Using propensity matching estimators to evaluate the impact of privatisation on wages*

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Abstract

Whether the transfer of ownership rights to the private sector leads to a decline (increase) on wage growth is theoretically ambiguous given that the outcome depends on the uncertain interaction between firms' and workers' behaviour. Using propensity matching techniques, this paper investigates the effects of privatisation on wages in the Portuguese banking industry. The empirical results obtained from *Quadros de Pessoal* for the period between 1989 and 1997, clearly confirm a positive relationship between wage variation and timing of economic restructuring for either men or women retained in the firm. Moreover, the results show that privatisation hit more intensely the most educated, experienced (oldest) and the best paid workforce irrespective of the gender.

Keywords: privatisation, wages, Portuguese banking industry, propensity matching estimators.

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1 Introduction

Despite the large and prolific literature on privatisation, the analysis of the causal effect of privatisation on wages remains almost neglected.[1] This is somewhat surprising as the transfer of ownership rights to the private sector has been the most notable structural reform introduced worldwide in the provision of public goods and services.[2] More importantly, its implementation has frequently been met with fierce resistance from both labour unions and local communities and attracted intensive press attention. Whilst policy-makers endlessly advocate gains in terms of firm's internal efficiency and profitability, labour unions fear adverse workforce adjustments including either displacement of jobs or reductions in pensions or wages, as a result of the restructuring process. Perhaps the lack of empirical research on this controversial topic merely reflects the unavailability of appropriate data. Prototypical research on privatisation uses data from firms' annual accountancy reports, which at the best, contain crude labour force information.

On the other hand, at the theoretical level the relationship between privatisation and labour market outcomes is not obvious: privatisation does not necessarily cut jobs or lower wages. Employment and wages may decline as privatisation implies a shift in the public firms' objective function towards profit maximisation and a reduction of union bargaining power. But if workers are willing to put in more effort after privatisation, then firms may settle for higher wages (see for instance, Goerke (1998), De Fraja (1993), Haskel and Szymansky

[1]Some notable exceptions include Brainerd (2002), Haskel and Szymansky (1993), Ho *et al.* (2002), La Porta and Silanes (1999), Monteiro (2002), Parker and Martin (1996) and Peoples and Talley (2001). Megginson and Netter (2001) survey the empirical literature on privatisation.

[2]Megginson *et al.* (1994) provide an excellent historical overview of postwar privatisations.

(1992) or Haskel and Sanchis (1995)). Similarly, if new ownership brings fresh capital and expertise, such changes are likely to generate growth and job creation.

This paper contributes to this discussion, by implementing a variety of increasingly popular nonexperimental methods, labelled *propensity matching estimators*, to assess the impact of privatisation on wages. In particular, this study re-visits the effects of privatisation in the Portuguese banking industry, where the already accomplished reform is considered a "*valuable experience for other countries",* since "*the main reform objectives were met*" without "*the concomitant financial instability experienced by many OECD countries*"(See OECD, 1999, page 64). In this way, this study also contributes to the long-standing debate in the literature, until now almost exclusively confined to the evaluation of *active labour market policies*, over whether treatment effects in observational studies can be reliably evaluated without a randomised experiment. This study is empirically fruitful for several reasons.

First, apart from the remarkable success of the already aforementioned policy in the banking sector, the design of the privatisation programme in this industry provides a promising opportunity for examining the effects of a change in ownership. Indeed, privatisation not only did not affect all public firms (there is still a large state-owned group) but also took place continuously over eight years. Hence, this *partial* and *ongoing* privatisation design permits us to pair individuals both in the same labour market and with common public employment status. Therefore, we avoid the potential bias resulting from labour market mismatch (Heckman *et al.*, 1998) commonly observed in observational studies and the self-selection bias inherent in the classical model of Heckman (1979) in the context of private and public sectors.

Second, the adoption of propensity matching estimators is also economically appealing for analysing the impacts of privatisation. In fact, as privatisation is likely to cause disproportionate changes in the composition of the workforce in privatised firms compared to public firms, we would prefer a strategy robust to this unequal employment composition variation. Because matching consists of ex post re-establishment of the conditions of an experiment (where the treatment and control groups are statistically equal in every respect except treatment status) by pairing each programme participant, according to observable attributes, with members of a comparison or non-treated group, this effect is naturally controlled. Besides, matching is a flexible approach that avoids definition of a specific form for either the outcome equation, decision process or the unobservable term.

On the other hand, this class of estimators is also appropriate to appraise the effects of the reform over both the short and long run. Indeed, the original cross-section pairwise matching estimators have been recently extended not only to new multiple matching schemes, but also to the case of repeated cross-sectional or longitudinal data (Heckman *et al.*, 1997). These new modified versions, which will be described below, are less restrictive in assumptions and thus can produce more accurate estimates.[3] The original matching assumptions are well suitable for short-run effects of treatment whereas these new extensions are likely to become more plausible as we attempt to pick up more persistent medium-long term effects of privatisation. Discrepancies across the various estimators are informative in pinpointing which assumptions are reasonable in each time period of analysis.

Finally, this class of estimators has heavy data requirements since the quality of matching estimates mirrors the quality/quantity of the variables employed. This paper uses data from

[3]For details see the original papers of Heckman *et al.* (1997, 1998) or the discussions in Smith and Todd (2000) and Blundell and Dias (2002), for instance.

a large dataset, *Quadros de Pessoal*, collected by the Portuguese Ministry of Labour and Solidarity. This extensive matched employer-employee database provides detailed information about each unit, firm or individual, during the period before and after the privatisation. Hence, it allows us to draw samples of different nature (cross-section and longitudinal) and then, implement all the entire class of matching estimators. Moreover, as all treated and control units respond to the same mandatory employer report, there is no bias resulting from differences in survey questionnaires (Heckman *et al.*, 1998).

The rest of this paper is structured as follows. Next Section 2 discusses briefly the main features of the privatisation process and the labour relations prevailing in the Portuguese banking sector. Section 3 presents an overview of the assumptions and variety of the *matching estimators*. The data implementation issues are addressed in Section 4. Section 5 outlines and discusses the empirical results. Section 6 closes the paper summarising the main lessons of this study.

2 Privatisation and the Portuguese banking labour market

The privatisation program was introduced in the banking sector, as a further step in the successful reform of the Portuguese financial system (OECD, 1999). This structural reform, starting in 1984, aimed to put an end to the heavily regulated and nationalised system imposed in the industry after the 25$^{\text{th}}$ April 1974 revolution. Less than one decade afterwards, when most of the deregulation reforms were already accomplished, including the dismantlement of the interest rate controls and the openness of the financial intermediation to the private sector, the privatisation program was then implemented.

The first privatisation law adopted in 1988 (law 84/88 from 20$^{\text{th}}$ July) allowed merely *partial* privatisation of public enterprises as the State still retained 51 per cent of the equity. For this first phase of privatisation, the government selected four profitable firms, which included one medium size bank. In April 1990, after a second Constitutional Amendment laid down in June 1989, the *lei Quadro das Privatizações,* (decree-law 11/90 from 5thApril) was passed allowing full privatisation of enterprises nationalised after 1974. The privatisation program was assumed to be an important mechanism for (1) improving the deteriorated performance of public economic units, (2) modernising and increasing their competitiveness and (3) widening the participation of Portuguese citizens in the ownership of enterprises, particularly among workers and small shareholders.[4]

The firms being privatised were first transformed into corporations, with a prior evaluation being made by two independent entities. But in contrast with some other economic sectors, (for instance, electricity and telecommunications) the government opted for a policy of no interfering in the public firms during the period before privatisation (Naumann, 1995 and Sousa and Cruz, 1995), leaving the economic restructuring for future private owners. In terms of scheduled order of privatisation, apart those firms which were selected on grounds of performance indicators for the partial privatisation phase (OECD, 1989), there was no firm schedule for subsequent firms' privatisation (OECD, 1991). Instead, the timetable was strongly affected by the economic and political domestic cycles' and by the international context.

By mid1997, ten out of twelve public banks became fully private: two banks were privatised in 1991, three in 1994, and each of the five remaining banks were privatised in 1989,

[4]Sousa and Cruz (1995) describe and discuss the economic and financial situation of public enterprises.

1990, 1992, 1993 and 1996, respectively.[5, 6] The most common privatisation procedure used, was public offer, and to a much less extent, direct sale or public tender. The broadening share-ownership goal clearly desired by the authorities was not achieved, instead a managerial dominant type of ownership emerged (although the employees had the right to subscribe to some part of the capital of the privatised firm at preferential rates). In most cases, ownership returned to former Portuguese groups, which owned them prior to the nationalisation wave in 1974.[7] Due to this *private-public-private* ownership path, privatisation in Portugal is termed *re-privatisation*.

As a result of the divestiture reform, significant improvements in terms of productivity and efficiency levels were registered in the Portuguese banking industry. For instance, the OECD 1999 survey, referring to the commercial banking industry, reports a continuous increase in the productivity level (balance sheet total per employee), which allowed not only a reduction in operate/staff costs (from 1.53 per cent of average assets in 1991 to 0.98 percent in 1997) but also a remarkable improvement in the profitability rate (return to equity) after 1995). This global rise in the efficiency level of the industry is also confirmed by Pinho (1999), who nevertheless attests to an increase that is particularly *more* pronounced *among* privatised institutions. In terms of labour outcomes, the main economic restructuring adjustments' are illustrated in Table 1. For comparison purposes, the public category refers to the 2 permanent public banks whereas the privatised category includes the 10 firms being

[5] This total number (ten) of firms privatised in the banking industry does not coincide with the eleven privatised firms reported by the OECD 1999 survey. This discrepancy is due to both the absence of one bank in the data, the exclusion of a bank, whose privatisation implied the transfer of a minority participation (15 %) to the private sector and the inclusion of the indirect privatisation of a public bank through the privatisation of the group to which it belongs (see page 22, Ministério das Finanças, 1999).

[6] According to the privatisation literature, the date of first tranche sell of each firm is considered the date of effective privatisation.

[7] International investors could buy a limited share of the equity ranging from two to forty percent of sales.

privatised.

In contrast with public firms whose level of employment remained fairly constant from 1991 on, the level of employment in privatised firms dropped steadily during the reform period. Each privatised firm lost on average 732 employees between 1989 and 1997 (implying a 23 per cent (3884/3152) rate of overstaffing), which corresponds to a loss of 92 employees per firm/year during the same period. This is further confirmed by the increasing number of workers, particularly women, declared unemployed from the financial industry over the referred period despite the absence of any failure or closing institution. Nevertheless, in terms of job security, at least when measured by the share of permanent full time workers, there was no deterioration in privatised firms once this proportion increased during the entire period of analysis.[8]

The trend in banking workers's wage is also clear: both public and privatised firms' workers experienced a strong (real) wage rise, mainly reflecting the fast economic growth observed in the economy, after Portuguese membership of the European Community in 1986. For privatised firms' workers however, the wage increase is slightly more pronounced (33 percent) than in public firms (27 percent), implying *vis-a-vis,* a positive privatisation impact on the wage level. On the other side, the rise in the wage dispersion in privatised firms, when measured by the standard deviation of hourly wage, may suggest *heterogeneous* privatisation wage impacts. Notice that this simple analysis besides not accounting for changes in the workforce composition, ignores the time elapsed since the introduction of the

[8] In some cases, the corporate economic restructuring involves the adoption of less secure job (human resource) practices, including either temporary or partial employment, in order to achieve more *flexible* industrial relations. Cam (1999), for example, reports significant jumps in the number of temporary posts in the Turkish cement industry.

reform in each firm, which possibly mitigates dynamic privatisation effects.

In general, the new firm's profit orientation is likely to exert a downward pressure on wages and hence, erode the existent worker rents (Vieira *et al.*, 1997) owed to regulation/nationalisation waves'. Nevertheless, the scope for this wage erosion is limited, as unions in the banking industry represent *all* of the workforce in the wage bargaining process, *regardless of the ownership* of the bank. Moreover, the union bargaining position in the this industry (historically the largest and most influential in the country) has been reinforced over the course of reforms, in contrast with other sectors in the UK and USA, which were exposed to similar market oriented policies (privatisation/deregulation).[9] Indeed, the union participation rate in the banking sector has expanded markedly between the period 1974-78 and 1991-95, from 71% to 106% (Cerdeira, 1997). *A priori,* the decentralised bargaining system should bring uniform wage levels across firms within the banking sector, although the positive differential between negotiated and effective wage levels has widened since the early nineties (Aperta *et al.*, 1994).

3 Econometric considerations

Assessing the impact of privatisation on wages of workers, whose firm's ownership was transferred from state to private hands, requires making an inference about the wages that would have been observed had the privatisation program not been introduced. As one can not observe the wage paid to each privatised firms' employee in case the reform had not taken place, the establishment of the casual effect becomes a problem of inference with missing

[9]Peoples (1998) reports a decline in the unionisation density after liberalisation of either trucking, telecommunications or airlines industry in the USA. This result is also found in developing countries, for instance in Turkey de-unionisation also accompanied privatisation reform (Cam, 1999).

data.

To be precise, let us state formally this causal effect. Denote by W_{i1} and W_{i0} the wage paid to an individual i (outcome or variable response) conditional on the presence and absence of *treatment* (privatisation), respectively. D_i is a participation variable that identifies whether employee i received "treatment", *i.e.* was employed in a firm that was privatised, $(D_i = 1)$ or not $(D_i = 0)$. Finally, X_i represents for each individual i, a set of *attributes,* variables such as gender or age, that are unaffected by the treatment under study. The missing data problem arises because it is impossible to form the impact of the policy for any $i - th$ individual, $\triangle_i = W_{i1} - W_{i0}$, as the observed wage for an employee i is given by $W_i = W_{i0} + D_i (W_{i1} - W_{i0})$ with only one of W_{i0} and W_{i1} being observed at any given point in time.[10] For all those individuals *treated*, one is interested in estimating the most common parameter in the evaluation literature, $E(W_{i1} - W_{i0} | D_i = 1, X_i)$, also referred as *the effect of the treatment on the treated.*

In social experiments, the evaluation problem is in principle solved, by virtue of random assignment to participation, which guarantees that the potential outcomes are independent of the assignment mechanisms, and then $E(W_{i0} | D_i = 1, X_i) = E(W_{i0} | D_i = 0, X_i)$.[11] In contrast, in observational studies, assignment is not random resulting either from individual self-sorting, selection made by a program manager or both. Much of the evaluation literature either on causal models in statistics or on selectivity models in econometrics, is devoted to

[10] We are implicitly adopting the stable unit-treatment value assumption (SUTVA) first expressed by Rubin (1980). This assumption requires that an individual's potential outcome is independent of the treatment status of other individuals, ruling out any eventual within-group or spillover (general equilibrium) effect.

[11] There are, however, several other problems, such as no perfect compliance with treatment (the absence of non random drop-outs) associated with the experimental design that may plague randomised experiments in social policy evaluation. See, *inter alia*, Heckman and Smith (1995) and Heckman *et al.* (2000), for detailed discussions.

finding the identifying assumptions that allow the estimation of $E\left(W_{i0}|D_i=1,\, X_i\right).$

In matching, the fundamental assumption, *Conditional Independence Assumption* (CIA), states that treatment assignment (D_i) conditional on attributes (X_i), is independent of the potential wages (W_{i0}, W_{i1}). In formal notation, this assumption corresponds to

$$\left(W_{i0}, W_{i1}\right) \perp D_i \mid X_i, \tag{1}$$

where $\perp$ denotes independence.[12], [13] This means, that given X_i, one can use non-participants' wages to approximate the (counterfactual) wage level of participants had they not participated.[14] Hence, matching consists of looking for each treated observation, a set of non-treated observations with the same realisation of X_i. In the language of Heckman and Robb (1985) matching assumes that selection occurs only *on observables*. Therefore, CIA excludes the familiar dependence between outcomes and participation that is central to econometric models of self selection: there are no important variables apart from X_i, on which the analyst can not condition, that effect both the non-treated outcome (W_{i0}) and assignment (D_i). If this were the case, then selection would be *on unobservables*.[15] Moreover, this assumption implies that individuals either do not participate in the program on the basis of potential gains $(W_{i1} - W_{i0})$ or that the analysts have as much information about the program being

[12] Ibems (2000) and Lechner (2001) generalise this simple framework to the case of multiple and alternative treatment states.

[13] "Ignorable treatment assignment" in the terminology of Rubin (1977) and Rosenbaum and Rubin (1983).

[14] More precisely, for estimating the *effect of the treatment on the treated*, $E\left(W_{i1} - W_{i0}|D_i=1,\, X_i\right)$, it is sufficient to fulfil a weaker version of CIA, $W_0 \perp D_i \mid X_i$ and then $E\left(W_{i0}|D_i=1,\, X_i\right) = E\left(W_{i0}|D_i=0,\, X_i\right).$

[15] Note however, that if an extra additive separability condition holds on the outcome equations, $W_0 = g_0\left(X\right) + U_0$ and $W_1 = g_1\left(X\right) + U_1$, CIA doesn't imply a zero (unobservable) selection bias, this is $E\left(U_0 \mid X, D=1\right) = 0$. Instead, matching *as* randomisation, *balances* the bias, $E\left(U_0 \mid X, D=1\right) = E\left(U_0 \mid X, D=0\right) = E\left(U_0 \mid X\right).$

studied as the agents entering into the program.

A practical implementation problem arises when the vector X_i is highly dimensional and contains continuous variables. To circumvent this difficulty, Rosenbaum and Rubin (1983) show that matching on a scalar function of X_i, such as the propensity score, $P(X_i) = \Pr(D_i = 1|X_i)$, the conditional probability of participation given the vector of observed attributes, is sufficient to balance the covariates X_i between the treatment and control units.[16] Therefore, if CIA holds conditional on X_i, it will also hold conditional on the propensity score,

$$(W_{i0}, W_{i1}) \perp D_i \mid P(X_i). \tag{2}$$

In this case, in order to have empirical content matching also requires,

$$0 < P(X_i) = \Pr(D_i = 1|X_i) < 1.^{17} \tag{3}$$

To satisfy this condition there must be both participants and non-participants for each covariate of the vector X_i. Failure to satisfy this assumption restricts the analysis to the region of support (all possible values of X_i) common to all treated and non-treated units and the estimated treatment effect has to be redefined as the mean treatment effect for those treated falling within the common region of support.

Under law of iterated expectations and assumptions (2) and (3), the effect of treatment

[16] This result holds for a more general balancing score $b(X_i)$. Balancing score is a function of the observed covariates X_i such that the conditional distribution of X_i given $b(X_i)$ is the same for treated and control units. X_i is the finest balancing score and the propensity score is the coarsest function of X_i that is a balancing score. In practice, it is possible to match on some variables X_i and on the propensity score. See for instance, Lechner (2002).

[17] This assumption together with CIA are the "strong ignorability treatment assignment conditions" in the terminology of Rosenbaum and Rubin (1983).

on treated can be expressed as follows,

$$
\begin{aligned}
& E\left(W_{i1}-W_{i0}|D_i=1,\ P\left(X_i\right)\right) \\
& =E\left(W_{i1}|D_i=1,\ P\left(X_i\right)\right)-E\left(W_{i0}|D_i=1,\ P\left(X_i\right)\right) \\
& =E_{P(X_i)}\left[E\left(W_{i1}|D_i=1,\ P\left(X_i\right)\right)-E\left(W_{i0}|D_i=1,\ P\left(X_i\right)\right)|D_i=1\right] \\
& =E_{P(X_i)}\left[E\left(W_{i1}|D_i=1,\ P\left(X_i\right)\right)-E\left(W_{i0}|D_i=0,\ P\left(X_i\right)\right)|D_i=1\right],
\end{aligned}
$$

where the outer expectation is over the distribution of $\left(P\left(X_i\right)\mid D_i=1\right)$.

By construction, matching eliminates two of the three selection bias sources identified by Heckman *et al.* (1998): the bias resulting from having different ranges of X_i for treated and control samples (comparing non-comparable individuals - failure of the common support condition) and the bias resulting from having different distributions of X_i across their common support (weighting comparable individuals incomparably). The remaining source of bias, differences on unobservables across groups, are ruled out by matching assumptions.

Under matching assumptions, the effect of treatment on treated is thus given by,

$$\sum_{i\in D=1} n_i\left(Y_{i1}-\sum_{j\in D=0} N_{ij}Y_{j0}\right), \tag{4}$$

where N_{ij} controls for the weight placed on each comparison observation j for individual i, n_i[18] represents the effective weight for the final treated sample and Y_{i1} and Y_{j0} stand now for a generic outcome, for the treatment and comparison groups, respectively.

A variety of different matching schemes are possible. Each scheme involves the definition

[18]This will tipically correspond to the size of the treatment group unless there are treated observations unmatched.

of a closeness criterion, a neighbourhood, and the selection of an appropriate weight function to associate the set of non-treated observations to each participant. For instance, the neighbourhood may range from a singleton set (one-to-one matching: nearest neighbour or within caliper) to a multiple set, eventually including all non-treated observations (n-nearest neighbours, radius matching, stratification, kernel and local linear regression-based matching).[19] The choice relies on the trade-off between variance and bias associated with each type of matching performed and the computational intensity allowed. In general, increasing the neighbourhood or bandwidth to construct the counterfactual will reduce the variance and increase the bias resulting from using on average more, but poorer matches. It will also rise the computational burden.[20]

After choosing the non-participants neighbour(s) for each i individual treated, the next step consists of selecting the weight function. The most common functions include the unity (equal) weight(s) to the nearest person(s) and zero to the others, and kernel weights, which downweight distant observations in terms of the propensity score. Silverman (1986) clarifies several alternative kernel functions.

A final remark concerns performing matching with or without replacement that is, using or not using the same comparison unit repeatedly in forming the comparison group. Similarly, using more than once the same non-treated unit may improve matching quality (reducing the bias) but it increases the variance.[21] In any case, differences resulting from the use of

[19] See Heckman *et al.* (1999) and Smith and Todd (2000) for a detailed description of each of these matching estimators.

[20] Using local linear weights enable a faster convergence rate at boundary points and better adaptation to different data densities (Heckman *et al.*, 1997).

[21] Dehejia and Whaba (1998, 1999) do find that the performance of simple matching with replacement very satisfactory when compared to more complex matching extensions or methods without replacement. In a multiple programe matching framework, it is required the use of matching with replacement. See, for instance, Sianesi (2001) and Lechner (2002).

different matching schemes, reflect the degree of overlap between treatment and comparison groups in terms of propensity score.

Matching can also be performed after regression adjustment as proposed by Rubin (1979) and formally derived by Heckman *et al.* (1997, 1998), leading to the *regression-adjusted matching estimator*. This estimator can be seen as a *restricted* version of the classical matching method once it imposes both additive separability and exclusion restrictions (widely used in the econometric models of self selection) across outcome and participation equations. Additive separability condition is motivated in order to confine any *bias* arising from the potential outcomes to a single "error term". Exclusion restrictions are adopted given the usual temporal discrepancy between enrolment and programme participation. Thus, the previous vector X_i can be split up into two groups of variables (R_i, Z_i) not necessarily mutually exclusive, where the subvector R_i determines the outcome equation, $W_{i0} = R_i\beta_0 + U_{i0}$, and the subvector Z_i determines programme participation given by $P(X_i) = \Pr(D_i = 1|X_i) = \Pr(D_i = 1|Z_i) = P(Z_i)$.[22] Thus, instead of the CIA, the regression adjusted estimator imposes

$$U_{i0} = \left[W_{i0} - R_i\widehat{\beta}_0\right] \perp D_i \mid P(Z_i), \tag{5}$$

implying that $E(U_{i0}|\, D_i = 0, P(Z_i)) = E(U_{i0} \,|\, D_i = 1, P(Z_i)) = E(U_{i0} \,|\, P(Z_i))$. Now, instead of assuming independence of the distribution of non-treated (treated) outcomes re-

[22] In contrast with the classical selection model, for which the identification conditions are well established (*see* Heckman and Robb, 1985), the necessary and sufficient identification condition(s) for the regression adjusted mathing estimator is(are) not clearly stated. However, despite the motivation of the exclusion resctrition suggest that $Z_i \not\supseteq R_i$, from the work done on the importance of number of conditioning variables included both in the propensity score and outcome equation, it seems to suggest that $Z_i \supseteq$ or $\subset X_i$.

garding participation provided one condition on X, one postulates that the distributions of the unobservables are the same in treated and comparison groups, once one conditions on $P(Z_i)$. As Heckman *et al.* (1998) show, the main advantage of this matching version is that it allows an improvement in the efficiency of the matching estimator by reducing its asymptotic variance.[23] In terms of implementation, the effect of treatment in this restricted matching version is given by (4) with $Y_{1i} = \left(W_{1i} - R_i\widehat{\beta}_0\right)$ and $Y_{0i} = \left(W_{0i} - R_i\widehat{\beta}_0\right)$.

In a repeated cross-section or panel context, it is still possible to implement another version of the matching estimator due to Heckman *et al.* (1997) called *nonparametric conditional difference-in-differences*. It results from an extension of the conventional difference-in-differences (DiD) estimator by defining outcomes conditional on X_i and using non-(or semi-)parametric methods to construct the differences. The critical identifying assumption, *the bias stability condition* using Eichler and Lechner (2002) terminology, states that conditional on X_i, the biases are the same on average in different time periods before and after the implementation of the program, so that differencing the differences between treated and non-treated units eliminates the bias. Let t and t' denote respectively, a time period after and before the program, then the effect of treatment on treated is identified if $E\left(W_{0t} - W_{0t'} | X_i, D = 1\right) = E\left(W_{0t} - W_{0t'} | X_i, D = 0\right)$. Thus, the effect of treatment under bias stability assumption is given by (4) for $Y_{1i} = (W_{1it} - W_{0it'})$ and $Y_{0j} = (W_{0jt} - W_{0jt'})$.

Compared to the original matching estimator, this new version is more robust once it requires a weaker assumption that allows for an unobserved determinant of participation. Hence, individuals' participation may be based on their potential program outcomes as long as the unobservability (individual and/or time-specific) rests on separable components of

[23] This new formulation may or not imply selection on unobservables. See details Heckman *et al.* (1998).

the error term.[24] Compared to pure DiD (Meyer, 1995), this estimator has the advantage of being nonparametric, so that successful identification does not depend on specific functional forms for the respective expectations.

If the additive separability and exclusion restrictions mentioned before, are fulfilled in periods t and t', then the assumption of the nonparametric conditional difference-in-differences becomes, $E\left(U_{0t}-U_{0t'}\right|P\left(Z_i\right), D_i=1) = E\left(U_{0t}-U_{0t'}\right|P\left(Z_i\right), D_i=0)$, leading to an analogous *regression-adjusted* nonparametric difference-in-differences estimator. In this restricted version, the effect treatment of treated is now defined for

$$Y_{1i}=\left[\left(W_{1it}-R_{it}\widehat{\beta}_{0t}\right)-\left(W_{0it'}-R_{it'}\widehat{\beta}_{0t}\right)\right] \text{ and } Y_{0j}=\left[\left(W_{0jt}-R_{jt}\widehat{\beta}_{0t}\right)-\left(W_{0jt'}-R_{jt'}\widehat{\beta}_{0t}\right)\right].$$

All these three new versions may be combined with any matching scheme mentioned previously. The choice again, relies on the computational burden and on the insurmountable trade-off between bias and efficiency.

4 Data and empirical specifications

The empirical part of this study relies on the *Quadros de Pessoal (QP).* This is a particularly large and informative data set collected annually by the Portuguese Ministry of Labour and Solidarity since the early eighties. It consists of a matched employer-employee database containing a high number of variables/concepts that meet international standards (see for example BIT, 1980) about each unit, firm or employee, observed. For instance, for each firm the data gives the location, level of employment, economic activity, type of manage-

[24] This assumption is consistent with the classic sample selection model.

ment, total sales and social capital. Similarly, for each employee, usual and unusual human capital variables, such as gender, level of schooling, tenure, promotion date, occupation, full-time/part-time status, earnings, duration of work and mechanisms of wage bargaining, among others are provided. This valuable dataset also includes an identification variable for either the firm or employee observed, which allows us to follow each unit over time.

Before describing the methodology used in this study for creating the data sample and the variables, let us state precisely the treatment effect one is interested in, which will condition the selection of treated and non-treated units. This study attempts to examine the effect(s) of privatisation on the wages of workers from the Portuguese banking industry using *all* variants of matching strategies described in section 3. As the *direct* target of this program is the firm *itself* and not the employees, one would ideally like to evaluate the privatisation impact on those employees that either remained, joined or left the firm after its privatisation.[25] In this case, for the "joiner or leaver" employee, it would also be required to know the reason for their moving in or out the firm, as the wage accepted by moving individuals varies remarkably according to their employment status. This kind of information is unfortunately unavailable in this dataset, which makes it difficult to interpret the results for these particular two groups. Further, if the employee became unemployed, self-employed or employed by local/central authorities (civil servants), one will not know which, as these organisations are not covered by this survey. In order to avoid these potential problems, this study strictly focuses only the effect(s) of privatisation on the wages for those employees that *remained* in the same firm after its privatisation. Therefore, our *treated units (*employees*)*

[25] This contrasts with *the active labour market policies,* in which both the policy and evaluation object targets coincide.

correspond to all individuals that both work in each public firm subject to privatisation and retain their jobs after the implementation of the reform. To be more precise, let t' and t denote two points in time, representing respectively one period before (pre-treatment) and one after (post-treatment) the privatisation of a given public firm. Thus, the treated group includes all individuals that work both in t' and t for the firm being privatised. The corresponding *control or untreated* group is composed of those workers employed in the remaining public firms (not subject to privatisation) and that, similarly, kept their jobs between t' and t. This choice allows us to match participants with controls not only across certain observable characteristics, but also by *pre-treatment public employment status*. Thus, one follows the spirit in the evaluation of active labour markets, in which only individuals with common labour market histories (employment) are matched.[26] More importantly, the selection of this particular control group enables us to bypass the self selection problem inherent in the classical selection model of Heckman (1979) in the context of private and public sectors, and then fully justify the plausibility/adequacy of matching assumptions in the present evaluation. In fact, it has long been recognised that employment in the public or private sector arises from an *endogenous* decision. Individuals sort themselves in either sector according to their own (mostly unobserved) skills and preferences (in terms of level of risk and complexity of the job, opportunity of internal promotion, quality of the working conditions, etc.), making the public employees a non-random sample from all (working) labour force. Because one is using information from the *remaining* public employees within the *same* industry for appraising the effects of privatisation, this unobservable component,

[26] Variables relating labour force status of treated individuals were found to be very significant (even more than earnings) in explaining the participation decision in trainning programmes.

responsible for the bias, is automatically controlled for. The remaining differences in terms of observable attributes among the public employees will be eliminated by using matching methods.

In addition, note that the purpose of analysis is to compute the *overall* impact of privatisation in the banking sector and not firm by firm effects. Consequently, the ten firm privatisations' need to be condensed into one "single privatisation". The creation of the data sample for estimation is a two step procedure. In the first step, for each firm being privatised is assigned one pre-treatment t' and post-treatment t points in time, and the respective treated and non-treated individuals are extracted. The choice of t' and t is driven by economic considerations. Because the firms' process of reactive restructuring occurred mainly after the implementation of the reform, as referred to in section 2, t' consists of a single calendar year prior to privatisation. In particular, the conventional procedure of the privatisation literature is followed, considering the calendar year of each firm privatisation, the year 0. Therefore $t' = -1$, corresponds to the calendar year prior to each privatisation date. In contrast, for the post-treatment period, one allows privatisation effects to vary over time following Gupta *et al.*'s (2001) discussion. The post-treatment period ranges between one and four years , $t = 1, 2, 3$ and 4, corresponding either to one, two, three or four calendar years after each privatisation date.[27] The second step consists of aggregating in each t' and t points in time, all treated and non-treated individuals of the respective ten firms privatised using a *moving window* as shown in Kluve *et al.*(1999). As a result, all individuals excluding those from the permanent public firms, are considered non-treated and treated at

[27] This postreatment period choice is also conditioned by the first merger wave in 1998 in banking industry, which involved recently privitised firms.

different points in time.

The empirical analysis is based on prime-age individuals not yet subject to retirement. Therefore, the sample is further restricted to individuals aged between 18 and 65 years according to the definition of the vertical collective agreement prevailing in the industry. Apart from these two requirements, only observations without complete demographic information in t' and t used for either for the matching algorithm or the outcome equation were dropped.

As the outcome variable, we use the logarithm of hourly wage constructed as the logarithm of the sum of monthly base wage, plus the regular and irregular components of the wage, payment indexed to tenure and overtime divided by normal and extra hours worked.[28] Hourly wage is preferable to monthly wage because workers from privatised and public firms experienced different length of hours of work after the reform.[29] In addition, wages were converted to real terms (1998 prices) using the Consumer Price Index (IPC). Table 2 and 3 display some selected characteristics of the treated and untreated (potential control) groups segmented by gender suitable for matching on each time period $t = 1$, 2, 3 and 4. Appendix describes the data more fully and gives other summary statistics for our 4 samples.

When the time elapsed from the reform is controlled for striking differences emerge. Looking at men (Table 2), the demographic variables indicate that the target treated group is slightly less educated and in contrast with the previous analysis (Table 1), has a significant lower fraction of full-time employees. Age and tenure are quite similar across the two groups with the exception of those employees that prevail the longest time period within the firms. In this case, target treated individuals tend to be older and more experienced than non target

[28] The hourly wage is constructed in the same way as the literature that uses the same dataset does. See, *inter alia*, Vieira (2001).

[29] See Appendix for summary statistics.

individuals. The difference in the payment level across groups mainly reflects the difference in human capital attainment: privatised employees are paid at lower hourly wage than the group of potential controls.

Inspection of women related figures (Table 3) shows a similar general picture. Target women are again less educated than in the control group and represent a substantially lower fraction of full time employees in the whole privatised firms set than in the control group. The major difference is that target women are slightly younger and less experienced than those working in the control group. Regarding the pay level outcome, the same pattern arises for women with target women earning less than non target women.

A naive way to evaluate the impact of the reform would consist in constructing a difference-in-differences estimator obtained by differencing the differences between target and non target groups in each of two time periods, $t = -1$ and t. The resulting difference-in-differences estimates (expressed in Table 4) though rough, confirm the *positive* relationship between wage variation and time period of restructuring for both men and women previously detected. Men (women) would have experienced wage cuts of 5 and 7 (1 and 11) percent after the first and second years post-reform which were followed up by gains of 9 (11) percent obtained after 4 years post reform.

The next issue concerns the selection of conditioning variables to be included in Z_i in order to estimate the propensity score. In the evaluation of the traditional *active labour market policies*, the selection of variables in the participation equation is easily conducted by the eligibility requirement rules of each programme.[30] In contrast, under privatisation

[30] Alternative methodological procedures have been used in the literature to specify the propensity score function. For example, Heckman *et al.* (1998a) use, among the variables suggested by the theory, only those that are significant at conventional levels and which increase the fraction of observations correctly predicted,

programme, *firms* and not workers, were selected to be privatised, and the time-ranking choice was based on objective firm performance indicators. This study assumes that the firm's performance is fully mirrored in the composition and observable quality of the workforce. This may be justified on the grounds that state enterprises tend to be overstaffed and pay excessive wages. Thus, finding individual(s) not under the reform similar to a given treated observation, will allow us to identify the privatisation effect on the treated.

The definition of the propensity score requires the inclusion of variables that can not be *potentially* changed by the programme itself. Given that, we concentrates upon time constant variables (such as, schooling, sex, region and privatisation date) and on time varying variables that were not affected by the programme itself (past experience and tenure). Firm attributes and variables that could be changed by privatisation (firm size, occupation, duration of work and part time status) were not included in the propensity score.[31] The inclusion of these variables was further restricted, whenever necessary, in order to comply with diagnostic tests for the propensity score, mainly the general misspecification test.[32] In addition, following the works of Eichler and Lechner (2002) or Heckman *et al.* (1998a), the monthly wage prior to entering the programme was extremely significant and whenever it increased the prediction rate it was included in the model.

Tables 5 reports the results of the probit regression of the propensity score for men and women, respectively, where the binary outcome takes the value 1 if the employee works in a privatised firm when t =1. Table 6 presents other specification tests for both propensity

while Eichler and Lechner (2002) adopt tests against heteroskedasticity, omitted variables, nonnormality and general misspecification to define the propensity function.

[31] As treated and control firms were very similar in terms of size, we did not include this variable in the propensity score.

[32] In two out of eight probit models estimated (for each gender and time period), the misspecification test required the inclusion of variables potentially changeable by privatisation.

scores.[33]

The estimation results show, unsurprisingly, that for both genders the conditional participation probability declines slightly with potential experience (age - schooling - 5) and increases with tenure. Employees with at least primary school have an increased probability of working in privatised firms. In particular, male or female employees with 6, 9, 14 or 16 years of schooling are clearly more likely to work in a privatised firm. The coefficients on the dummy variables for regions and privatisation dates reflect the location and size of the banks being privatised. Therefore, whereas living in Lisbon and North of the country increases the probability of working in a privatised firm, living in islands reduces it.[34] The coefficients on privatisation date are almost all negative, given that the years 1989 and 1994 correspond to the privatisation date of the four largest banks in the industry.[35]

For the actual matching estimation we also require that the pool of potential controls to which a given treated observation may be paired belong to the same year.[36] By matching within the year we remove explicitly any time specific unobservables not controlled for by the propensity score and avoid that each individual being matched with him(her)self. This is the matching analogy to the fixed effects. Also notice that including this variable (privatisation date) both in the propensity score and as additional matching variable amounts to increasing the weight of this variable when forming the matches.

Finally, for the *regression adjusted* matching versions, the outcome wage equation is

[33] We estimated a separate participation probit model according to each gender and period of time. In the Appendix we show the probit estimates and the specification tests for $t = 2, 3$, and 4.

[34] The variable region was excluded in the probit for women in order to comply with the misspecification test.

[35] The year 1990 is missing in our data and no privatisation took place in 1995

[36] It is not possible to match within the same region-year. More detailed information is provided in the Appendix.

adjusted by the conventional variables used in the literature. In particular, we follow Vieira (2001), including a vector of human capital variables, such as, education, experience and experience squared, tenure and tenure squared, duration of work and indicator variables aimed for controlling gender, part time status, occupation, region and privatisation calendar time.

5 Impact estimates

As discussed in Section 3, different matching schemes generate different estimates. This study adopts two extreme estimators in terms of neighbourhood size: the one-to-one nearest neighbour estimator with replacement and the gaussian kernel estimator.[37] Given the general strong similarity between the results obtained with both these two strategies, in what follows the nearest neighbour (NN) estimates are presented.[38] The NN matching estimator, relative to the gaussian estimator, has the advantage of being robust to the misweighted error due to choice-based sample scheme (see detailed discussion in Smith and Todd, 2000).

The NN matching strategy succeeds in reducing the variability of the observable attributes of both male and female workers in both groups. For instance, prior to matching, the estimated average propensity score for women working and not working in privatised firms during one year post-reform, were respectively, 0.46507 (standard deviation of 0.18510) and 0.24290 (0.19689). After matching, the propensity score for the control group became 0.46505 with the standard error of 0.18505.

Table 7 reports the impact of privatisation on the logarithm of hourly wage for men,

[37] Given that the size of the treatment group in some cases is smaller than the control group size, matching with replacement is the unique reasonable option.

[38] In occasional instances, whenever these two estimates diverge, the discrepancy will be explicitly noted.

over four different time periods, using four different matching strategies. In the first two rows, estimates from two versions of matching implemented in the context of cross section samples are presented: simple matching and regression adjusted matching. In the last two rows, these two same matching versions are reproduced under weaker assumptions using longitudinal data. The pre-programme period for each privatised firm is given by $t = -1$ while the post-programme is given by t ranging between one and four years. For example, the figure -.095 (first row, first column) indicates that during the first year post-reform, the wage paid to retained men in privatised firms grew 9.1 percent ($e^{-.095} - 1$) less than the wage paid to their respective counterparts in public firms.

The overall picture depicted in Table 7 and Figure 1, even though not reproducing exactly the same magnitude of the impacts, broadly confirms the dynamics of the treatment effects formerly identified and explained in Monteiro (2002). Retained workers from privatised firms initially experience wage growth losses (during the first two years post reform) corresponding to a firm's cost reducing strategy. After the third year post-reform, this pay strategy is reversed once the remaining maintained workforce has to be better paid in order to equate the wage level paid by public firms and thus reduce turnover.

The difference is that now, wage losses are *marginally* greater. For example, in the first year the wage cut hovers between .081 and .103 log points whereas before it was .092. After 2 years post-reform, the existent gap between both estimates remains the same. The wage loss now varies between .086 and .111 compared to the earlier estimate of .087. In particular, notice the remarkable ability of the regression adjusted difference-in-differences matching estimator in reproducing the same magnitude detected previously with the difference-in-differences estimator. During the period of (wage) recovery (four years after the introduction of the reform) an opposite pattern is found. All four matching strategies tend to underestimate the wage growth gain that is formerly identified. In fact, the previous gain of 0.082 is above of any matching estimates (ranging between -.048 and 0.077) produced.

A point worth noting concerns the performance of the four matching strategies implemented. In fact, two different patterns seem to emerge according to the time period of analysis. During the first two years, similar estimates in terms of magnitude and significance are obtained across the four matching versions. In contrast, considerable variation (in terms of both sign and magnitude) is found among the estimates produced by the two cross sectional matching versions for the latest time periods of analysis. This result possibly indicates the implausibility of the cross section matching assumptions for analysing long treatment effects. Recall that in the case of a pure random experiment, different methodological strategies would yield similar results within each time period. Therefore deviations are seen as indicators of the presence of frailty or unreliable assumptions.

Turning now to the women, their respective impacts are reported in Table 8. As we can infer from Table 8 or Figure 2, the overall pattern seems to deviate from the earlier findings. Indeed, the former apparent U-shaped pattern between wage loss and time of restructuring when using this specific control group, is now replaced by a positive relationship with the strongest and negative effects being felt during the first year after privatisation. As a result, the divergence in magnitude of the effects is now clearly pronounced. In the first year post-reform the wage loss suffered is now more intense than formerly identified (.062 log points) whereas after two years and in contrast to men, the intensity of the wage loss does decline to a level below the .115 previously detected. In terms of wage gains, the same male trend is observed with matching estimators underestimating the earlier estimate of .043. The major difference is that the longitudinal estimates loose their significance. Nevertheless the corresponding kernel estimates differ substantially reflecting a sparce overlapping level of the propensity score. In fact, the difference-in-differences kernel matching estimate is .060 while the respective regression adjusted difference-in-differences matching estimate is 0.011.

Finally, the dichotomy regarding the performance of the four matching strategies is also perceived. Again, the same instable and erratic pattern of cross sectional matching estimates is observed, suggesting that there is considerable selection on unobservables that is contaminating the longer privatisation cross matching impacts.

We turn now to the question of identifying sources of heterogeneity other than gender and timing for which privatisation effects are most prominent. Therefore, Table 9 and Table 10 report results obtained from one-to-one difference-in-differences matching estimates for men and women respectively, for different groups stratified according to age, tenure, education, occupation, full time status, position on the wage disribution and macroeconomic environment involving privatisation.[39]

It turns out that consideration of a single privatisation effect per time period masks a great deal the variation of privatisation effects. Nevertheless, although significant differences across and within skill groups arise from these two tables, a fairly similar trend can be detected for most skill groups *regardless* of the gender. In fact, the same positive relationship (sometimes resembling a U-shaped pattern) of wage adjustment previously identified, is now clearly exhibited for both men and women for the sub-categories of age, tenure, education, occupation, it is also present but less clear for employment status and economic cycle

[39] The following partition (into different subcategories) is somewhat arbitrary as we seek to have significant estimates within each sub-group.

breakdowns. The positive adjustment, though somewhat *wavy*, is also evident in different positions (quartiles) of the wage distribution for both genders. Figure 3 and Figure 4 help to uncover these trends.

Starting with age, the results seem to penalise relatively more the oldest employees of both genders. In fact, employees aged more than 50 years, experienced a significant wage cut (around 13 percent) during the first two years, clearly more pronounced than the average of the respective gender group within each time period. This is not particularly surprising as the prospect of this age-group for nearly retirement is significantly high. On the other hand, whereas compulsory wage promotions are defined for the initial years of the career (for low- and medium-paid occupations) by the wage agreement contract, these are optional for the latest years of the career and for the highest paid occupations within the firm. Hence, it is understandable that firms prefer to cut labour costs on the oldest individuals. This same reason explains why the highest wage increase occur for the youngest working group (both males and females).

Evidence on tenure subgroups also reflects this restricted firm's freedom to set wages for certain experienced groups. Individuals who remained the longest time within the firm suffered the highest wage losses over a longer time period. At the same time younger individuals enjoyed necessary the highest wage gains.

Looking at educational breakdowns, a surprising result is displayed. In contrast with our expectation, the best (and not the least) educated male and female employees are the most hit workforce, suffering sharp and lasting reductions (which are never reversed) in their relative wages in particular after two years of the implementation of the reform. A possible explanation for this finding might rely on the unknown size of the noncash component of compensation paid to this group. Given tax allowances firms might have preferred to reduce their relative wage and allow employees to still enjoy fat nonwage compensation, such as free car. In contrast, the wage gains are fairly similar across different educational groups and gender.

In what concerns occupational sub-categories, both Tables 9 and 10 indicate that privatisation eroded far more the relative wages of those employees (either men or women) in the highest (managers) and lowest (unskilled) occupations within the firm.[40] Yet in contrast with the latter group (in particular unskilled men) whose relative pay recovers significantly later on, the former group (both male and female in managerial/professional positions) incurs larger and longer lasting wage losses. Hence, despite the broad concept of managers used here, this result seems to contradict the positive prediction (from a variety of theories) of the impacts of privatisation on CEO pay level.[41] The reasoning for this finding is likely to be related to the downsizing strategy followed by privatised firms possibly implying a lower level of supervision and responsibilities for employees in this occupation.

The negative privatisation effects between part and full-time female employees differ mainly in terms of the timing of occurrence with wage cuts being more pronounced in the first year (two years) for the full-time (part-time) workers. On the other hand, wage gains are felt only by male part-time workers. The unexpected extreme wage jump for this group possibility arises from their insecure and risky position in the labour market before the reform took place. Therefore, the compensation of part-time workers could reflect their harder involvement (effort) in their job after the introduction of the reform. The significant

[40] These two occupations have been enjoying (at least in 1991) the widenest gap between negotiated and effective wages (see Aperta *et al.*, 1994).

[41] See Rosen (1992) or Wolfram (1998) for a theoretical and empirical survey on the executive pay.

reduction of the part-time employees ratio from 33% to 7.9% between t=-1 and t=4 may further reinforce this explanation.

The partition based on the position of the wage distribution summarises all previous effects. The most skilled and well-paid employees (both genders) before the reform, who were probably the most highly educated and experienced endured to a far greater degree the greater and longer-lasting losses, while the remaining workers were able to enjoy later some benefits from the reform. For women, however, the least skilled also experienced a wage loss similar to the most highly skilled during the first year.

Finally, note the unsurprising change on the wage adjustment pattern over recessive macroeconomic environments. In fact, for both genders, the positive relationship is now replaced by an inverted U-shaped wage adjustment pattern, reflecting more urgency on the restructuring process.

6 Concluding Remarks

The causal effect of privatisation on wages remains an important and controversial topic amongst policy-makers, economists and econometricians. Among policy-makers significant interest persists since the implementation of this reform has typically been contested by public opinion and labour unions. The usual resistance arises from the likely adoption of adverse contained labour cost strategies, including both displacement and wages reductions, despite the lack of empirical evidence supporting this last claim. For economists, additional interest results from the commonly *ambiguous* predictions proceeding from different theoretical approaches to the impact of privatisation on wages. Among econometricians, the discussion is centred on the *habitual* missing data problem inherent in the evaluation of causal effects in observational studies. In contrast with active labour market policies, privatisation has not been the target of a lively discussion from an evaluation standpoint and therefore deserves further scrutiny.

The purpose of this paper is to investigate the effects of privatisation on wages in the Portuguese banking industry. In particular, we were interested in testing if earlier findings on privatisation wage effects' are robust to the methodology selection. Following earlier analysis, the underlying complexity arising from employment adjustments is again simplified by strictly focusing on the wage effects on those employees who remained within the firm after the reform. One then implements the four variants of matching estimators to address the issue. Because we are interested in the effects over the course of different time periods (hovering between one and four years after privatisation) and because the data set used is rich enough to allow us to extract samples of different nature (both cross section and longitudinal) for each time period, we is also able to test the assumptions reasonableness' of different matching estimators in each time period.

In general, the results point to an *overall* confirmation of previous findings. Indeed, the same general positive relationship between wage variation and time of restructuring is again observed for both men and women. For women, however, this pattern deviates slightly from prior evidence, since formerly the strongest privatisation impact occurs after two years of privatisation. When the wage effects are broken down to account for the heterogeneity of the effects, a persistent positive pattern prevails irrespective of the gender. The evidence provided here also shows that the restructuring process hit more intensively the most educated employees. This surprising result, which contrasts with the conventional wisdom from the public/private wage literature, may imply that instead of education, seniority and experience are much more valuable and count for much in this particular labour market. On the other side, a dichotomous pattern regarding the performance of the four matching strategies emerges. Whereas in short term analysis, a clear similarity emanates across the four matching estimators, an instable and erratic pattern among *only* cross section matching estimates is observed in long term analysis. This result hence suggests that there is considerable selection on unobservables that is contaminating the longer privatisation cross matching impacts.

In terms of the policy agenda, the evidence gathered here has two important implications. First, privatisation seems to be a gender *neutral* policy given the strong similarity between the effects by gender either in terms of trend or intensity. Thus this result appears to contradict Gary Becker's prediction about the relationship between market structure and discrimination. Nevertheless, more research is clearly needed to assess if women are or not actually relatively worse off than men after privatisation. Second, the evidence presented so far also shows that the *wage cuts threat* so argued by labour unions to the remaining workforce is unfounded. Indeed, wage losses if they occur are only temporary as the long term dynamics presented here seem to be in favour of the law of one price in the labour market.

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Table 1: Employment and wage levels during the privatisation period

	1989	1991	1993	1995	1997
Average employment					
Public	7 323	6 771	6 812	6 793	6 856
Privatised	3 884	3 733	3 663	3 425	3 152
Full time status %					
Public	89.1	91.5	95.2	98.0	98.5
Privatised	83.3	98.5	96.3	98.4	98.7
Average of log of real hourly wage					
Public	7.22 (.328)	7.37 (.372)	7.46 (.360)	7.54 (.326)	7.49 (.328)
Privatised	7.12 (.354)	7.23 (.361)	7.36 (.411)	7.44 (.365)	7.45 (.368)
Number of unemployed people from the financial sector					
Women	1 400	3 400	5 800	8 600	8 700
Total	3 100	5 700	12 000	18 300	16 200

Source: Own computations based on Quadros de Pessoal, MSST (1989-1997) and INE (Inquérito ao Emprego - 4^{o}trimestre).

Figure 1: Matching estimates of the impact of privatisation on the hourly wage of men

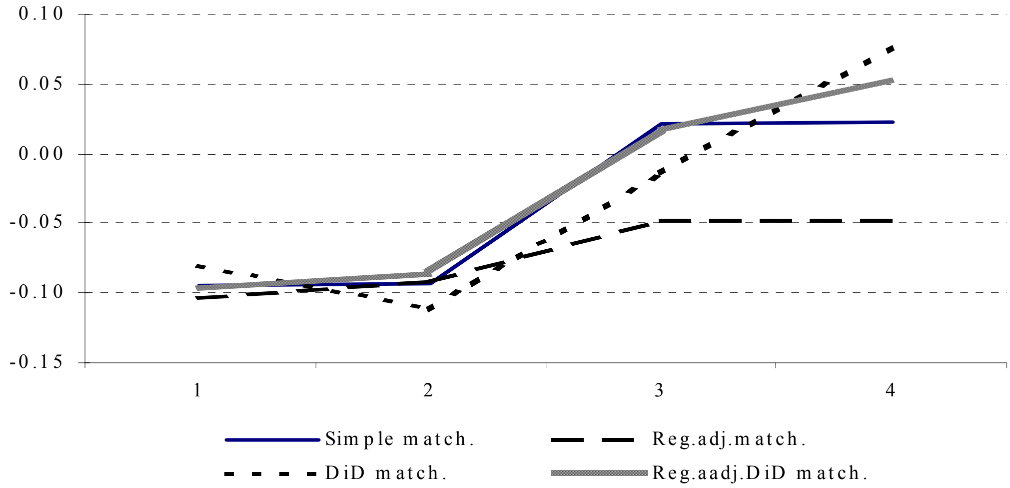

Figure 2: Matching estimates of the impact of privatisation on the hourly wage of women

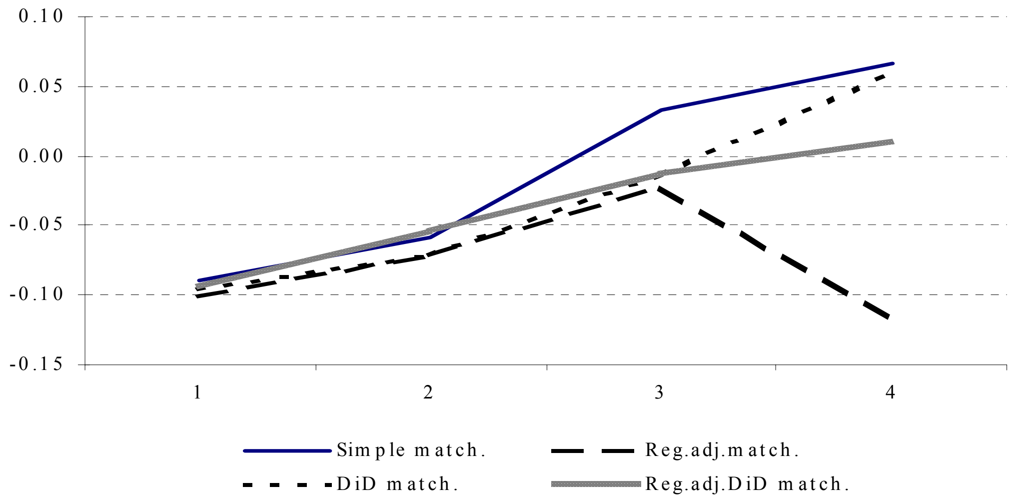

Figure 3: The impact of privatisation on the hourly wage by gender, across age, tenure, education and occupation groups

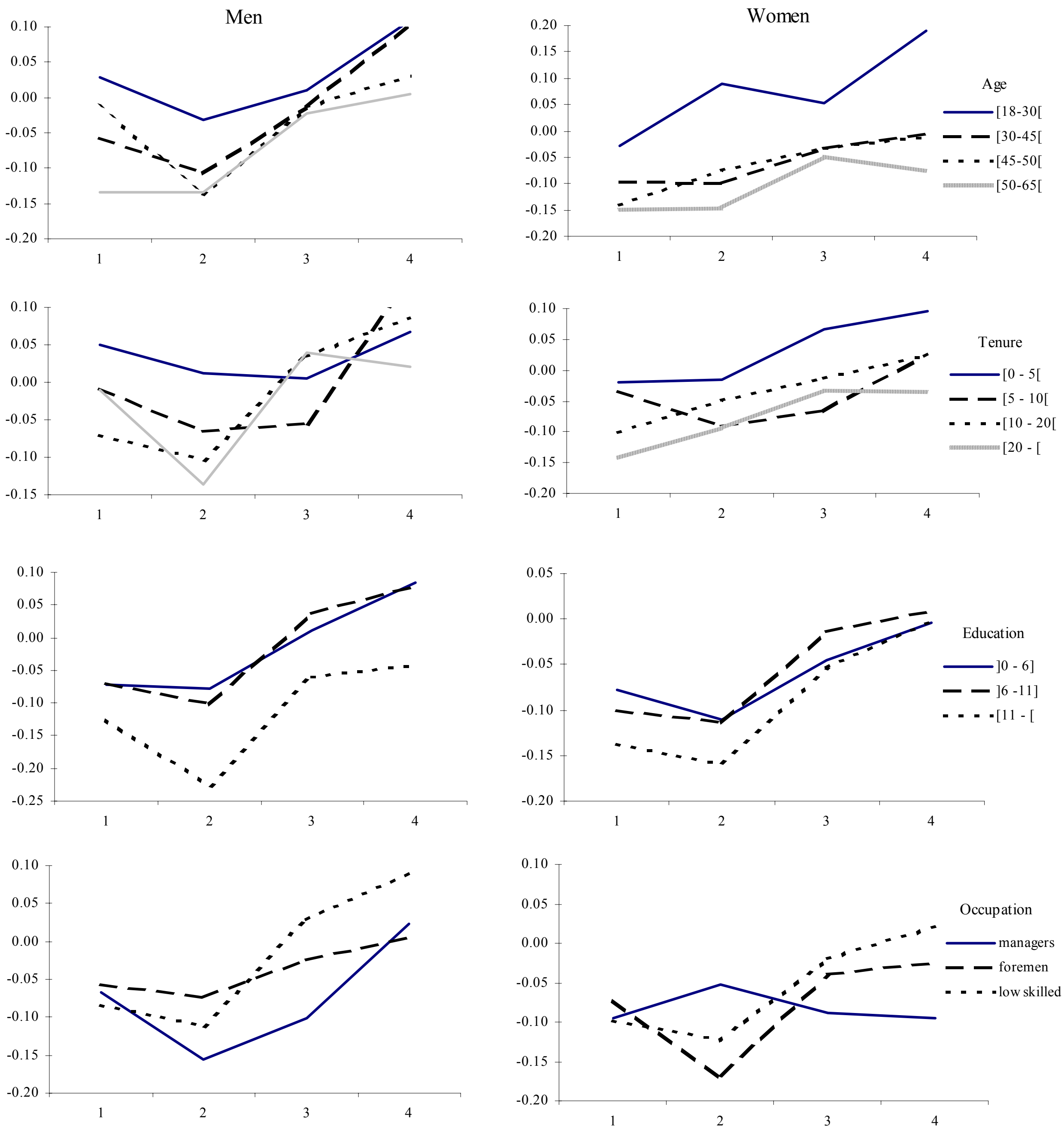

Figure 4: The impact of privatisation on the hourly wage by gender, across employment status, position in the wage distribution and economic cycle

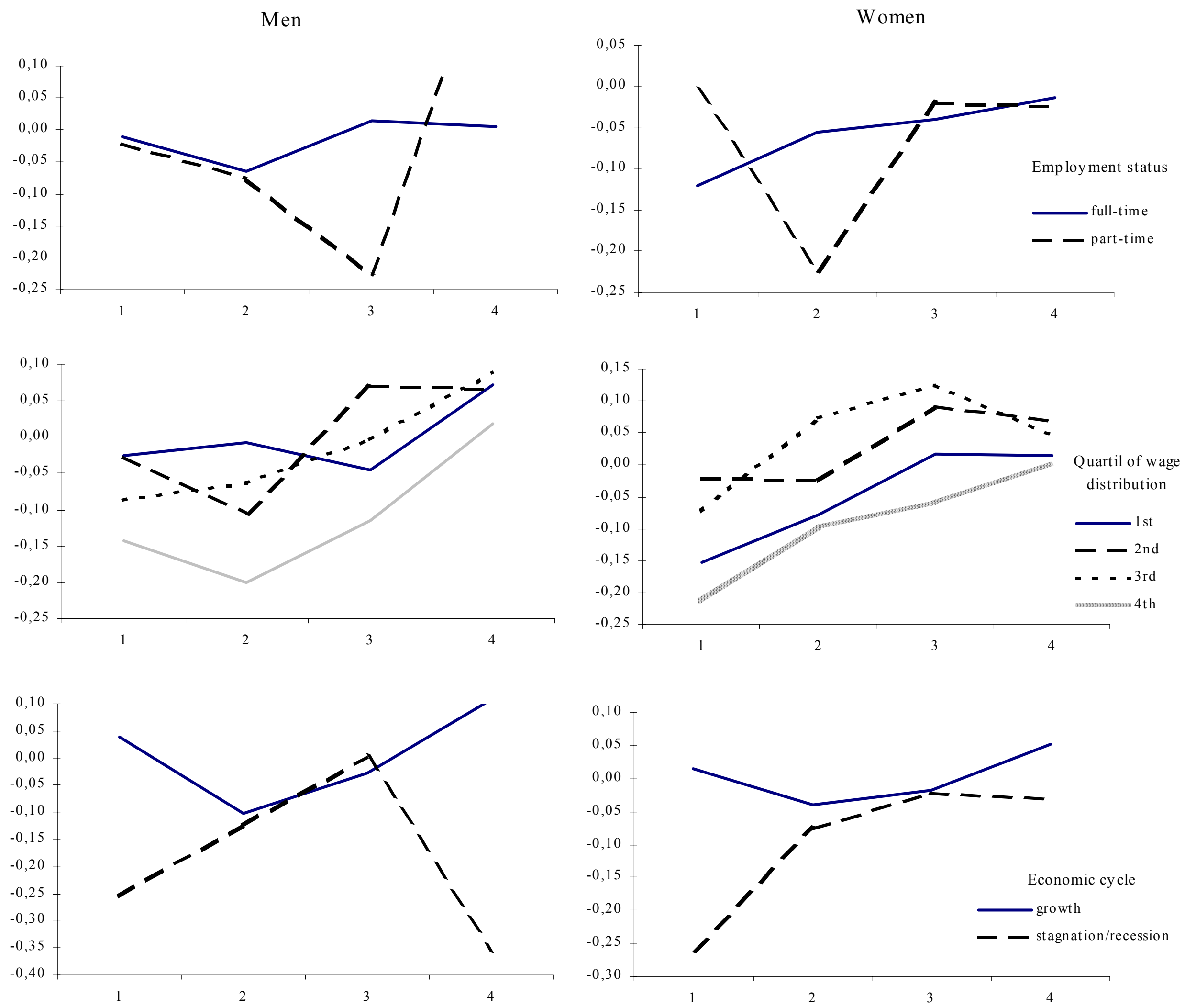

Table 2: Mean attributes for the potential control and treated male groups in time period t

	t = 1		t = 2		t = 3		t = 4	
	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.
Demographic variables*								
Age	43.4	43.5	43.4	43.0	42.6	42.6	39.7	40.9
Education	9.7	9.2	9.7	9.2	10.0	9.3	10.1	9.3
Tenure	16.6	16.6	17.2	16.3	16.6	15.9	12.9	14.5
% Full time	96.7	84.9	93.0	81.9	91.0	81.3	94.0	69.1
Hourly wage**								
t = -1	7.40	7.34	7.38	7.35	7.41	7.35	7.32	7.28
	(.35)	(.39)	(.34)	(.40)	(.35)	(.40)	(.33)	(.45)
t	7.50	7.39	7.48	7.37	7.54	7.43	7.49	7.55
	(.36)	(.36)	(.36)	(.34)	(.37)	(.30)	(.30)	(.38)
Sample Size	26,839	17,214	9,095	13,913	5,777	12,726	7,690	6,801

Source: Own computations based on QP, MSST (1989-1997).
Notes: * Computed at t = -1 for all samples. ** Standard deviation in parentheses.

Table 3: Mean attributes for the potential control and treated female groups in time period t

	t = 1		t = 2		t = 3		t = 4	
	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.
Demographic variables*								
Age	39.6	38.9	42.3	42.9	41.5	41.4	38.8	39.7
Education	9.4	9.0	9.5	9.1	9.8	9.2	10.0	9.2
Tenure	14.1	12.9	16.5	15.4	16.3	15.0	12.7	13.6
% Full time	90.3	80.0	90.6	80.7	88.7	80.3	91.2	67.0
Hourly wage**								
t = -1	7.23	7.12	7.18	7.14	7.23	7.17	7.18	7.08
	(.35)	(.35)	(.35)	(.36)	(.34)	(.35)	(.31)	(.37)
t	7.30	7.18	7.31	7.16	7.37	7.26	7.32	7.34
	(.35)	(.32)	(.36)	(.31)	(.36)	(.26)	(.35)	(.32)
Sample Size	13,738	6,236	5,150	4,967	3,710	4,486	4,814	2,151

Source: Own computations based on QP, MSST (1989-1997).
Notes: * Computed at t = -1 for all samples. ** Standard deviation in parentheses.

Table 4: Difference-in-differences estimates of the privatisation impact by gender

Groups*	time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Men		−.05 (.003)	−.07 (.004)	−.05 (.005)	.09 (.005)
Women		−.01 (.004)	−.10 (.006)	−.04 (.006)	.11 (.008)

Source: Own computations based on QP, MSST (1989-1997).
Note: * Standard error in parantheses.

Table 5: Results from the participation probit for men and women when t = 1

	Men		Women	
	Coefficient	Std. error	Coefficient	Std. error
Constant	3.25*	.419	-2.29*	.430
Tenure	.012*	.005	.051*	.008
Tenure^2	-.001*	.000	-.003*	.000
Experience	-.059*	.006	-.050*	.006
Experience^2	.001*	.000	.001*	.000
Education (yeas of schooling)$^{a)}$				
Primary (4)	1.871*	.307	.631*	.093
Preparatory (6)	2.060*	.309	.631*	.109
Lower secondary (9)	2.282*	.319	.976*	.121
Upper secondary (11)	1.860*	.315	.686*	.128
Upper secondary (12)	1.802*	.319	.666*	.147
Baccalaureate (14)	2.272*	.328	1.012*	.171
Baccalaureate (15)	1.682*	.418	.347	.399
University (16)	2.197*	.326	.868*	.160
University (17)	1.840*	.352	.585*	.282
Region				
Lisbon and Tagus valley	.381*	.017	-	-
Madeira and Azores	-.099*	.036	-	-
Privatisation date				
1991	-.824*	.024	-.212*	.032
1992	-1.158*	.030	-.658*	.038
1993	-.261*	.032	-.003	.045
1994	.318*	.028	.657*	.040
1996	-2.189*	.039	-1.769*	.061
log monthly wage at t = -1	.256*	.025	.218*	.038
LR chi-squared	13,305	.000**	5,095	.000**
Sample size	44,053		19,974	

Source: Own computations based on QP, MSST (1989-1997).
Notes: Reference group: education: none or less than 4 years of schooling, living in the north of the country and privatisation occurred in 1989. * denote significant at the 1 percent level.
** P-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatisation participation.
a) As a result of the extension of the high school from eleven to twelve years in 1987, the length of higher education varies.

Table 6: Other diagnostic tests for the propensity score when t = 1

Test for*	Men		Women	
	$\chi^2\,(df)$	P-value	$\chi^2\ (df)$	P-value
Non-normality	1 674.908	.000	6.894	.031
Incorrect functional form	4.475	.034	.013	.908
Fraction correctly predicted		73		74

*The score test suggested by Bera et al.(1984) tests normality against Pearson family distributions and the functional form test is a modified version of the RESET test of omitted/misspecification test.

Table 7: NN matching estimates of the impact of privatisation on log hourly wage of men

Time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Matching version				
Simple matching	−.095* (.007)	−.093* (.012)	.022 (.022)	.023* (.010)
Reg. adj. match.	−.103* (.005)	−.092* (.007)	−.048* (.012)	−.048* (.006)
DiD matching	−.081* (.005)	−.111* (.010)	−.014 (.018)	.077* (.009)
Reg. adj. DiD match.	−.096* (.005)	−.086* (.009)	.017*** (.013)	.053* (.008)
Treated sample size	17, 214	13, 913	12, 726	6, 801

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses.* and *** denote statistically significant from zero at the 1 and 10 percent levels.

Table 8: NN matching estimates of the impact of privatisation on log hourly wage of women

Time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Matching version				
Simple matching	−.089* (.010)	−.058* (.012)	.033* (.016)	.067*** (.044)
Reg. adj. match.	−.101* (.006)	−.071* (.008)	−.023** (.010)	−.062*** (.027)
DiD matching	−.096* (.008)	−.072* (.011)	−.014 (.018)	.033 (.034)
Reg. adj. DiD match.	−.093* (.008)	−.053* (.011)	−.012 (.013)	−.024 (.030)
Treated sample size	6, 236	4, 967	4, 486	2, 151

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses.*,** and *** denote statistically significant from zero at the 1, 5 and 10 percent levels.

Table 9: The impacts of privatisation on the log hourly wage of men

DiD matching	Time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Age					
[18 - 30[		.028	−.031	.010	.109*
		(.041)	(.082)	(.066)	(.042)
[30 - 45[		−.057*	−.106*	−.014	.104*
		(.007)	(.017)	(.026)	(.011)
[45 - 50[		−.010	−.140*	−.013	.030*
		(.011)	(.023)	(.029)	(.016)
[50 - 65]		−.134*	−.135*	−.023*	.004
		(.017)	(.024)	(.040)	(.030)
Tenure					
[0 - 5[		.050	.012	.005	.068
		(.037)	(.054)	(.048)	(.042)
[5 - 10[		−.009	−.065*	−.056	.138*
		(.013)	(.043)	(.051)	(.014)
[10 - 20[		−.070*	−.103*	.034	.086*
		(.008)	(.016)	(.024)	(.014)
[20 - [		−.010	−.137*	.039	.020
		(.012)	(.023)	(.038)	(.019)
Education (years of schooling)					
[0 - 6]		−.072*	−.078*	.011	.085*
		(.015)	(.016)	(.027)	(.019)
]6 -11[		−.070*	−.101*	.037	.079*
		(.007)	(.010)	(.029)	(.010)
[11 - [		−.126*	−.226*	−.060	−.043
		(.024)	(.037)	(.047)	(.039)
Occupation					
top and other managers		−.070*	−.169*	−.106*	.023
		(.014)	(.031)	(.052)	(.024)
foremen and supervisors		−.058*	−.076*	−.024	.005
		(.015)	(.024)	(.032)	(.030)
low skilled personnel		−.087*	−.118*	.028	.086*
		(.006)	(.012)	(.021)	(.009)
Employment status					
Full-time		−.010*	−.065	.015	.005
		(.005)	(.007)	(.012)	(.011)
Part-time		−.023	−.076	−.228	.314*
		(.057)	(.304)	(.247)	(.083)
Quartil of wage distribution					
1		−.026	−.008	.045**	.073*
		(.011)	(.019)	(.022)	(.029)
2		−.027	−.105*	.071	.067*
		(.010)	(.032)	(.074)	(.017)
3		−.086*	−.064*	−.003	.091*
		(.010)	(.022)	(.030)	(.010)
4 (best paid)		−.142*	−.200*	−.114*	.018
		(.018)	(.025)	(.033)	(.021)
Economic cycle					
Growth		.040*	−.101*	−.027	.107*
		(.006)	(.016)	(.029)	(.009)
Stagnation/recession		−.253*	−.124*	.001	−.354*
		(.010)	(.012)	(.015)	(.021)
Treated sample size		17,214	13,913	12,726	6,801

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses.*,** and *** denote statistically significant from zero at the 1, 5 and 10 percent levels.

Table 10: The impacts of privatisation on the log hourly wage of women

DiD matching	Time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Age					
[18 - 30[		−.028 (.025)	.090** (.053)	.052 (.071)	.190* (.046)
[30 - 45[		−.096* (.010)	−.100* (.017)	−.032** (.016)	−.007 (.017)
[45 - 50[		−.142* (.025)	−.075* (.027)	−.033 (.029)	−.010 (.025)
[50 - 65]		−.149* (.031)	−.141* (.044)	−.048 (.064)	−.074 (.049)
Tenure					
[0 - 5[		−.019 (.028)	−.016 (.050)	.065 (.050)	.091*** (.047)
[5 - 10[		−.034* (.013)	−.095* (.038)	−.067* (.029)	.027 (.039)
[10 - 20[		−.108* (.013)	−.050* (.018)	−.014 (.019)	.024 (.020)
[20 - [		−.153* (.019)	−.098* (.023)	−.033 (.024)	−.035 (.025)
Education (years of schooling)					
[0 - 6]		−078* (.016)	−.110* (.023)	−.045** (.022)	−.004 (.027)
]6 -11[		−.101* (.010)	−.114* (.015)	−.014 (.019)	.009 (.036)
[11 - [		−.138* (.031)	−.160* (.060)	−.053 (.053)	−.002 (.048)
Occupation					
top and other managers		−.095* (.036)	−.052 (.056)	−.088 (.060)	−.095* (.048)
foremen and supervisors		−.072* (.027)	−.171* (.038)	−.040 (.039)	−.025 (.052)
low skilled personnel		−.098* (.008)	−.123* (.016)	−.019 (.015)	.022 (.015)
Employment status					
Full-time		−.120* (.009)	−.056 (.011)	−.040* (.014)	−.013 (.014)
Part-time		−.005 (.029)	−.223** (.105)	−.021 (.071)	−.023 (.063)
Quartil of wage distribution					
1		−.152* (.015)	−.078* (.021)	.016 (.026)	.015 (.030)
2		−.021 (.014)	−.023 (.026)	.091* (.039)	.068** (.032)
3		−.071* (.017)	.074* (.038)	.126* (.035)	.045 (.102)
4 (best paid)		−.213* (.024)	−.096* (.030)	−.057*** (.032)	.008 (.022)
Economic cycle					
Growth		.015* (.008)	−.040* (.015)	−.018 (.023)	.052** (.024)
Stagnation/recession		−.260* (.015)	−.078* (.021)	−.022 (.013)	−.032* (.013)
Treated sample size		6,236	4,967	4,486	2,152

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses.*,** and *** denote statistically significant from zero at the 1, 5 and 10 percent levels.