procedures have been developed in literature and they differ in terms of the system of weights adopted for choosing the potential controls (such as assigning a unity weight to the nearest untreated observations and zero to all the others; equal weight to all the controls within a certain radius from the propensity score of the treated; kernel weights; etc.)[12].

The proper use of Propensity Score Matching estimators allows then to reduce (not to eliminate) the evaluation bias. By definition this method, essentially based on the observables, can't deal with the bias due to selection on unobservables. However, the

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the outcome measured before and after the treatment is completely due to the treatment itself; finally, the DD estimators exploits the assumption that the average change in the no-treatment outcome should be the same for the treated and the untreated.

[12] For a discussion on the different matching procedures, see Becker and Ichino (2002).

availability of a rich data set should make this type of bias less relevant. More in general, the reliability of the results obtained with any matching estimators heavily depends on the quality of the available information.

Detailed information on the pre-treatment characteristics of both the treated and the untreated is actually crucial to make the CIA assumption convincing. Ideally, you would like to match observations that are identical in terms of time invariant characteristics and all the past values of time variant variables. Despite of the quality of the information, the CIA is a quite strong assumption if the individuals decide also in the basis of their forecast outcome (Blundell and Costa Dias, 2002).

By combining matching with Difference-in-Differences methods, it is possible to generalize and make less restrictive the CIA assumption (Heckman et al., 1997). The basic idea is that, even if the CIA does not hold, it may reasonable to assume that the evaluation bias is constant over time (or at least it is the same for one date before the treatment and on date after it). More intuitively, if the true impact of the treatment is really zero before the treatment takes place, but we find some effects estimating a propensity score matching in period τ before the treatment, then we can consider it as an estimate of the bias. This could be used to correct the estimate of the treatment effect in the post-treatment period.

With longitudinal data, the ATT with Difference-in-Differences and propensity score matching becomes:

$$ATT_{DD} = \sum_{i \in T} \left\{ (Y_{i,t+\sigma} - Y_{i,t-\tau}) - \sum_{j \in C} W_{ij} (Y_{j,t+\sigma} - Y_{j,t-\tau}) \right\} w_i =$$

$$\sum_{i \in T} \left\{ (Y_{i,t+\sigma} - \sum_{j \in C} W_{ij} Y_{j,t+\sigma} \right\} w_i - \sum_{i \in T} \left\{ (Y_{i,t-\tau} - \sum_{j \in C} W_{ij} Y_{j,t+\tau} \right\} w_i = \qquad [16]$$

$$ATT_{PSM,t+\sigma} - ATT_{PSM,t-\tau}$$

where T and C denotes, respectively, the Treatment and the Control group, W_{ij} is the weight assigned to the j-th untreated observation when matched with the i-th treated one and w_i is a system of weights that takes into account the distribution of the treated sample.

This generalized version of the CIA assumption is in literature known as the conditional Bias Stability Assumption (BSA). The main advantage of using this identifying assumption is that it implicitly contains the CIA assumption and it allows to test its validity (Eichler and Lechner, 2001). Furthermore, assuming the BSA requires less information for ATT identification than under the CIA. The main drawback is shared with the traditional DD estimator and it consists of the choice of the triplet (τ, σ and X): neither economic theory nor econometric tests can guide this choice and the results heavily depend on it.

In order to test the theoretical predictions found in section 2 taking into account the econometric issues discussed above, the empirical analysis will be carried out using both traditional evaluation estimators and the more recent matching estimators.

6. The data

The empirical analysis is based on a representative sample of metalworking Italian firms. The data is derived from the annual survey carried out by the national employer association of the metalworking sector (Federmeccanica), mainly for wage bargaining purposes. For this research, waves from 1989 to 1997 were available. On average around 3240 establishments employing 460 thousands employees are surveyed each year covering, respectively, 25% of the firms and 50% of the workers in this sector. Even if the survey was not created with a longitudinal design[13], over the period considered almost 90% of the establishments were surveyed at least twice and around 400 establishments (around 1 out of 10 of each cross section) are present throughout all the period considered.

The sample is representative of the composition of the metalworking sector in Italy, with the partial exception of small and Southern firms[14].

The questionnaire is made of two main sections: one related to the whole firm, the other to each single establishment within the firm. The first section asks questions about the main firm's features (sector, employment level, sales, outsourcing and exports), unions activity (union density, union organizations and strikes), firm-level bargaining (actors and contents), wage levels and composition, medium run growth prospects (new establishments that will be either opened or closed in the next three years), immigrant workers. The second part of the questionnaire asks, to each establishment, questions related to employment composition (by sex, type of contract and qualification), employment flows (hires by type of contract and separations by reason), shifts, working time (including overtime hours, temporary lay-offs and absenteeism), on the job injuries and investments.

In light of the aims of the empirical analysis, it is important to discuss further the unit of analysis and how the treatment was measured.

Even if information is available at the establishment level, the introduction of flexible compensation schemes usually happens at the firm level. For this reason, data was aggregated by firm and year. The sample was then composed by 25509 observations from 1989 to 1997.

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[13] A sort of panel was introduce in the Eighties (the survey started in 1976), mainly to keep under control the quality and variability of wage time series. However, only one establishment out of four belongs to the "official" panel, while many more are observed more than once in the time span considered.

[14] The comparison with the relevant administrative data of the National Social Security Agency (INPS data) shows that, over the period considered, firms under 10 employees constituted around 50% of the total population (against 11% in the Federmeccanica sample). This is actually the only size group with a relevant different incidence in the two data-sets. The same comparison shows that firms located in the Centre-South are around 30% in the administrative data, 11% in the Federmeccanica sample. The two sources present the same share of firms located in the North-Eastern regions (25%), while those located in the North-West are over-represented in the Federmeccanica sample (64%, against 43% in INPS archives).

In each wave firms can be divided into three main groups:

1. firms without a firm contract
2. firms signing a firm contract but without wage flexibility
3. firms signing a firm contract introducing wage flexibility

Firms belonging to the first two groups are untreated, while those in the last group are treated. We excluded all the firms declaring to adopt a firm contract but not providing details about their wage policy. Once deleting them, we ended out with a total sample of 16444 observations over the 1989-97 period.

It is also important to point out that, due to the institutional changes occurred in the first half of the Nineties discussed in section 2, the definition of wage flexibility and the corresponding questions were changed in the survey since 1995.

Before that year there were neither incentives nor clear public policy guidelines regarding the introduction of flexible wage schemes, which was essentially an individual choice probably based on profit maximization considerations or the results of some costs-benefits analysis. In the surveys from 1989 to 1994, it was asked to the bargaining firms whether wages were among the bargained objects and whether they introduced variable premiums, either individual or collective (such as productivity-related or profit sharing schemes). I considered as "treated" all those firms that answered positively to at least one of the questions related to the introduction of flexible wage schemes.

Since 1995, following the principles of the 1993 Agreement and the guidelines of the new national contract for the metalworking sector signed in July 1994, the questions related to flexible schemes were re-formulated using the same terminology proposed by the national contract (which introduced the definition of a performance-related pay scheme, "*premio di risultato*"), without no longer distinguishing between individual and collective premiums. I classified as "treated" all the bargaining firms that declared to have followed the national contract by introducing any type of performance-related wage premium.

6.1. Defining the outcome variables

The theoretical model presented in section 3 provides some testable predictions in terms of causality effects generated by the introduction of wage flexibility on productivity level and variance, workers sorting and wages.

In this section we discuss how those outcome variables can actually be measured with the data available.

Incentive effects and productivity levels

In absence of a good measure of gross output or value-added (and of the necessary information to calculate them), we use the value of real sales per worker as a proxy of labour productivity. As long as the firm specific ratio of sales to gross output (or value-added) is constant, the value of sales is naturally a good proxy of labour productivity. Even if this condition doesn't hold, it can be shown that sales are approximately equal to gross output in dynamics terms (i.e., if lagged sales are included among the regressors; Nickell et. al, 1991). In this case, the value of sales is preferred to a bad measure of value-added, mainly if the latter is calculated making further assumptions on pre-tax profits or interest payments.

Sorting effects and workers turnover

Since the data set doesn't provide individual turnover rates, we could capture eventual sorting effects only through aggregate turnover rates or turnover rates by skill (blue/white collars). The use of an overall turnover rate doesn't seem a good solution, since the effect of wage flexibility on it is ambiguous. According to the theoretical model, the introduction of a performance-related pay scheme should in fact attract the most productive workers from outside and disincentive the ablest incumbent workers to leave, thus producing opposite effects on overall labour turnover.

However, since risk aversion, average ability and the sensitivity to incentives are correlated with skill levels (Prendergast, 1999), sorting effects can probably be partly captured by looking at turnover rates of blue and white collar workers separately.

Rent sharing

The theoretical model predicts that the introduction of flexible wage schemes, mainly if accompanied by a minimum (fixed) wage level, should increase average wages. This effect should be even more relevant if wage flexibility increases labour productivity and firms share part of these extra-rents with their workers through higher wages.

We then use average annual firm's real wage levels to capture this effect. Wages also include annual or *una tantum* bonuses, both at the individual and collective levels.

Output variance

The data set contains information on sales only at the firm level. In absence of any measures of output dispersion within the firms and assuming that wages are correlated with productivity, we use wage dispersion (measured by the coefficient of variation) as a proxy of output variance. Wages may be a useful indicator for firm productivity even if the firm doesn't pay their workers according to their marginal productivity (Winter-Ebner and Zwimuller, 1999). For instance, in a situation in which firms and workers split the firm's rents in a bargaining framework or according to a sharing rule, whenever the pie becomes larger due to higher productivity, workers should be paid better even if they do not earn exactly the marginal product.

6.2. The eligibility issue

In order to choose the proper control group among the untreated firms, it is important to verify whether the choice of introducing flexible wage schemes is subject to any kind of eligible rule. The choice of the comparison group is particularly relevant, since it affects the property of any evaluator estimators eventually used (Heckman et al., 1999).

In principle, all the firms can introduce some forms of wage flexibility, either as a top-down decision (as in Lazear's study on the autoglass company, 1996 and 1999) or as the result of bargaining with local unions.

In the Italian context, mainly in the metalworking sector, wage flexibility is usually bargained with local unions and it is unlikely that firms succeed to adopt variable or performance-related pay schemes without unions consensus. Non-bargaining firms are on average small and characterized by low unionization rates; usually they are stick to the national contract of the metalworking sector, paying the wage levels that national metalworking unions and employer associations agreed upon (and that are reported in the most recent contract). The probability of introducing wage flexibility is then highly correlated with the probability of adopting a firm contract. As long as a firm can potentially sign a local contract, that firm can also potentially introduce a performance-related pay scheme.

We will take into account this implicit eligible rule in the following empirical analysis.

7. Some empirical evidence

Table 1 presents the distribution of the sampled firms by year of introduction of flexible wage schemes. Over the period considered, around 1600 firms (10% of total sample) adopted any kind of flexible pay schemes (piece rates, team output-related schemes, profit sharing, individual performance-related incentives, etc.). Even if the total number of firms sampled each year is relatively stable (between 10-13% of total sample), the share of firms introducing flexible wages varies significantly, ranging from 2-4% in 1991-93 to 26% in 1996.

This may due to the effect of both the economic cycle and institutional changes. The table clearly shows that the share of firms introducing flexible wage schemes is in fact lower in downturn years (1992-94), while it is higher before the economic situation deteriorates (1989-90) or when the recovery starts (since 1995). Furthermore, the incidence of firms introducing wage flexibility is higher in 1995-97 than earlier, probably due to the joint effect of the 1993 Agreement and the 1994 metalworking contract discussed in section 2[15].

In order to study the effects of the introduction of flexible pay schemes, and to reduce endogeneity and self-selection bias, we need to observe the main characteristics of the sampled firms before the introduction of wage flexibility and the values of the outcome variables at some points in time since the implementation of the flexible schemes (see also section 5).

Since it is also possible that a shift in the wage policy may need some time to influence firm and workers performance, we should evaluate both short run (e.g., one year since the treatment) and medium run effects (e.g., three years since the treatment).

[15] Previous studies have already pointed out how the 1993 Agreement produced significant changes in both the types of wage premiums and the role of union organizations within the bargaining process at the local level (Origo, 2000).

We then restrict our analysis to those firms that are observed, other than t (the year of treatment), also at least at t-1, t+1 and t+3. This leads us to a sample of 2273 firms, of which 99 were treated. This also imply that we are considering only the 1990-94 years as the treatment period (i.e., the years of introduction of wage flexibility). The restriction to this period can also explain the reduction in the share of firms introducing wage flexibility (4.4%, vs 10% in the whole sample), given the effects of the economic cycle and institutional changes discussed above. In each relevant year, the share of treated firms in the restricted sample is anyway similar to that in the initial sample (table 1)[16].

Figure 2 depicts the evolution of the outcome variables over time by treatment and control group (i.e., firms adopting wage flexibility vs other firms). In light of the eligibility issues discussed in section 6.2, the control group is split into firms without a firms contract and firms with a firm contract but without flexible pay schemes. Overall, the analysis of the outcome variables over time seems to reveal common trends between the treated and the control groups, mainly in the case of bargaining firms without wage flexibility, but the size of the change (with the exception of the proxy of productivity variance) is larger in the case of firms introducing wage flexibility.

To see if part of the differences between the two groups can be explained by differences in their average observed characteristics, table 2 reports the mean of the main characteristics by firm type. If we compare all the firms adopting wage flexibility with those not doing it (column (e) vs column (a) in the table), the treated firms are essentially much large and unionized than the others. Consequently, the are also more likely to have more than one establishment, to outsource part of the production, to be more capital intensive and to use more shift workers. Furthermore, they present a much lower number of hours of temporary lay-offs per worker (17 vs 44), meaning that it is more difficult to introduce flexible compensation schemes when the firm is facing a severe demand decline and it is trying to re-organize its workforce.

If we restrict the control group to the firms with a local contract but without flexible wage schemes, the differences with the treated group are now much smaller (column (e) vs column (c)). Actually, even if the firms belonging to this specific control sub-group are still smaller (mainly in terms of number of establishments), they are more unionized (the share of unionized workers is 46%, with respect to 41% in the treated group) and with more conflictual industrial relations (as shown by the highest number of hours of strike per worker). This result seems to suggest that strong unions might oppose to the introduction of wage flexibility[17] and this change can be opposed by a risk-adverse workforce, that can eventually see wage flexibility as a shift of the entrepreneurial risk from the firm to the workers (Ichino, 1994).

In light of the possible effects of the economic cycle on both the probability of introducing flexible wage schemes and their subsequent impact on firm performance, it is interesting to study whether firms characteristics change with the timing of adoption. If we consider the value-added of the metalworking sector as an indicator of the business cycle (figure 3), we can split the 1990-94 period into two different sub-periods:

[16] The comparison between the large and the restricted sample shows that they differ mainly for those characteristics that are sensitive to the cycle (firm-level bargaining and use of lay-offs).

[17] For similar results on the UK, see Gregg and Machin (1998). For previous results on the Italian evidence, see Origo (2000). For further evidence, see also the first chapter of this dissertation.

1990-91 (before the downturn gets severe) and 1992-94 (the downturn). The last four columns of table 6 present the main characteristics of the firms introducing wage flexibility by year of adoption. The comparison of the means reported in column (g) and column (i) reveals that the average treated firm in 1990-91 is not identical to that in 1992-94. Firms adopting flexible wage schemes in the first sub-period are in fact larger but less export-oriented, less capital intensive, with a higher share of white collar workers but a workforce that is on average less tenured. As expected, the number of hours of temporary lay-offs is much higher during the downturn sub-period (23 hours in 1992-94, 8 hours in 1990-91), but also the share of firms using overtime is higher (96% in 1992-94, 92% earlier). Workforce unionization is much higher in 1992-94 than in 1990-92 (respectively, 47% and 32%) and so is internal conflict, as shown by the number of hours of strike per worker (0.10 in 1992-94 vs 0.2 in 1990-91).

8. Econometric results

Table 3 presents some first estimates of the impact of the introduction of wage flexibility on the outcome variables discussed above. This set of estimates were obtained using comparison of unadjusted means between the treated and the control group, before-after estimator and difference-in-differences estimator.

According to all three estimators, the introduction of wage flexibility seems to increase the level of productivity and the productivity gain is higher the longer is the time since the treatment. This effect looks larger if we compare the productivity level of the treated firms before and after the treatment itself, while it is lower if we also control for similar time trend experienced by the control group (as with the D-D estimator). Overall, these estimates suggest that the introduction of flexible wage schemes increase the productivity of labour by 16-23 thousands Euros one year after the treatment (around a 20% increase), 22-36 thousand Euros three years since the treatment took place (around a 27% increase).

Estimates of the effect of wage flexibility on labour turnover are much more ambiguous: regardless the group of workers considered (either blue or white collars), the sign (other than the size) of the effect changes with the estimator used and most of the estimates are not statistically significant[18]. This seems to suggest that either sorting effects are negligible (probably also due to the stricter employment protection characterizing the Italian labour market with respect to the American one) or aggregate turnover rates are not a good proxy for capturing these effects over time, since individual behaviour are quite heterogeneous and production ability is not perfectly correlated with skills.

Regarding real wage levels, all the three estimators point out that the introduction of wage flexibility increases wage levels both one year and three years later. On the basis of the mean comparison and the D-D estimator, we can argue that flexible wage schemes increase real wages by 4-6% one year since the treatment, 6-8% three years later.

[18] Similar results were obtained also using total separation rates and voluntary quits as a measure of workers turnover

If we finally consider the productivity variance, also in this case the empirical evidence is quite mixed: estimates sign varies with the estimator used and estimates are not always statistically robust. As in the case of turnover rates, this result might suggest that either wage flexibility doesn't really change output dispersion within the firm or that our proxy of this outcome variable can't fully capture this effect.

Overall, estimates presented in table 3 suggest that the introduction of wage flexibility increases productivity levels and part of the productivity gain is shared with the workforce through higher wage levels.

In the following part of the empirical analysis we will then investigate more deeply the effect of wage flexibility on both productivity and wage levels, with the aim of isolating the net impact of wage flexibility on these two outcome variables.

Table 4a reports OLS estimates – i.e., the comparison of conditional/adjusted means - of the effect of wage flexibility on labour productivity and real wages at t+1 and t+3 (i.e. one and three years since the treatment). According to these estimates, wage flexibility increases productivity both one and three years since its introduction, but the estimates are no longer statistically significant once controlling for observable characteristics. In the case of real wages, the treatment still produces positive (but not statistically significant) effects on the outcome variable at t+1, while the effect at t+3 is now negative (even if not statistically significant). Similar results are obtained also for the adjusted versions of the before-after and difference-in-difference estimators[19]. Other variables are then responsible of higher productivity and wage levels: such as firm size, the share of white collar, shift and unionized workers, the use of overtime, the economic sector and the region of localization.

Table 4b report the OLS estimates of the effect of wage flexibility considering only firms with a local contract and looking separately at two sub-periods of treatment: 1990-91 and 1992-94. Restricting the analysis to the sub-sample of firms adopting a contract doesn't change significantly the results. Nonetheless, the estimates of table 4b reveal that the effects on both productivity and wages are more relevant in the first sub-period than in the second one, confirming that the effects of the introduction flexibility might change with business conditions and, more in general, the period in which the treatment takes place. The strongest and statistically significant effects are in this case those on wage levels three years since the introduction of flexible compensation schemes in the two sub-periods: wages in treated firms are 5% higher than in control ones for the 1990-91 sub-period, 4% lower for the 1992-94 sub-period.

Even if OLS allow to partly take into account the effect of other observable factors (correlated with wage flexibility) on the outcome variables, there are some cases in which the bias of the conventional unadjusted three estimators persists even after controlling for observable characteristics (Heckman and Roselius, 1994). If, for example, the distribution of the vector X of observable characteristics is different between the treated and the untreated, conditioning on X may eliminate systematic differences in outcomes between the two groups. The OLS estimates discussed above might then be negligible (or not statistically different from zero) only because we are

[19] For example, the estimates of the effect of wage flexibility obtained using the DD estimator with controls are: 5.4 (robust t value=0.8) on productivity at t+1; 7.9 (robust t value =1.1) on productivity at t+3; 0.02 (robust t value =0.7) on wages at t+1; -0.001 (robust t value =0.03) on wages at t+3.

artificially comparing firms that are not actually comparable in light of their average observable characteristics.

Non parametric procedures, in particular matching estimators discussed in section 5.1, might then help to find the best control match for each treated firm.

Table 5 reports the estimated effect of the introduction of wage flexibility based on the propensity score matching. Treated and untreated firms have been matched on the basis of the propensity score estimated with the parsimonious model reported in annex I. The chosen specification satisfies the balancing property. Only observations belonging to the common support are selected. All estimates were performed for the whole sample and separately for the two sub-periods considered (1990-91 and 1992-94). The propensity score has been calculated each time for each sub-group considered. Estimates of the average effect on the treated (ATT) were obtained using both a nearest Neighbour Matching estimator (ATTN, column a) and a Kernel matching estimator (ATTK, column b). In column c the ATT was estimated restricting the control group only to firms adopting a firm contract. Column d reports estimates based on a specification controlling also for sample selection (panel attrition), taking into account that the firms observed for at least four periods in the restricted sample may not be a random selection of the larger (initial) sample. The propensity score matching was then computed using a bivariate probit model with sample selection[20].

Table 5 confirms that wage flexibility seems to have a positive (even if in general weakly significant) effect on productivity and a strong significant effect on wages. Some differences emerge in the two sub-groups and according to the specification adopted. Overall, propensity score estimates show that the introduction of wage flexibility increases labour productivity by 6-17% one year later, 8-15% three years later. Estimates by sub-period reveal that these gains are actually relevant only in the 1990-91 sub-period and mainly in the short run. Furthermore, they tend to be lower also when the control group is restricted to the firms adopting a local contract.

Similar results emerge in the case of wages: the estimated ATT is around 5% both one and three years since the treatment, but this effect is less relevant in downturn years and it is less statistically significant when only firms with local contracts are used in the matching procedure.

These results overall suggest that the introduction of wage flexibility may have positive and significant effects on both labour productivity and wages, but these results are sensitive to the business cycle and the definition of the control group.

DD matching estimates reported in table 6 mostly confirm the previous results. The structure of the table and the different specifications used are identical of those in table 5. Even when we replace the Conditional Independence Assumption (CIA) with the conditional Bias Stability Assumption (BSA), we still obtain that the introduction of flexible pay schemes makes both labour productivity and wages to rise, respectively, by 4-9% and 3-5%. Productivity effects are however less significant in the medium run, mainly when the control group is restricted to the nearest match with firm contract. Estimates by sub-period confirm the positive effect on productivity in 1990-91, while the effect on wages seems less relevant, suggesting that the CIA assumption may be in this case too strong.

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[20] For further details, see Annex II.

8.1. Sensitivity tests on the larger panel in the short run

It may be argued that most of the results previously presented depend on the specific firms selected and/or the time period studied, despite of the control for sample selection explicitly performed in some estimates. In fact, firms staying in the panel for at least four periods (t-1, t, t+1 and t+3) are likely to be larger and more stable than the average sampled firm in each cross-section: even if they introduce flexible compensation schemes, actual effects on both productivity and wages might be less relevant than in smaller and more dynamic firms. Furthermore, since the restricted sample doesn't include any of the years following the institutional changes discussed in section 2, it is uncertain whether these results can be generalized to other time periods. The question is particularly relevant also in light of the timing of local bargaining and the different number of firms that eventually introduced performance related pay schemes, respectively, before and after 1993-94 (see again table 1).

In order to test the sensitivity of the results discussed above, we extended the analysis of the effects on the outcome in the short run including in the sample also the firms treated in 1995-1996 that were observed both at t-1 (respectively, 1994 and 1995) and t+1 (1996 and 1997). This larger sample is made of about 5000 firms, of which 442 were treated somewhen between 1990 and 1996. The incidence of treated firms is now closer to that in the total sample (8.1% vs 10%) and we can also study whether something changed since the application of the 1993 Agreement and the 1994 national contract of the metalworking sector.

Table 7 and 8 report OLS, PSM and DD-matching estimates obtained for this extended sample[21]. Evaluation is necessarily carried out looking only at the effects of the introduction of wage flexibility on outcome variables at t+1.

These estimates are on average less robust and less clear-cut than those obtained with the restricted sample but, overall, they confirm most of the results previously discussed. Wage flexibility produces positive effects on both labour productivity (+4-6%) and wage levels (+1-2%). These effects are on average lower than in the restricted sample and they tend to fade away when the analysis is performed for the 1995-96 sub-period, for which only DD-matching estimates with the large control group are statistically significant.

These results may also suggest that the provision of incentives and top-down recommendations for the introduction of flexible wage schemes might induce firms to do so despite of the actual gains they can get, acting more on the basis of a sort of "mimic" process. Public support to flexible wage schemes may then incentive the introduction of "cosmetic" variable schemes, which are actually more similar to traditional fixed wage premiums.

8.2. Further investigations on the role of unions

The estimates discussed so far don't explicitly take into account the interaction between wage flexibility and unions discussed in section 3.1. Union presence and union density have been actually used as control variables: union density was included among

[21] These tables share the same structure of table 4b, 5 and 6, with the obvious exception of estimates controlling form sample selection.

the regressors in OLS estimates while, in the case of matching estimates, we assured that treated and untreated firms belonging to the same block had on average the same share of unionized employees in the workforce.

To directly test the role of unions in determining the effects of pay flexibility (proposition 4), we need to model eventual interactions between union power and the adoption of performance-related pay schemes.

More specifically, in OLS estimates we tried two different specifications: first, we interacted the continuous variable measuring union density with the dummy related to the introduction of wage flexibility; alternatively, the latter was interacted with a dummy capturing a relative high union power (greater than 30%, the median of the restricted sample). In the case of the two matching estimators, firms were split into two sub-groups on the basis of their relative position with respect to the median unionization rate and propensity score matching was then applied to each sub-group separately. The main results are reported in tables 9-10.

According to OLS estimates, the introduction of wage flexibility increases both productivity and wage level, but this effect is less strong the higher is the unionization rate (or in firms where union density is relatively high). As in the previous analysis, OLS estimated coefficients are however statistically significant only in the wage equation for the whole sample.

Matching estimates show that the productivity effect is actually relevant only in firms with a relatively low union density, where productivity gains might be quite substantial (between 25-50% both right after the pay switch and three years later). In the case of low unionized firms, estimates are still weakly significant (and more relevant in the case of DD - matching estimates) even when the control group is restricted to the firms with a local contract.

Results are less clear-cut in the case of wages, mainly if we look at the impact of wage flexibility in the medium run. Three years since the pay switch, wages are in fact 4-7% higher in the treated firms only for the sub-group with high unionization rates, while wages are lower (even if the difference is not statistically significant) in the treated firms with low unionization rates.

These results overall confirm that productivity gains generated by the adoption of performance-related pay schemes can be reduced by the presence of unions, but the latter can be strong enough to bargain relatively high (fixed) wages despite of the lower productivity gains[22].

[22] We should also test whether the effect of union power varies with the median skills of the workforce (proposition 5). OLS estimates were actually performed also separately for, respectively, low and high skilled firms and by interacting the wage flexibility dummy with union density and skill density. In both cases, estimates were coherent with the theoretical predictions but they were not statistically significant. In the case of matching estimators, the relatively low number of treated firms didn't allow to split further the sample.

9. Conclusions

The aim of this research was to study the effects of a change in the compensation policy on firm's performance. More specifically, the theoretical and the empirical analysis investigated whether and to what extent a shift from a fixed to a flexible (performance-related-pay) pay scheme could produce incentive, sorting and inequality effects, taking specifically into account the timing of adoption and the role of labour market institutions.

The main results of the analysis show that incentive effects are very relevant, while both sorting and inequality effects are either negligible or not well measured.

More specifically, productivity and wages are positively influenced by the introduction of a flexible wage scheme, but the magnitude of these effects depend on the timing of adoption (i.e., on business conditions) and the presence of unions. On average, the introduction of wage flexibility increases productivity and wages by, respectively, 5-15% and 3-5% in both the short (i.e., one year since introduction) and medium run (i.e., three years since introduction), but this effect is more marked in growing years than in downturn ones.

Productivity effects are also more relevant in low unionized firms, while the effect on wages is less clear cut, probably because highly unionized firms can anyway bargain higher (fixed) wages at the local level.

All these effects are also lower and less statistically significant if the comparison group is limited to the firms with a local contract.

From a methodological point of view, the analysis points out the importance of the choice of the control group and the issue of the common support in order to correctly evaluate the average effect of a certain treatment on the treated. The use of semi-parametric techniques allows to explicitly take into account these evaluation aspects, constructing at desk a situation resembling a natural random experiment. The availability of panel data allows to control for these issues even further, indirectly testing whether the available (cross-section) information is rich enough to replicate a random experiment.

In terms of policy implications, these results clearly suggest that public support to performance-related-pay schemes might be offset by adverse business conditions (or, more in general, if the output is not perfectly predictable) and by quite strong local unions, mainly when the workforce lack of the necessary skills to exploit the flexible scheme in terms of higher earnings.

The introduction of public incentives to the use of flexible wage schemes should also take into account that changes in pay arrangements at the firm level usually reflect adjustments of management strategy in the light of intense competition, new production organization and key changes in the organisational context. All of these factors are likely to be different from one organisation to the other, hence leading to 'personal' outcomes concerning variable pay (Arrowsmith and Sisson, 1999). Public support to wage flexibility should be itself flexible enough to allow each firm to adopt the most suitable flexible pay form, thus increasing the probability of actually obtaining good results in terms of performance.

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ANNEX I
Propensity Score estimates

Dep variable: introduction of wage flexibility (1=yes)

	Coeff.	z		Coeff.	z
ln(size)	0.5714	8.04	Sector (metal)		
multiplant	-0.1539	-0.64	Foundries	0.3872	1.09
% export	0.0007	0.31	Metallic tools	-0.7082	-2.27
% outsourcing	0.0038	1.14	Machines	-0.2751	-1.11
K density	0.0003	1.07	Metal micro-parts	-0.0031	-0.01
% white collars	0.3002	0.72	Precision tools	-0.6523	-1.62
% females	0.6608	1.64	Electronic equipments	-0.3846	-1.33
% skilled workers	0.4368	1.24	Transportation	-0.7300	-2.31
immigrants	0.0681	0.45	Machine installation	-0.5063	-1.39
TFR per worker	0.0219	1.22	Assistance mech.	0.1853	0.73
$(\text{TFR per worker})^2$	-0.0003	-0.54	Technical offices	-0.1069	-0.16
% shift workers	0.1741	0.51	Area (Nort-West)		
% unionized workers	0.0066	2.43	North-East	-0.1929	-1.27
strike hours per worker	0.5404	1.31	Centre-South	-0.0203	-0.09
overtime (1=yes)	0.3463	1.48	_cons	5.8197	1.2
lay off hours per worker	-0.0022	-2.34			
Added value (time fe)	0.0000	-0.54			
Lagged values of:					
productivity	-0.00002	-0.03			
turnover blue collars	-0.2340	-0.85			
turnover white collars	0.0287	0.13			
log(wage)	-0.9301	-2.28			
cv(wage)	-0.6727	-1.33			

Estimated propensity score

	Percentiles		
1%	0.0005	Mean	0.0494
10%	0.0011	Std. Dev.	0.0963
25%	0.0035	Smallest	0.0004
50%	0.0118	Largest	0.9843
75%	0.0487		
90%	0.1459		
99%	0.4677		

The region of common support is [.000394, .98433507]

Final number of bloks: 7
Balancing property is satisfied

Distribution of controls and treated by block

Inferior of block of pscore	Number Controls	Number Treated	Total
0.0004	1487	14	1501
0.05	182	17	199
0.1	147	23	170
0.2	65	27	92
0.4	6	9	15
0.6	2	5	7
0.8	2	4	6
Total	1891	99	1990

ANNEX II

Correction for sample selection bias

The restricted sample used for most of the estimates discussed in section 8 may not be a random selection of the initial sample. In fact, firms staying in the panel for at least four periods (t-1, t, t+1 and t+3) are likely to be different (for example, larger and more stable) than the average sampled firm in each cross-section. Since the adoption of matching estimators usually requires pre and post-treatment information on both firm characteristics and outcome variables, we are actually interested to see whether the restricted sample discussed above was randomly drawn from the larger panel of firms for which we have information at least one period before and after the treatment.

One way to control for sample selection when the final equation is non-linear (as in the case of the propensity score) is to compute a bivariate probit model with a selection equation as follows:

$$y_1 = 1[x_1\beta_1 + u_1 > 0] \qquad \text{propensity score equation}$$

$$y_2 = 1[x\delta_2 + u_2 > 0] \qquad \text{sample selection equation}$$

y_1 is observed only when y_2=1; x is always observed. In our case, the introduction of wage flexibility (y_1) is observed only if the firm stays in the panel for at least four periods (y_2=1).

x contains the same regressors as x_1 and an additional identifying variable that determines the probability of being selected but not the probability of adopting wage flexibility. This variable is a dummy used by the employer association to identify those firms that can be used to calculate wage time series. We assume and test that this variable influences the probability of being selected but not the probability of using wage flexibility.

The following table reports the estimated coefficients for the selection equation.

Annex II
Sample selection equation

Dependet variable: sample=1 if in the sample at least in three subsequent periods (t-1, t, t+1)

	Coef.	z
panel*	0.3103	7.3
ln(size)	0.0727	3.6
multiplant	-0.0851	-1.0
% export	0.0006	0.9
% outsourcing	0.0019	2.0
K density	0.0001	1.6
% white collars	-0.4513	-4.8
% females	0.0951	0.9
% skilled workers	0.3934	4.5
immigrants	-0.2062	-4.6
TFR per worker	-0.0001	-0.2
% shift workers	-0.0229	-0.2
% unionized workers	-0.0016	-2.1
strike hours per worker	-0.2390	-2.3
overtime	0.0313	0.7
lay off hours per worker	0.0000	0.7
Sector (metal)		
Foundries	0.1539	1.2
Metallic tools	-0.0365	-0.5
Machines	-0.1083	-1.4
Metal micro-parts	-0.1264	-1.4
Precision tools	-0.1039	-1.0
Electronic equipments	-0.1431	-1.6
Transportation	-0.1945	-2.1
Machine installation	-0.0894	-1.0
Assistance mech.	-0.0538	-0.7
Technical offices	-0.1587	-1.0
Area (Nort-West)		
North-East	-0.1301	-3.1
Centre-South	-0.4766	-7.5
Added value (time fe)	-0.0001	-31.0
Lagged values of:		
productivity	-0.0007	-4.8
turnover blue collars	-0.0381	-1.5
turnover white collars	-0.0305	-1.2
log(wage)	-1.1777	-11.9
cv(wage)	-0.0749	-0.5
_cons	22.1072	20.0

* It identifies the firms that are in the panel of the employer association for wage time series purposes
Corr(panel, sample)=0.10
Panel is used as identifdying restriction

Table 1
Adoption of wage flexibility, number of firms by year

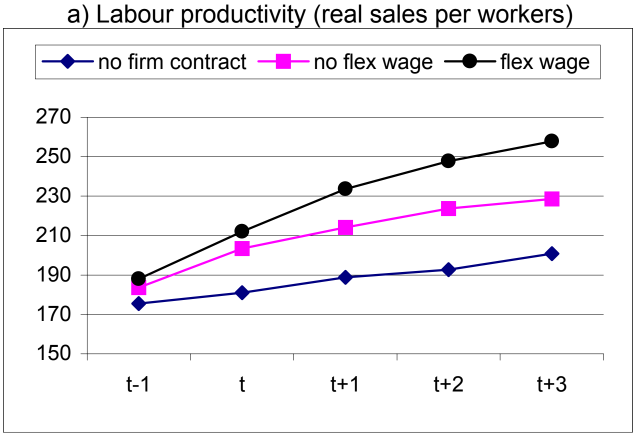
	TOTAL SAMPLE				RESTRCITED SAMPLE			
year	No	Yes	Total	yes/tot	No	Yes	Total	yes/tot
89	1960	252	2212	11.4				
90	1685	140	1825	7.7	453	30	483	6.2
91	1648	36	1684	2.1	441	10	451	2.2
92	1698	72	1770	4.1	537	24	561	4.3
93	1586	67	1653	4.1	400	17	417	4.1
94	1643	92	1735	5.3	343	18	361	5.0
95	1652	262	1914	13.7				
96	1459	521	1980	26.3				
97	1471	200	1671	12.0				
Total	14802	1642	16444	10.0	2174	99	2273	4.4

Figure 2

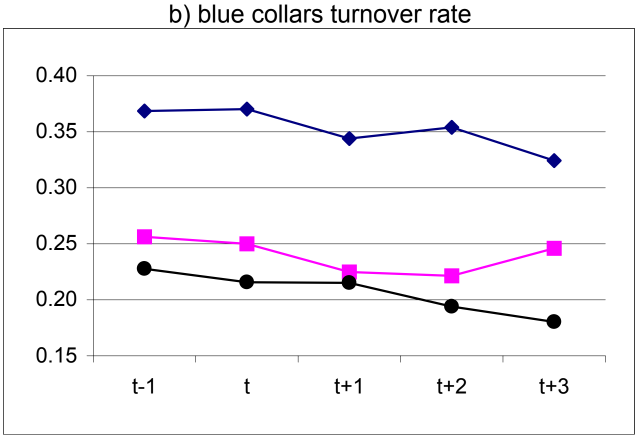
Trend of outcome variables, treatment and control group, restricted sample

ABSOLUTE VALUES

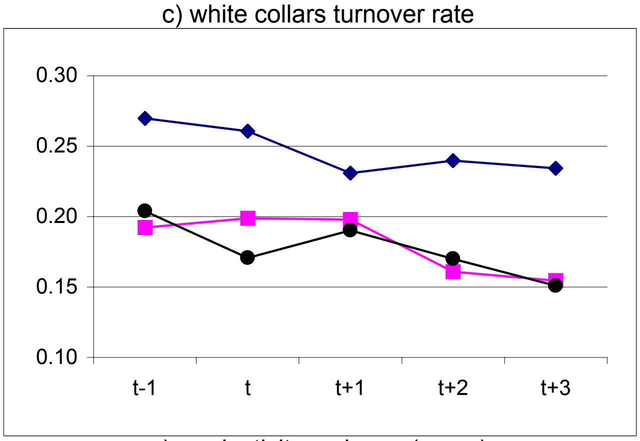
a) Labour productivity (real sales per workers)



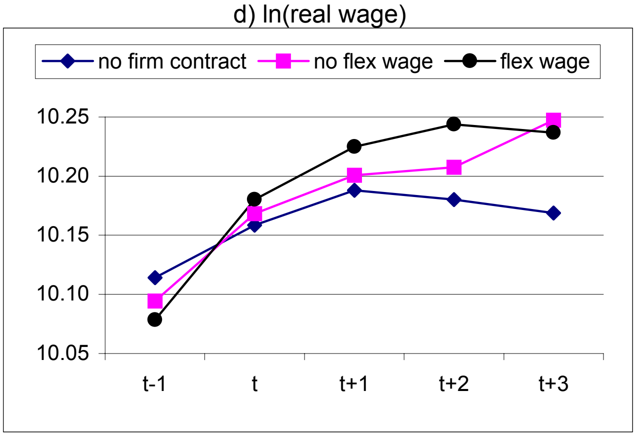
b) blue collars turnover rate



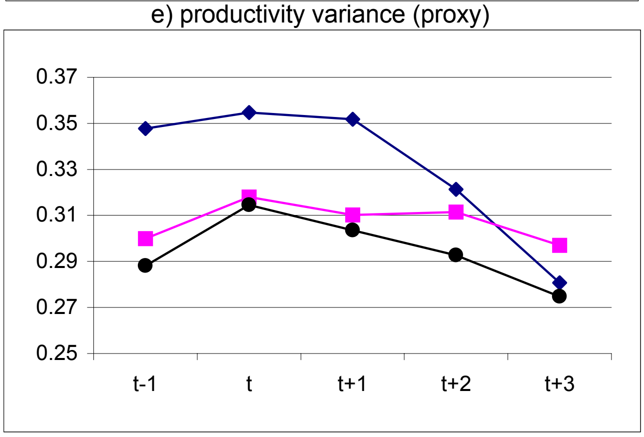
c) white collars turnover rate



d) ln(real wage)



e) productivity variance (proxy)



no firm contract = firms wothout firm contract
no flex wage = firms with a firm contract but without wage flexibility
flex wage = firms with a firm contract introducing wage flexibility

Figure 3
Real value added (base year=1995), metalworking sector
Million Euros

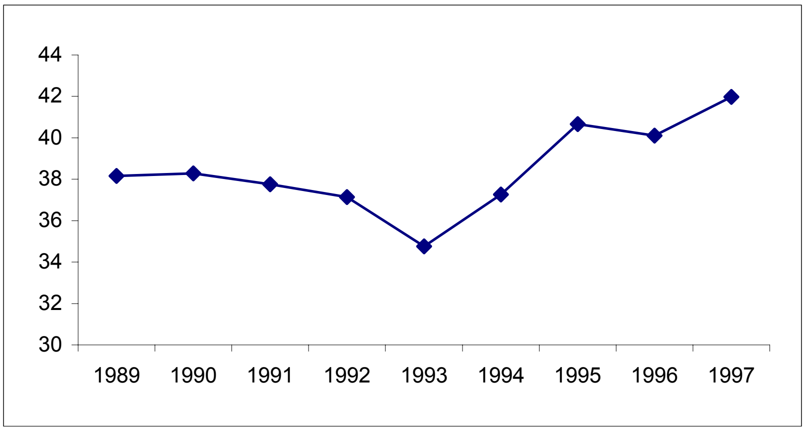


Table 2
Main characteristics by firm type, restricted sample

	No wage flex				Wage flexibility					
	all		with firm contract		all		1990-91		1992-94	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)	(l)
ln(size)	3.245	0.986	4.386	1.035	4.913	1.486	5.164	1.978	4.743	1.014
multiplant (1=yes)	0.041	0.199	0.060	0.238	0.131	0.339	0.150	0.362	0.119	0.326
% export	0.196	0.272	0.300	0.287	0.339	0.319	0.265	0.300	0.389	0.324
% outsourcing	0.108	0.181	0.142	0.202	0.149	0.198	0.140	0.188	0.156	0.205
K density (proxy)	50.374	158.614	59.273	60.326	75.380	68.592	64.430	75.911	82.803	62.738
% white collars	0.301	0.206	0.319	0.188	0.309	0.205	0.351	0.260	0.281	0.155
% females	0.196	0.176	0.190	0.158	0.215	0.181	0.209	0.187	0.218	0.178
% skilled workers	0.545	0.187	0.518	0.190	0.507	0.195	0.525	0.233	0.494	0.165
immigrants (1=yes)	0.153	0.360	0.147	0.355	0.232	0.424	0.150	0.362	0.288	0.457
Seniority (proxy, TFR per worker)	12.658	53.264	12.203	6.539	13.413	6.344	10.637	6.025	15.296	5.889
% shift workers	0.057	0.157	0.094	0.169	0.181	0.225	0.180	0.238	0.182	0.218
% unionized workers	0.178	0.244	0.456	0.232	0.409	0.226	0.322	0.196	0.468	0.227
strike hours per worker	0.047	0.149	0.100	0.161	0.069	0.113	0.021	0.040	0.101	0.134
overtime (1=yes)	0.782	0.413	0.933	0.250	0.949	0.220	0.925	0.267	0.966	0.183
temp lay off hours per worker	43.735	583.075	19.050	73.521	16.798	60.512	7.762	32.773	22.924	73.283
Distribution by sector:										
Metals	0.069	0.254	0.087	0.282	0.111	0.316	0.075	0.267	0.136	0.345
Foundries	0.025	0.157	0.047	0.212	0.051	0.220	0.075	0.267	0.034	0.183
Metallic tools	0.154	0.361	0.127	0.334	0.040	0.198	0.000	0.000	0.068	0.254
Machines	0.243	0.429	0.293	0.457	0.222	0.418	0.250	0.439	0.203	0.406
Metal micro-parts	0.072	0.258	0.087	0.282	0.111	0.316	0.100	0.304	0.119	0.326
Precision tools	0.044	0.205	0.047	0.212	0.040	0.198	0.025	0.158	0.051	0.222
Electronic equipments	0.095	0.294	0.127	0.334	0.152	0.360	0.100	0.304	0.186	0.393
Transportation	0.062	0.241	0.053	0.225	0.091	0.289	0.100	0.304	0.085	0.281
Machine installation	0.108	0.311	0.027	0.162	0.030	0.172	0.050	0.221	0.017	0.130
Assistance mech.	0.116	0.321	0.093	0.292	0.141	0.350	0.200	0.405	0.102	0.305
Technical offices	0.012	0.109	0.013	0.115	0.010	0.101	0.025	0.158	0.000	0.000
Distribution by area										
North-West	0.731	0.443	0.620	0.487	0.687	0.466	0.800	0.405	0.610	0.492
North-East	0.208	0.406	0.327	0.471	0.222	0.418	0.125	0.335	0.288	0.457
South	0.060	0.238	0.053	0.225	0.091	0.289	0.075	0.267	0.102	0.305
Lagged outcome variables (t-1)										
productivity	176.022	109.314	183.558	124.395	188.013	96.158	171.623	71.426	199.124	109.004
blue collars turnover rate	0.361	0.552	0.256	0.223	0.228	0.189	0.261	0.237	0.205	0.147
white collars turnover rate	0.264	0.654	0.192	0.191	0.204	0.216	0.211	0.224	0.199	0.212
ln(real wage)	10.113	0.176	10.094	0.130	10.078	0.172	10.075	0.154	10.081	0.184
cv(wage)	0.344	0.115	0.300	0.098	0.288	0.131	0.287	0.073	0.289	0.160
N. obs	2274		150		99		40		59	

Table 3
Unadjusted means comparison, before-after estimator and difference-in-difference estimator

	Means comparison		Before-After estimator		DD estimator	
	Estimate	t-value	Estimate	t-value	Estimate	t-value
Productivity level*						
t+1	22.2	3.5	23.6	5.4	16.0	4.1
t+3	28.5	4.2	36.1	5.4	22.3	4.6
Blue collars turnover						
t+1	-0.12	1.4	0.01	0.3	0.03	0.3
t+3	-0.14	1.3	0.01	0.3	0.06	0.4
White collars turnover						
t+1	-0.04	0.5	-0.02	0.5	0.02	0.2
t+3	-0.08	0.4	-0.10	2.5	-0.06	0.2
Wage level (ln)						
t+1	0.04	2.5	0.15	9.1	0.06	3
t+3	0.06	3.4	0.16	9.7	0.08	3.4
Productivity variance (cv)						
t+1	-0.05	3.5	0.01	0.9	0.01	0.4
t+3	-0.02	1.6	-0.01	0.7	0.03	1.9

* Thousand Euros

Table 4a
OLS estimates - restricted sample

	Productivity level (thousand euros)				**Wage level** (ln)			
	t+1	t (robust)	t+3	t (robust)	t+1	t (robust)	t+3	t (robust)
flex wage	**3.30**	**0.4**	**7.86**	**0.8**	**0.013**	**0.8**	**-0.008**	**-0.5**
nowflex_nocaz	-10.44	-2.0	-8.46	-1.4	-0.023	-2.1	-0.042	-2.9
ln(size)	-2.75	-1.7	-0.91	-0.5	0.009	2.1	0.021	4.5
multiplant	14.84	2.1	11.60	1.6	0.002	0.2	-0.001	-0.1
% export	0.20	3.9	0.16	3.2	-3.3E-04	-2.4	-1.6E-04	-1.0
% outsourcing	0.66	8.1	0.67	7.5	2.6E-04	1.4	2.1E-04	1.0
K density	0.02	1.3	0.01	0.9	-1.4E-05	-0.9	1.8E-05	1.2
% white collars	73.94	8.4	70.21	6.9	0.027	1.3	0.111	5.0
% females	11.41	1.4	6.54	0.7	0.006	0.3	-0.048	-1.7
% skilled workers	-9.92	-1.5	-7.70	-1.0	0.137	5.3	0.114	6.0
immigrants	4.58	1.3	3.24	1.0	0.018	1.7	-0.018	-2.1
TFR per worker	-0.02	-1.1	-0.02	-1.7	0.001	1.8	-8.5E-06	0.0
% shift workers	39.07	4.3	43.29	4.5	-0.026	-1.3	-0.058	-2.6
% unionized workers	-0.12	-2.1	-0.08	-1.1	-1.3E-04	-0.8	-3.4E-05	-0.2
strike hours per worker	-7.75	-1.1	-8.88	-1.3	0.003	0.1	0.021	0.7
overtime	11.88	4.2	11.75	3.7	-0.003	-0.3	-0.007	-0.7
lay off hours per worker	0.00	-0.9	0.00	0.1	-2.3E-05	-0.8	-5.4E-05	-1.8
Sector (metal)								
Foundries	-30.40	-3.0	-32.73	-3.3	0.013	0.6	-0.015	-0.5
Metallic tools	-26.27	-3.6	-28.24	-3.8	-0.029	-1.5	-0.039	-2.4
Machines	-37.63	-5.4	-39.29	-5.3	-0.043	-2.2	-0.021	-1.4
Metal micro-parts	-36.61	-4.7	-34.47	-4.2	-0.038	-1.9	-0.027	-1.4
Precision tools	-63.41	-7.5	-61.55	-6.9	-0.030	-1.2	-0.015	-0.7
Electronic equipments	-36.41	-4.6	-40.53	-5.1	-0.044	-2.3	-0.038	-2.0
Transportation	-38.13	-5.0	-41.03	-5.2	-0.020	-1.0	-0.036	-2.1
Machine installation	-35.25	-4.7	-41.43	-5.2	-0.054	-2.5	-0.053	-3.0
Assistance mech.	-35.03	-4.5	-39.05	-5.0	-0.033	-1.7	-0.002	-0.1
Technical offices	-91.01	-6.3	-75.67	-3.5	-0.057	-1.7	-0.049	-1.1
Area (Nort-West)								
North-East	3.16	1.1	1.72	0.6	-0.017	-2.1	-0.008	-0.9
Centre-South	17.91	3.3	14.59	2.6	-0.036	-2.7	-0.063	-5.0
Added value (time fe)	0.00	-2.4	0.00	-3.0	5.4E-07	0.4	7.5E-06	5.3
_cons	199.04	4.9	231.25	5.3	10.091	84.6	9.562	87.6
Nobs	2273		2273		1816		1816	
R2 adjusted	17.15		14.46		5.79		11.52	
F test	13.03		11.69		3.76		9.08	
(d.f.)	(30, 2242)		(30, 2242)		(30, 1785)		(30, 1785)	

Table 4b
OLS estimates - subsamples

Estimates of "flex wage" coefficient	t+1	t (robust)	t+3	t (robust)
Productivity level				
only firms with contract	2.59	0.3	6.56	0.6
1990-91	6.66	0.6	15.20	1.0
1992-94	-0.89	0.1	4.10	0.4
Wage level				
only firms with contract	0.01	0.8	-0.01	0.5
1990-91	0.03	1.3	0.05	2.1
1992-94	0.01	0.4	-0.04	1.8

Note: OLS regressors as in table 4a.

Table 5
Propensity score estimates
(avg difference between treated and controls, %)*

t+1	a	b	c	d
Productivity level				
All	0.17	0.14	0.06	0.07
t	1.6	2.1	0.6	0.9
1990-91	0.19	0.19	-0.02	0.22
t	1.3	1.6	0.1	1.5
1992-94	0.01	0.07	0.005	0.09
t	0.1	0.9	0.1	0.6
Wage level				
All	0.05	0.04	0.01	0.05
t	2.5	2.6	0.7	2.5
1990-91	0.07	0.08	0.07	0.08
t	2	3.2	1.7	2.6
1992-94	-0.01	0.007	-0.018	-0.009
t	0.6	0.3	0.9	0.3
t+3	a	b	c	d
Productivity level				
All	0.09	0.15	0.08	0.10
t	1.0	1.8	0.7	1.2
1990-91	0.15	0.16	0.07	0.16
t	0.8	1.0	0.4	1.0
1992-94	-0.02	0.10	0.02	0.10
t	0.2	1.1	0.2	0.8
Wage levels				
All	0.05	0.04	-0.01	0.05
t	2.0	2.3	0.3	2.2
1990-91	0.07	0.076	0.03	0.06
t	1.8	2.4	0.9	2.0
1992-94	-0.04	0.004	-0.06	0.016
t	1	0.2	1.3	0.4
Whole sample:				
N treated	99	99	99	99
N controls	83	1891	66	78
1990-91:				
N treated	40	40	40	40
N controls	33	514	24	29
1990-94:				
N treated	59	59	59	59
N controls	48	1089	45	49

* 0.X means that the outcome is X% higher for the treated
t-values based on bootstrapped standard errors
a. Nearest Neighbour Matching (ATTN)
b. Kernel matching (ATTK)
c. ATTN with restricted comparison group (only firms with local contract)
d. ATTN with control for sample selection

Table 6
DD propensity score estimates
(avg difference between treated and controls, %)*

t+1	a	b	c	d
Productivity level				
All	0.09	0.10	0.04	0.09
t	1.4	2.3	0.6	1.5
1990-91	0.13	0.20	0.14	0.15
t	1.1	2.1	1.0	1.7
1992-94	-0.05	0.05	-0.07	0.06
t	0.5	0.7	0.6	0.8
Wage level				
All	0.05	0.05	0.00	0.03
t	2.0	2.5	0.01	1.3
1990-91	0.01	0.02	0.01	0.004
t	0.4	1.1	0.5	0.2
1992-94	0.05	0.07	-0.01	0.07
t	1.2	2.7	0.4	1.9

t+3	a	b	c	d
Productivity level				
All	0.02	0.12	0.06	0.12
t	0.3	1.8	0.6	1.9
1990-91	0.10	0.16	0.23	0.10
t	0.8	1.5	1.5	0.7
1992-94	-0.07	0.08	-0.05	0.07
t	0.8	1.2	0.5	0.8
Wage levels				
All	0.06	0.05	-0.01	0.03
t	2.2	2.4	0.4	1.0
1990-91	0.02	0.03	0.004	0.008
t	0.5	1.4	0.2	0.2
1992-94	0.01	0.06	-0.05	0.04
t	0.3	1.6	1.1	0.8

Whole sample:				
N treated	99	99	99	99
N controls	83	1891	66	78
1990-91:				
N treated	40	40	40	40
N controls	33	514	24	29
1990-94:				
N treated	59	59	59	59
N controls	48	1089	45	49

* 0.X means that the outcome is X% higher for the treated
t-values based on bootstrapped standard errors
a. Nearest Neighbour Matching (ATTN)
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c. ATTN with restricted comparison group (only firms with local contract)
d. ATTN with control for sample selection

Table 7
Sensitivity test: OLS estimates for the extended short-run panel

Estimates of "flex wage" coefficient	t+1	t (robust)
Productivity level (thousand Euros)		
whole sample	2.18	0.5
only firms with contract	3.40	0.7
1995-96	5.54	0.7
Wage level (%)		
whole sample	0.01	0.7
only firms with contract	-0.004	0.4
1995-96	-0.03	2.0
N obs:		
total	5445	
firm contract, no wage flex	289	
firm contract, yes wage flex	442	
N obs 1995-96:		
total	1505	
firm contract, no wage flex	65	
firm contract, yes wage flex	290	

Note: OLS regressors as in table 4a.

Table 8
Sensitivity test: estimates for the extended short-run panel
(avg difference between treated and controls, %)*

Propensity score matching

	a	b	c
Productivity level			
All	0.04	0.08	0.06
t-test	0.8	2.0	1.0
1995-96	0.001	0.04	0.16
t-test	0.1	0.5	1.3
Wage level			
All	0.02	0.01	0.01
t-test	2.2	1.8	0.5
1995-96	0.01	0.01	-0.04
t-test	0.3	0.3	1.3
Whole sample:			
N treated	402	402	402
N controls	180	3609	144
1995-96:			
N treated	244	244	244
N controls	39	52	39

DD matching (nonparametric DD)

	a	b	c
Productivity level			
All	0.04	0.05	0.01
t-test	0.8	1.8	0.3
1995-96	0.04	0.07	-0.01
t-test	0.6	1.6	0.1
Wage level			
All	0.01	0.01	-0.02
t-test	0.8	1.6	1.1
1995-96	0.04	0.03	-0.02
t-test	1.8	1.7	0.7
Whole sample:			
N treated	402	402	402
N controls	180	3609	144
1995-96:			
N treated	244	244	244
N controls	39	52	39

* 0.X means that the outcome is X% higher for the treated
t-values based on bootstrapped standard errors
a. Nearest Neighbour Matching (ATTN)
b. Kernel matching (ATTK)
c. ATTN with restricted comparison group (only firms with local contract)

Table 9
OLS estimates with interactions between union and wage flexibility

Estimates of "flex wage" coefficient	t+1	t (robust)	t+3	t (robust)
Productivity level				
whole sample	11.88	0.7	17.97	0.7
%unioniz. & flex wage	-0.21	0.7	-0.26	0.5
whole sample	6.40	0.4	13.32	0.6
high union & flex wage	-4.39	0.3	-7.85	0.4
only firms with contract	3.20	0.2	-4.6	0.2
%unioniz. & flex wage	-0.02	0.1	0.3	0.5
Wage level				
whole sample	0.07	1.7	0.03	0.9
%unioniz. & flex wage	-0.001	1.8	-0.001	1.6
whole sample	0.05	1.1	0.004	0.1
high union & flex wage	-0.05	1.1	-0.01	0.6
only firms with contract	0.01	0.2	-0.01	0.3
%unioniz. & flex wage	0.0004	0.5	-0.0001	0.1
N obs:				
total	2273		2273	
firm contract	249		249	

Note: OLS regressors as in table 4a.

Table 10
Matching estimates by unionization rate
(avg difference between treated and controls, %)*

Propensity score matching					***DD matching (nonparametric DD)***				
t+1	a	b	c	d	**t+1**	a	b	c	d
Productivity level					Productivity level				
Low union	0.47	0.31	0.25	0.41	Low union	0.27	0.26	0.53	0.32
t	2.0	1.9	1.1	2.0	t	1.6	2.1	1.8	2.3
High union	-0.02	0.09	0.02	0.02	High union	0.10	0.09	0.02	0.05
t	0.2	1.1	0.2	0.2	t	1.3	1.5	0.2	0.7
Wage level					Wage level				
Low union	0.04	0.07	0.05	0.08	Low union	0.01	0.02	-0.002	-0.01
t	0.6	1.4	1.1	1.5	t	0.2	0.5	0.1	0.3
High union	0.03	0.03	0.02	0.04	High union	0.04	0.05	0.01	0.04
t	1.2	1.7	1.1	1.5	t	1.2	1.8	0.2	1.4
t+3	a	b	c	d	**t+3**	a	b	c	d
Productivity level					Productivity level				
Low union	0.52	0.31	0.20	0.43	Low union	0.33	0.26	0.42	0.35
t	1.7	1.2	0.7	1.6	t	1.7	1.6	1.8	2.0
High union	-0.05	0.07	0.06	0.01	High union	0.05	0.07	0.05	0.03
t	0.5	1.1	0.6	0.2	t	0.6	1.4	0.7	0.5
Wage levels					Wage levels				
Low union	-0.13	-0.06	-0.21	-0.10	Low union	-0.16	-0.12	-0.26	-0.21
t	1.1	0.7	1.4	0.8	t	1.2	1.2	1.4	1.6
High union	0.05	0.04	0.03	0.05	High union	0.07	0.06	0.01	0.05
t	2.2	2.0	1.1	2.0	t	2.1	2.5	0.4	1.7
Low union:					Low union:				
N treated	30	30	30	30	N treated	30	30	30	30
N controls	24	990	15	24	N controls	24	990	15	24
High union:					High union:				
N treated	69	69	69	69	N treated	69	69	69	69
N controls	49	408	36	50	N controls	49	408	36	50

* 0.X means that the outcome is X% higher for the treated
Low union = unionization rate up to 30% (sample median);
High union = unionization rate above 30%
t-values based on bootstrapped standard errors
a. Nearest Neighbour Matching (ATTN)
b. Kernel matching (ATTK)
c. ATTN with restricted comparison group (only firms with local contract)
d. ATTN with control for sample selection