Using propensity matching estimators to evaluate the impact of privatization on wages

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Whether the transfer of ownership rights to the private sector leads to a decline or increase in wage growth is theoretically ambiguous, given that the outcome depends on the uncertain interaction between firms and workers. Using propensity matching techniques, this article investigates the effects of privatization on wages in the Portuguese banking industry. The empirical results, obtained from Quadros de Pessoal for the period between 1989 and 1997, generally show a negative (positive) short-run (long-run) effect of privatization on relative wage growth for both men and women retained in the privatized firms. Moreover, the results show that the most educated and experienced (oldest) workers, as well as those in the high skill occupational categories, were more likely to experience a negative wage effect.

I. Introduction

Despite the large and prolific literature on privatization, the analysis of the causal effect of privatization on wages remains fairly neglected.¹ This is somewhat surprising, since the transfer of ownership rights to the private sector has been perhaps the most important structural reform, introduced worldwide, in the increasing use of markets to allocate resources.² More importantly, its implementation has frequently been met with fierce resistance from both labour unions and local communities, and has attracted intensive press attention. Whilst policy-makers tend to advocate gains in terms of firms’ 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 relative lack of empirical research on this controversial topic merely reflects the unavailability of appropriate data. Typical research on privatization uses data from firms’ annual accountancy reports, which, at best, contain crude labour force information.

At the theoretical level, the relationship between privatization and labour market outcomes is not obvious; privatization does not necessarily cut jobs or lower wages. Employment and wages may decline as privatization implies a shift in the public firms’ objective function towards profit maximization, which affects the outcome of wage bargaining (Haskel and Szymanski, 1992, 1993). However, if workers are willing to put in more effort after

² Megginson et al. (1994) provide an excellent historical overview of postwar privatizations in developed countries. For a study of privatization effects in a large number of developing countries, see Al-Obaidan (2002).
privatization, then firms may settle for higher wages (e.g. De Fraja, 1993; Haskel and Sanchis, 1995; Goerke, 1998). Similarly, if new ownership brings fresh capital and expertise, such changes are likely to generate growth and job creation.

The present article contributes to this discussion by implementing a variety of increasingly popular nonexperimental methods, labelled propensity matching estimators, to assess the impact of privatization on wages. In particular, this study re-visits the effects of privatization 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’ (OECD, 1999, p. 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 randomized experiment.

The present study is empirically fruitful for several reasons. First, apart from the remarkable success of the above mentioned policy in the banking sector, the design of the privatization program in this industry provides a promising opportunity for examining the effects of a change in ownership. Indeed, not only did privatization not affect all public firms (there is still a large state-owned group), but it was also implemented continuously over 8 years. Hence, this partial and ongoing privatization 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 privatization. In fact, as privatization is likely to cause disproportionate changes in the composition of the workforce in privatized firms (compared with public firms), we would prefer a strategy that is robust to this unequal employment composition variation. By pairing each program participant, according to observable attributes, with members of a comparison (nontreated) group, matching leads – in principle – to ex post re-establishment of the conditions of an experiment. Therefore, this effect is naturally controlled for. Besides, matching is a flexible approach that avoids definition of a specific form for either the outcome equation, decision process or the unobservable term.

Furthermore, 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 has been recently extended not only to new multiple matching schemes, but also to the case of repeated cross-sectional or longitudinal data. These new modified versions, which will be described below, are less restrictive in assumptions and can thus produce more accurate estimates. 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 privatization.

Finally, this class of estimators has heavy data requirements since the quality of matching estimates mirrors the quality/quantity of the variables employed. This article uses data from 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 privatization. Hence, it allows us to draw samples of different nature (cross-section and longitudinal) and implement 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 article is structured as follows. Section II discusses briefly the main features of the privatization process and the labour relations prevailing in the Portuguese banking sector. Section III presents an overview of the assumptions and variety of the matching estimators. The data implementation issues are addressed in Section IV. Section V outlines and discusses the empirical results. Section VI closes the article, summarizing the main lessons of this study.

II. Privatization and the Portuguese Banking Labour Market

The privatization program was introduced in the banking sector as a further step in the successful

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3 For details, see the original papers of Heckman et al. (1997, 1998a) or the discussions in, e.g. Smith and Todd (2005) and Blundell and Dias (2000).
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reform of the Portuguese financial system (OECD, 1999). This structural reform, starting in 1984, aimed to put an end to the heavily regulated and nationalized system imposed in the industry after the 25th 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 privatization program was then implemented.

The first privatization law adopted in 1988 (law 84/88 from 20th July) allowed merely partial privatization of public enterprises as the state still retained 51% of the equity. For this first phase of privatization, 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 5th April) was passed, allowing full privatization of enterprises nationalized after 1974. The privatization program was assumed to be an important mechanism for (1) improving the deteriorated performance of public economic units, (2) modernizing 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 privatized were first transformed into corporations, with a prior evaluation being made by two independent entities. However, in contrast with some other economic sectors (e.g. electricity and telecommunications), the government opted for a policy of no interference in the public firms during the period before privatization (Naumann, 1995; Sousa and Cruz, 1995), leaving the economic restructuring for future private owners. In terms of scheduled order of privatization, apart from those firms which were selected on grounds of performance indicators for the partial privatization phase (OECD, 1989), there was no set schedule for subsequent firms’ privatization (OECD, 1991). Instead, the timetable was strongly affected by the economic and political domestic cycles, and by the international context.

By mid-1997, 10 out of 12 public banks became fully private: two banks were privatized in 1991, three in 1994, and each of the five remaining banks were privatized in 1989, 1990, 1992, 1993 and 1996, respectively.5,6 The most common privatization procedure used, was public offer, and, to a much lesser 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 privatized firm at preferential rates). In most cases, ownership returned to former Portuguese groups, which owned them prior to the nationalization wave in 1974.7 Due to this private–public–private ownership path, privatization in Portugal is termed re-privatization.

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 operating/staff costs (from 1.53% of average assets in 1991 to 0.98% 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 privatized institutions.8

The developments at the ownership level conditioned the type of industrial labour relations prevailing in the industry, which are unique to Portugal. Covering three different geographical areas, the oldest labour unions in the mainland represent all employees in the bargaining process, regardless the ownership of the bank. Indeed, trade unions and a group of banks (employers), public and private (domestic and foreign), meet each year to negotiate the vertical collective bargaining agreement. This collective agreement, the most detailed and extensive in Portugal, regulates the employment conditions, the remuneration and the duration of work. In particular, it delimits the starting wage level and the compulsory wage progressions for each of its 18 levels of the 4 groups defined to cover the entire banking workforce.

Beyond this broad scope of the collective agreement, banking unions also enjoy the strongest

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

5 This total number (10) of firms privatized in the banking industry does not coincide with the 11 privatized firms reported by the OECD 1999 survey, due to the absence of one bank in the data.

6 According to the privatization literature, the date of the first tranche sale of each firm is considered the date of effective privatization.

7 International investors could buy a limited share of the equity, ranging from 2 to 40% of sales.

8 A contrary conclusion is reached by Kraft et al. (2006), with data from the Croatian banking sector, showing that privatizations did not have an immediate effect on improved efficiency.
attachment in the economy. Indeed, the unionization density has expanded markedly between the periods 1974–78 and 1991–95, from 71 to 106% (Cerdeira, 1997).9 Despite this notable reinforcement, unions did not act against privatization. The resistance offered was very limited, not coordinated and mostly being made through internal speeches which were rarely reported in the national press. An interesting indicator of the tranquility is the total absence of any strike action during the privatization reform.10 However, unions improved their relative negotiated wage growth during the privatization period. Indeed, during the period 1989–97, unions in the banking industry (rest of the economy) obtained an average annual growth rate of negotiated wages of 7.7% (8.3%), while in the pre-reform period (1981–88) they obtained 16.2% (17.4%).11 A priori, the coordinated bargaining system should bring uniform wage levels across firms within the banking sector, although the positive differential between the wage defined by the collective agreement and the actually paid wage has widened since, the early 90s (Aperta et al., 1994).

In terms of labour outcomes, the main economic restructuring adjustments are illustrated in Table 1.12 For comparison purposes, the public category refers to the two permanent public banks, whereas the privatized category includes the 10 firms being privatized. In contrast to public firms, whose level of employment remained fairly constant from 1991 onwards, the level of employment in privatized firms dropped steadily during the reform period. Each privatized firm lost on average 732 employees between 1989 and 1997 (implying a 23% rate of overstaffing), which corresponds to a loss of 92 employees per firm/year during the same period. Nevertheless, this downsizing of employment is accompanied by a significant increase in the total working hours and in the share of permanent full time workers, more notable in privatized than in public firms.13

The trend in the banking workers’ wages is also clear, both public and privatized 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 privatized firms’ workers, however, the wage increase is clearly more pronounced than for public employees. At first glance, this suggests a positive privatization impact on the wage level. Between 1989 and 1997, privatized employees enjoyed a wage increase of 40% whilst public employees enjoyed a wage gain of 30%.14 This convergence in payment level is particularly notable as important dissimilarities in terms of human capital attainments, present already in 1989 (before privatization), became more evident after privatization took place. Employees in privatized firms, even after the reform, are the least educated, the oldest and the most experienced in the banking sector. On the other side, the rise in the wage dispersion in privatized firms, when measured by the standard deviation (SD) of hourly wage, may suggest heterogeneous privatization 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 reform in each firm, which possibly mitigates dynamic privatization effects.

### III. Econometric Considerations

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

To be precise, let us state formally this causal effect. Denote by $W_i$ and $W_0$ the wage paid to an individual $i$ (outcome or variable response) conditional on the presence and absence of treatment (privatization), respectively. $D_i$ is a participation

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9 The unionization density includes unionized, both active and retired, employees.
10 Source: Greves Anual, Informação Estatística (Síntese), various years, MSST.
12 Unit of currency = escudos (PTE). 1 Euro = 200.482 PTE
13 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.
14 The T-test for the estimated wage difference between the treatment and the control group is statistically significant at the 1% level.
variable that identifies whether employee \( i \) received ‘treatment’, i.e. whether he/she was employed in a firm that was privatized (\( D_i = 1 \)) or not (\( D_i = 0 \)). Finally, \( X_i \) represents, for each individual \( i \), a set of attribute 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, \( \frac{W_i}{C_1} = \frac{W_i}{C_0} \), as the observed wage for an employee \( i \) is given by \( W_i = W_i^0 + D_i(W_i^1 - W_i^0) \), with only one of \( W_i^0 \) and \( W_i^1 \) being observed at any given point in time.\(^{15}\) For all those individuals treated, one is interested in estimating the most common parameter in the evaluation literature, \( E(W_i^1 - W_i^0 | D_i = 1, X_i) \), 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 \( E(W_{i0} | D_i = 1, X_i) = E(W_{i0} | D_i = 0, X_i) \). In contrast, in observational studies, assignment is not random, resulting either from individual self-sorting, selection made by a program manager, or both.

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 (\( W_i^0, W_i^1 \)). In formal notation, this assumption corresponds to

\[ (W_{i0}, W_{i1}) \perp D_i | X_i, \]

where \( \perp \) denotes independence.\(^{16}\) This means that, given \( X_i \), one can use nonparticipants’ wages to approximate the (counterfactual) wage level of participants who have not participated. Hence, matching consists of finding, for each treated

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\(^{15}\)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.

\(^{16}\)‘Ignorable treatment assignment’, in the terminology of Rubin (1977) and Rosenbaum and Rubin (1983).
observation, a set of nontreated observations with the same realization 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 cannot condition), that affect both the nontreated outcome ($W_{i0}$) and the assignment ($D_i$). If this were the case, then selection would be on unobservables.

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, $P(X_i) = \Pr(D_i = 1|X_i)$, is sufficient to balance the covariates $X_i$ between the treatment and control units. Therefore, if CIA holds conditional on $X_i$, it will also hold conditional on the propensity score,

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

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

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

To satisfy this condition, there must be both participants and nonparticipants 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 nontreated 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.

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, and the bias resulting from having different distributions of $X_i$ across their common support. The remaining source of bias, differences on unobservables across groups, are ruled out by the matching assumptions.

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

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

where $N_{ij}$ controls for the weight placed on each comparison observation $j$ for individual $i$. $n_i$ 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 of a closeness criterion, a neighbourhood, and the selection of an appropriate weight function to associate the set of nontreated observations to each participant. For instance, the neighbourhood may range from a singleton set to a multiple set, eventually including all nontreated observations. 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 raise the computational burden. For 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 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 nontreated unit may improve matching quality (reducing the bias), but increases the variance.

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 based on bias stability condition, using the terminology of Eichler and Lechner (2002) 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 nontreated units eliminates the bias. Let $t$ and $t'$ denote, respectively, a point in time after and
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19 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 organizations are not covered by this survey.

In order to avoid these potential problems, this study focuses only the effect(s) of privatization on the wages for those employees that remained in the same firm after its privatization. Therefore, our treated units (employees) correspond to all individuals that work in each public firm subject to privatization 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 privatization of a given public firm. Thus, the treated group includes all individuals that work both in \( t' \) and \( t \) for the firm being privatized. The corresponding control (nontreated) group is composed of those workers employed in the remaining public firms (not subject to privatization) 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, we follow the spirit in the evaluation of active labour markets, in which only individuals with common labour market histories (employment) are matched.

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 recognized 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 nonrandom sample from the entire labour force. Because we are using information from the remaining

**IV. 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 80s. It consists of a matched employer–employee database containing a high number of variables/concepts that meet international standards about each unit, firm or employee, observed. For instance, for each firm the data gives the location, level of employment, economic activity, type of management and total sales. Similarly, for each employee, usual human capital variables, such as gender, level of schooling, tenure, occupation, full-time/part-time status, earnings, length of working time 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 nontreated units. As the direct target of the privatization program is the firm itself and not the employees, one would ideally like to evaluate the privatization impact on those employees that either remained, joined or left the firm after its privatization.19 In this case, the active labour market policies, in which both the policy and evaluation object targets coincide.

Variables relating labour force status of treated individuals were found to be very significant (even more than earnings) in explaining the participation decision in training programs.
public employees within the same industry for appraising the effects of privatization, this unobservable component, 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 the analysis is to compute the overall impact of privatization in the banking sector, not firm-by-firm effects. Consequently, the 10 firm privatizations need to be condensed into one ‘single privatization’. The creation of the data sample for estimation is a two-step procedure. In the first step, for each privatized firm, we assign two points in time: one pre-treatment \( t \) and one post-treatment \( t' \). The respective treated and nontreated individuals are then extracted. The choices of \( t' \) and \( t \) are driven by economic considerations. Because the firms’ process of restructuring occurred mainly after the implementation of the reform, as referred to in Section II, \( t' \) consists of a single calendar year prior to privatization. In particular, the conventional procedure of the privatization literature is followed, considering the calendar year of each firm privatization, the year 0. Therefore \( t' = -1 \), corresponds to the calendar year prior to each privatization date. In contrast, for the post-treatment period, we allow privatization effects to vary over time, following the discussion of Gupta et al. (2001). The post-treatment period ranges between 1 and 4 years, \( t = 1, 2, 3 \) and 4, corresponding either to one, two, three or four calendar years after each privatization date.\(^{21}\) The second step consists of aggregating, in each \( t' \) and \( t \) points in time, all treated and nontreated individuals of the respective privatized firms, 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 nontreated and treated at 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 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. Hourly wage is preferable to monthly wage because, workers from privatized and public firms experienced different length of working time during the reform. In addition, wages were converted to real terms (1998 prices) using the Consumer Price Index (IPC). Tables 2 and 3 display some selected characteristics of the treated and nontreated (potential control) groups segmented by gender, suitable for matching in each time period.

When the time dimension is controlled for, a striking difference emerges. Considering men (Table 2), the demographic variables indicate that the treated individuals have the same age and experience as the individuals in the control group, are less educated and work more hours. Furthermore, the treated individuals enjoy on average a much longer period without being promoted (5 years) and have a significantly lower fraction of full-time workers than workers in public banks. The only exception is those employees who stay longer within the firm, who tend to be older and more experienced, spend the same time working and are promoted at the same pace as nontreated individuals. Occupational position within the firm reflects differences in the educational level of workers. Therefore, there are a slightly larger proportion of treated individuals in low skilled occupations. Once more, the exception is those employees who remain for the longest time within the firm, for whom occupational position reflects seniority instead of educational level. The geographical distribution indicates that the bulk of workers/firms are located in Lisbon. Finally, the difference in the payment level before privatizations mainly reproduces differences in human capital attainment across groups, privatized employees are paid a lower hourly wage than the group of potential controls.

The corresponding figures for women (Table 3) show a very similar picture. Treated women are again less educated, spend more time working, are promoted much less frequently than nontreated women, and have a substantially lower fraction of full time employees. The major difference is that treated women are slightly younger and less experienced than those of the control group. The occupational distribution indicates that treated women have a slightly lower share in the high-skill category. The exception occurs again for those women who stay for longest time period within the firm. Regarding pay levels, the previous pattern applies also for women,\(^{21}\) This post-treatment period choice is also conditioned by the first merger wave in 1998 in the banking industry, which involved recently privatized firms.
Table 2. Mean attributes for the potential control and treated male groups in time $t$

<table>
<thead>
<tr>
<th>Demographic variables*</th>
<th>$t = 1$</th>
<th>$t = 2$</th>
<th>$t = 3$</th>
<th>$t = 4$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>43.4</td>
<td>43.5</td>
<td>43.0</td>
<td>42.6</td>
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<td></td>
<td>42.6</td>
<td>39.7</td>
<td>40.9</td>
<td></td>
</tr>
<tr>
<td>Tenure</td>
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<td>16.6</td>
<td>16.3</td>
<td>15.9</td>
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<tr>
<td></td>
<td>16.6</td>
<td>12.9</td>
<td>14.5</td>
<td></td>
</tr>
<tr>
<td>Potential experience</td>
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<td>27.8</td>
<td>27.3</td>
</tr>
<tr>
<td></td>
<td>26.6</td>
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</tr>
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<td></td>
<td>10.6</td>
<td>10.7</td>
<td>9.3</td>
<td></td>
</tr>
<tr>
<td>Total working hours</td>
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<td>148.3</td>
<td>141.8</td>
<td>147.3</td>
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<td>per month</td>
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<td>142.4</td>
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</tr>
<tr>
<td>Number of months</td>
<td>30.5</td>
<td>57.9</td>
<td>28.8</td>
<td>61.0</td>
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<tr>
<td>since last promotion</td>
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<td>25.5</td>
<td>61.3</td>
<td>24.0</td>
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<td>Full-time status (%)</td>
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<td></td>
<td>91.0</td>
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</tr>
<tr>
<td>Occupation (%)</td>
<td>30.1</td>
<td>28.9</td>
<td>31.3</td>
<td>32.4</td>
</tr>
<tr>
<td></td>
<td>27.8</td>
<td>24.0</td>
<td>29.6</td>
<td></td>
</tr>
<tr>
<td>High skilled</td>
<td>69.9</td>
<td>71.1</td>
<td>68.7</td>
<td>71.8</td>
</tr>
<tr>
<td>Low skilled</td>
<td>67.6</td>
<td>72.2</td>
<td>76.0</td>
<td>70.4</td>
</tr>
<tr>
<td>Region (%)</td>
<td>24.7</td>
<td>21.8</td>
<td>24.2</td>
<td>23.9</td>
</tr>
<tr>
<td>North</td>
<td></td>
<td></td>
<td>0.2</td>
<td>23.6</td>
</tr>
<tr>
<td>Lisbon and Tagus Valley</td>
<td>70.4</td>
<td>76.3</td>
<td>65.5</td>
<td>76.4</td>
</tr>
<tr>
<td>Madeira and Azores</td>
<td>4.9</td>
<td>1.9</td>
<td>10.3</td>
<td>10.4</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>3.9</td>
</tr>
<tr>
<td>Real hourly wage**</td>
<td>1750 (850)</td>
<td>1683 (869)</td>
<td>1720 (901)</td>
<td>1698 (879)</td>
</tr>
<tr>
<td>$t = -1$</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Logarithm of real hourly wage**</td>
<td>7.40 (0.345)</td>
<td>7.34 (0.392)</td>
<td>7.38 (0.338)</td>
<td>7.35 (0.397)</td>
</tr>
<tr>
<td>$t = -1$</td>
<td>7.50 (0.361)</td>
<td>7.39 (0.360)</td>
<td>7.48 (0.361)</td>
<td>7.37 (0.343)</td>
</tr>
</tbody>
</table>
| Source: Own computations based on Quadros de Pessoal, MSST (1989–1997).
Notes: * Computed at $t = -1$ for all samples. ** SD in parentheses.
Table 3. Mean attributes for the potential control and treated female groups in time $t$

<table>
<thead>
<tr>
<th>Demographic variables*</th>
<th>Control</th>
<th>Treatment</th>
<th>Control</th>
<th>Treatment</th>
<th>Control</th>
<th>Treatment</th>
<th>Control</th>
<th>Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>39.6</td>
<td>38.9</td>
<td>40.2</td>
<td>42.9</td>
<td>39.8</td>
<td>38.0</td>
<td>37.3</td>
<td>36.0</td>
</tr>
<tr>
<td>Tenure</td>
<td>14.1</td>
<td>12.9</td>
<td>15.4</td>
<td>15.4</td>
<td>15.7</td>
<td>12.5</td>
<td>12.4</td>
<td>10.8</td>
</tr>
<tr>
<td>Potential experience</td>
<td>24.2</td>
<td>23.9</td>
<td>25.2</td>
<td>23.6</td>
<td>24.3</td>
<td>22.8</td>
<td>21.6</td>
<td>20.8</td>
</tr>
<tr>
<td>Education</td>
<td>9.7</td>
<td>9.3</td>
<td>9.6</td>
<td>9.1</td>
<td>10.0</td>
<td>9.7</td>
<td>10.3</td>
<td>9.6</td>
</tr>
<tr>
<td>Total working hours per month</td>
<td>139.8</td>
<td>144.5</td>
<td>137.9</td>
<td>144.1</td>
<td>138.6</td>
<td>144.6</td>
<td>139.5</td>
<td>140.8</td>
</tr>
<tr>
<td>Number of months since last promotion</td>
<td>31.1</td>
<td>56.0</td>
<td>30.7</td>
<td>61.1</td>
<td>27.4</td>
<td>61.5</td>
<td>25.3</td>
<td>27.7</td>
</tr>
<tr>
<td>Full-time status (%)</td>
<td>90.3</td>
<td>80.0</td>
<td>81.2</td>
<td>77.2</td>
<td>85.1</td>
<td>77.5</td>
<td>86.7</td>
<td>60.2</td>
</tr>
<tr>
<td>Occupation (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High skilled</td>
<td>12.7</td>
<td>12.3</td>
<td>12.8</td>
<td>12.0</td>
<td>14.1</td>
<td>12.7</td>
<td>9.7</td>
<td>14.3</td>
</tr>
<tr>
<td>Low skilled</td>
<td>87.3</td>
<td>87.7</td>
<td>87.2</td>
<td>88.0</td>
<td>85.9</td>
<td>87.3</td>
<td>90.3</td>
<td>85.7</td>
</tr>
<tr>
<td>Region (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>North</td>
<td>20.5</td>
<td>23.7</td>
<td>23.1</td>
<td>28.1</td>
<td>3.5</td>
<td>28.47</td>
<td>–</td>
<td>6.5</td>
</tr>
<tr>
<td>Lisbon and Tagus Valley</td>
<td>76.0</td>
<td>74.5</td>
<td>70.4</td>
<td>71.9</td>
<td>90.8</td>
<td>71.5</td>
<td>97.7</td>
<td>93.5</td>
</tr>
<tr>
<td>Madeira and Azores</td>
<td>3.5</td>
<td>1.9</td>
<td>6.5</td>
<td>5.7</td>
<td>2.3</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Real hourly wage**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$t = -1$</td>
<td>1476 (618)</td>
<td>1326 (629)</td>
<td>1404 (572)</td>
<td>1347 (553)</td>
<td>1470 (574)</td>
<td>1384 (558)</td>
<td>1377 (513)</td>
<td>1280 (591)</td>
</tr>
<tr>
<td>$t$</td>
<td>1578 (822)</td>
<td>1388 (629)</td>
<td>1613 (1134)</td>
<td>1359 (480)</td>
<td>1717 (1274)</td>
<td>1474 (455)</td>
<td>1606 (584)</td>
<td>1629 (700)</td>
</tr>
<tr>
<td>Logarithm of real hourly wage**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$t = -1$</td>
<td>7.23 (0.353)</td>
<td>7.12 (0.353)</td>
<td>7.18 (0.346)</td>
<td>7.14 (0.358)</td>
<td>7.23 (0.338)</td>
<td>7.17 (0.346)</td>
<td>7.18 (0.314)</td>
<td>7.08 (0.370)</td>
</tr>
<tr>
<td>$t$</td>
<td>7.30 (0.345)</td>
<td>7.18 (0.320)</td>
<td>7.31 (0.361)</td>
<td>7.16 (0.309)</td>
<td>7.37 (0.363)</td>
<td>7.26 (0.260)</td>
<td>7.32 (0.348)</td>
<td>7.34 (0.322)</td>
</tr>
</tbody>
</table>

Source: Own computations based on Quadros de Pessoal, MSST (1989–1997).
Notes: * Computed at $t = -1$ for all samples. ** SD in parentheses.
with treated women earning less than nontreated women. The next issue concerns the selection of conditioning variables to be included in $X_t$, 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 program. In contrast, under the privatization program, firms, not workers, were selected to be privatized. As mentioned previously, this study assumes that the firm’s performance is fully mirrored in the composition and observable quality of the workforce, which is consistent with the well established public–private wage differential literature (e.g. Katz and Krueger, 1991; Disney and Gosling, 1998). Therefore, we include all time constant and time varying attributes of individuals that were not affected by the privatization reform, such as, schooling, privatization date, past experience, tenure, occupation and time elapsed since last promotion. For various reasons, the following three variables were not included in our specifications: total working time, location and full-time status. Total working time is our outcome variable, since we compute logarithm of hourly wage, while location reflects the region where the head office of the bank is located instead of the actual location of the bank or branch. The inclusion of the variable full-time status violates the assumption (3), as we obtain perfect prediction of being employed in a privatized firm, and destroys the balancing of the variables after matching.

Table 4 reports the results of the probit regression for the propensity score correspondent to Equation 3 presented in the previous section. We estimate two propensity scores, for men and women respectively, where the binary outcome takes the value 1 if the employee works in a privatized firm when $t = 1$. In total, we estimate eight sets of scores, according to each gender and period of time. Tables 9–11 in the appendix show the probit estimates for $t = 2, 3$ and 4. Table 12 (also in the appendix) summarizes the propensity score obtained for each treatment and control group, across gender and period of time.

The estimation results show, unsurprisingly, that for both genders the conditional participation probability increases with tenure and declines slightly with potential experience (age-schooling – 5). Employees with at least primary school have an increased probability of working in privatized firms. In addition, male or female employees with 6 or 9 years of schooling are clearly more likely to work in a privatized firm. The coefficient estimates for time elapsed since last promotion and occupation are in the expected direction, given the differences observed in Tables 2 and 3. Workers with longer periods of time without being promoted and in low skilled occupations are also more likely to be employed by privatized firms. The coefficients on privatization date reflect the proportion of employees in privatized firms relative to the employees in the control firms. Hence, for both males and females, the magnitude of the effect of privatization date is positive in 1994 given that in 1989 and 1994, the largest banks in the industry were privatized. For the actual matching, we also require, beyond the propensity score, that the pool of potential controls, to which a given treated observation may be paired, belong to the same year. We use the Mahalanobis metric for matching in these two variables. By matching within the year we remove explicitly any time-specific unobservables not controlled by the propensity score, and avoid that each individual is matched with him(her)self. This is the matching analogy to the fixed effects. Also notice that including this variable (privatization date) both in the propensity score and as additional matching variable amounts to increasing the weight of this variable when forming the matches.

V. Impact Estimates

As discussed in Section III, different matching schemes generate different estimates. This study adopts two estimators which are extreme in terms of neighbourhood size. We adopt the nearest-neighbour matching estimator and the Gaussian kernel estimator, without imposing any restriction in the region of common support. In terms of notation from Equation 4, we define $N_{ij} = 1$ for the

---

22 We also tried to include the size of the firm (for the banking sector, this is the only available firm characteristics variable in the data set) in order to control for observable selection of the firms being privatized. Perhaps because all banks are similar in size (with the exception of one bank that is substantially larger than the average), the overall impacts remained unaffected by the inclusion of this variable, affecting (worsening) only the quality of matching.

23 We also tried different specifications for the propensity score, including these and other variables, such as the monthly wage before privatization, as suggested by the work of Heckman et al. (1998). Once again, the magnitude of the impacts remains unaffected by the inclusion of these variables in the specification, affecting (worsening) only the quality of matching.

24 The matching estimates were obtained using command PSMATCH2 in Stata, written by Leuven and Sianesi (2003).

25 Table 12 in the appendix shows that lack of common support is not an issue in the present evaluation.
nearest-neighbour matching estimator, since each treated individual is matched with the closest non-treated individual. For the Gaussian kernel estimator, the outcome for each treated unit $i$ is matched with a kernel-weighted average of outcomes for all non-treated individuals, where the weight given to the nontreated unit $j$ is in proportion to the closeness, in terms of propensity score, between $i$ and $j$.

Formally, the outcome $Y_{i0}$ is weighted by $N_{ij} = K_{ij} / \sum_{j \neq i} K_{ij}$, where $K(\cdot)$ is based on the Gaussian distribution, $p$ is the propensity score and $h$ is size of the neighbourhood, i.e. the bandwidth. We chose $h = 0.06$. The overall effect of privatization in both cases is given by the arithmetic mean of all individual effects and $n_i$ is thus given by the inverse of the sample size of the treated group.

The results remain qualitatively unchanged if we adopt different neighbourhood sizes for each participant.
Overall, matching with the nearest-neighbour or with the kernel estimators on the estimated propensity score reduces substantially the variability in observable attributes, whether measured by the median absolute bias or the pseudo $R^2$ (see Table 13 in the appendix).

Table 5 reports the impact of privatization on the logarithm of hourly wage for men, over four different time periods, using four different matching strategies. In the first two rows, we present results from the nearest-neighbour and kernel matching estimators, implemented in the context of cross-section samples. The privatization effects are estimated using Equation 4 for $Y_{1t} = W_{1t} - W_{0t}$ and $Y_{0t} = W_{0t} - W_{0t}$. We also present the estimates obtained with a parametric difference-in-differences estimator, using the same control group as in Monteiro (2004). The pre-program period for each privatized firm is given by $t = 0$, while the post-program period is given by $t$, ranging between 1 and 4 years. For example, the figure $-0.064$ (first row, first column) indicates that during the first year post-reform, the wage paid to retained men in privatized firms grew 6.2% ($e^{-0.064} - 1$) less than the wage paid to their respective counterparts in public firms.

The overall picture depicted in Tables 5 and 6, and Figs 1 and 2, confirms the dynamic of the privatization effects formerly identified in Monteiro (2004).

### Table 6. Matching estimates of the impact of privatization on log hourly wage of women

<table>
<thead>
<tr>
<th>Time effect</th>
<th>Matching version</th>
<th>+1 year</th>
<th>+2 years</th>
<th>+3 years</th>
<th>+4 years</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Cross section</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1NN</td>
<td>-0.081* (0.029)</td>
<td>-0.028 (0.036)</td>
<td>0.051 (0.042)</td>
<td>-0.003 (0.015)</td>
</tr>
<tr>
<td></td>
<td>Kernel</td>
<td>-0.083* (0.024)</td>
<td>-0.037 (0.032)</td>
<td>0.048 (0.034)</td>
<td>0.003 (0.010)</td>
</tr>
<tr>
<td></td>
<td>Longitudinal</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1NN</td>
<td>-0.118* (0.023)</td>
<td>-0.108* (0.033)</td>
<td>-0.027 (0.036)</td>
<td>0.050* (0.012)</td>
</tr>
<tr>
<td></td>
<td>Kernel</td>
<td>-0.121* (0.020)</td>
<td>-0.101* (0.028)</td>
<td>-0.016 (0.029)</td>
<td>0.057* (0.009)</td>
</tr>
<tr>
<td></td>
<td>Parametric</td>
<td>-0.062* (0.004)</td>
<td>-0.115* (0.008)</td>
<td>-0.056* (0.006)</td>
<td>0.043* (0.007)</td>
</tr>
<tr>
<td></td>
<td>difference-in-differences</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Treated sample size</td>
<td>6 235*</td>
<td>4 967</td>
<td>4 486</td>
<td>2 151</td>
</tr>
</tbody>
</table>

*Source: Own computations based on Quadros de Pessoal, MSST (1989–1997).
Notes: SEs in parentheses. * denotes significant at the 1% level.

### Table 7. The impacts of privatization on the log hourly wage of men

<table>
<thead>
<tr>
<th>DiD matching</th>
<th>Time effect</th>
<th>+1 year</th>
<th>+2 years</th>
<th>+3 years</th>
<th>+4 years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>18 – &lt;30</td>
<td>0.030 (0.029)</td>
<td>0.009 (0.046)</td>
<td>0.027 (0.057)</td>
<td>0.089** (0.037)</td>
</tr>
<tr>
<td></td>
<td>&gt;30 – &lt;50</td>
<td>-0.084* (0.022)</td>
<td>-0.130* (0.034)</td>
<td>0.022 (0.037)</td>
<td>0.070* (0.008)</td>
</tr>
<tr>
<td></td>
<td>&gt;50 – 65</td>
<td>-0.124** (0.062)</td>
<td>-0.134*** (0.070)</td>
<td>-0.093 (0.073)</td>
<td>-0.056** (0.027)</td>
</tr>
<tr>
<td>Tenure</td>
<td>&gt;0 – &lt;10</td>
<td>-0.026** (0.011)</td>
<td>-0.131* (0.025)</td>
<td>0.019 (0.038)</td>
<td>0.089* (0.013)</td>
</tr>
<tr>
<td></td>
<td>&gt;10 – &lt;20</td>
<td>-0.067*** (0.035)</td>
<td>-0.109** (0.045)</td>
<td>0.065*** (0.049)</td>
<td>0.074* (0.011)</td>
</tr>
<tr>
<td></td>
<td>&gt;20</td>
<td>-0.138* (0.046)</td>
<td>-0.114** (0.052)</td>
<td>0.060 (0.066)</td>
<td>-0.013 (0.018)</td>
</tr>
<tr>
<td>Education</td>
<td>&gt;0 – &lt;6</td>
<td>-0.163* (0.027)</td>
<td>-0.138* (0.029)</td>
<td>-0.032 (0.034)</td>
<td>-0.0004 (0.030)</td>
</tr>
<tr>
<td></td>
<td>&gt;6 – &lt;16</td>
<td>-0.083* (0.027)</td>
<td>-0.121* (0.035)</td>
<td>0.020 (0.041)</td>
<td>0.069* (0.008)</td>
</tr>
<tr>
<td></td>
<td>&gt;16</td>
<td>-0.164* (0.037)</td>
<td>-0.183* (0.053)</td>
<td>-0.118*** (0.068)</td>
<td>-0.068** (0.039)</td>
</tr>
<tr>
<td>Occupation</td>
<td>High skilled</td>
<td>-0.066* (0.020)</td>
<td>-0.095* (0.028)</td>
<td>-0.053*** (0.041)</td>
<td>0.006 (0.015)</td>
</tr>
<tr>
<td></td>
<td>Low skilled</td>
<td>-0.106* (0.032)</td>
<td>-0.121* (0.041)</td>
<td>0.036 (0.048)</td>
<td>0.090* (0.009)</td>
</tr>
<tr>
<td>Treated sample size</td>
<td>17 210</td>
<td>13 912</td>
<td>12 726</td>
<td>6 801</td>
<td></td>
</tr>
</tbody>
</table>

*Source: Own computations based on Quadros de Pessoal, MSST (1989–1997).
Notes: SEs in parentheses. *,**, and *** denotes statistically significant from zero at the 1, 5 and 10% levels.
Table 8. The impacts of privatization on the log hourly wage of women

<table>
<thead>
<tr>
<th>DiD matching</th>
<th>Time effect</th>
<th>+1 year</th>
<th>+2 years</th>
<th>+3 years</th>
<th>+4 years</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Age</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>&gt;18 – &lt;30</td>
<td>-0.013</td>
<td>(0.026)</td>
<td>-0.096*</td>
<td>(0.046)</td>
<td>-0.061</td>
</tr>
<tr>
<td>&gt;30 – &lt;50</td>
<td>-0.121*</td>
<td>(0.029)</td>
<td>-0.108*</td>
<td>(0.040)</td>
<td>-0.037</td>
</tr>
<tr>
<td>&gt;50 – &lt;65</td>
<td>-0.127*</td>
<td>(0.036)</td>
<td>-0.155*</td>
<td>(0.048)</td>
<td>-0.065</td>
</tr>
<tr>
<td><strong>Tenure</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0 – &lt;10</td>
<td>-0.014</td>
<td>(0.013)</td>
<td>-0.140*</td>
<td>(0.029)</td>
<td>0.009</td>
</tr>
<tr>
<td>&gt;10 – &lt;20</td>
<td>-0.144*</td>
<td>(0.037)</td>
<td>-0.068***</td>
<td>(0.046)</td>
<td>-0.025</td>
</tr>
<tr>
<td>&gt;20</td>
<td>-0.156*</td>
<td>(0.049)</td>
<td>-0.119***</td>
<td>(0.071)</td>
<td>-0.024</td>
</tr>
<tr>
<td><strong>Education</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0 – &lt;6</td>
<td>-0.155*</td>
<td>(0.022)</td>
<td>-0.163*</td>
<td>(0.027)</td>
<td>0.062***</td>
</tr>
<tr>
<td>&gt;6 – &lt;16</td>
<td>-0.115*</td>
<td>(0.027)</td>
<td>-0.133*</td>
<td>(0.035)</td>
<td>-0.033</td>
</tr>
<tr>
<td>&gt;16</td>
<td>-0.142**</td>
<td>(0.080)</td>
<td>-0.117</td>
<td>(0.118)</td>
<td>-0.079</td>
</tr>
<tr>
<td><strong>Occupation</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High skilled</td>
<td>-0.108*</td>
<td>(0.029)</td>
<td>-0.217*</td>
<td>(0.045)</td>
<td>-0.054</td>
</tr>
<tr>
<td>Low skilled</td>
<td>-0.125*</td>
<td>(0.026)</td>
<td>-0.117*</td>
<td>(0.037)</td>
<td>-0.025</td>
</tr>
</tbody>
</table>

**Notes**: SEs in parentheses. *, ** and *** denote statistically significant from zero at the 1, 5 and 10% levels.

**Source**: Own computations based on Quadros de Pessoal, MSST (1989–1997).

Fig. 1. The impact of privatization on the hourly wage of men

Fig. 2. The impact of privatization on the hourly wage of women
In fact, in contrast with prior evidence, both tables and figures reveal that the privatization effects vary in sign and magnitude according to the time of evaluation. Moreover, both figures seem to suggest a positive relationship between time of restructuring and relative growth rate of wages.

During the first 2 years after privatization, both men and women suffer lower wage growth rates, but this development tends to be reversed in the subsequent periods. Consequently, this result supports the general objective of restructuring (cost reduction) implicit in the implementation of the policy, and indirectly confirmed by Pinho (1999). As previously referred, the Portuguese banking industry experienced a significant efficiency improvement during the period 1988–97, particularly among the privatized institutions. Our short-term wage effects are therefore consistent with the findings of McGuckin and Nguyen (2001) regarding the effects of ownership changes in the US manufacturing sector: around 76% of employees enjoyed lower wage growth rates after the ownership change. On the other hand, deregulation of the product market – a related policy implemented in order to increase the degree of product market competition – leads in general to declines in the wage growth rate. For example, Black and Strahan (2001) find that, in the US banking industry, male wages fell by 12.5%.

Three years post-reform, the matching impacts of privatization are mixed and insignificant, whereas, in the fourth year, retained employees are in advantage if longitudinal matching estimates or previous results are considered. For this period of analysis, the results are then consistent with those found by Parker and Martin (1996), despite the fact that their analysis includes the entire workforce, regardless of gender. These authors find that 4 or 5 years after privatization, wages, on average, had increased (up to 8.4%) in 7 out of 11 privatized firms in the UK, when compared to the whole economy.

These results seem to suggest a change in the pay policy of privatized firms. After firms have completed the main adjustments, elimination of redundant workers – consistent with figures on elapsed time since last promotion from Tables 2 and 3 – and reduction of wage growth, the remaining labour force is better rewarded. The rise in wage growth rates following the adjustment period corresponds well with predictions from theoretical privatization models that include effort or efficiency wages (Haskel and Sanchis, 1995; Goerke, 1998). Workers might have exerted a higher level of effort, also acknowledged in the bargaining contract, and thus increased productivity, as a response to increased fears of dismissal, given the uncertainty caused by the reform.

A point worth noting concerns the performance of the four matching estimators implemented. Both longitudinal matching estimators tend to overestimate the impact found by the corresponding cross-sectional estimator, and the difference between the estimators increases with time after the reform. May be the relatively small number of variables used in this study, compared with the studies of evaluation of employment and training programs, may explain the different performance between cross-sectional and longitudinal estimators. Alternatively, this difference may indicate the implausibility of the cross-sectional matching assumptions for analysing long term effects. Recall that in a pure random experiment different methodological strategies would yield similar results within each time period. Thus, there might be some unobservable or observable variables, not accounted for, that are contaminating the results.

We turn now to the question of identifying sources of heterogeneity, other than gender and timing, for which privatization effects are most prominent. Therefore, Table 7 and 8 report results obtained from the nearest neighbour difference-in-differences matching estimator for men and women, respectively, for different groups stratified according to age, tenure, education, occupation and full-time status.

It turns out that assuming a single privatization effect per time period masks, to a considerable extent, the variation of privatization effects. Nevertheless, although significant differences across and within skill groups arise from these two tables, a fairly similar positive trend can be detected for most skill groups, regardless of gender. Figure 3 helps to uncover these trends.

Starting with age, the results indicate that privatization penalized relatively more the oldest employees of both genders. In fact, employees aged 50+ years experienced significant wage losses during the first 2 years, clearly more pronounced than the average of the respective gender group within each time period. This may not be particularly surprising, as the workers in this age-group were relatively close to retirement. On the other hand, whereas compulsory wage promotions are defined for the initial years of the career by the wage agreement contract, these are optional for the latter years of the career and for the highest paid occupations within the firm. Therefore, results suggest that firms cut wages for the oldest individuals, while rewarding the youngest employees.

Evidence on tenure subgroups also reflects this restriction of the firms’ freedom to set wages for certain experienced groups. Individuals who remained the longest time within the firm suffered
Fig. 3. The impact of privatization on the hourly wage by gender, across age, tenure, education and occupational groups
the highest wage losses over a longer time period, while the younger individuals enjoyed the highest wage gains.

Looking at educational breakdowns, a surprising result is displayed. In contrast with our expectation, the best educated male and female employees are the most negatively affected sub-groups in the workforce, suffering sharp and lasting reductions (which are never reversed) in their relative wages, in particular after 2 years of the implementation of the reform. A possible explanation for this finding might relate to 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 generous nonwage compensation, such as free car. On the other hand, since banks incurred substantial expenses in continuous job-training programs to update an old workforce, formal education might have become relatively less relevant in terms of pay policy.

Finally, in what concerns occupational sub-categories, both tables indicate that, during the first 2 years, privatization had a negative impact on relative wages for both low- and high-skilled workers. Yet, in contrast with the low-skilled category, the high-skilled workers never enjoyed a relative wage increase. 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 privatization on CEO pay levels. The reason for this finding is likely to be related to the down-sizing strategy of privatized firms, possibly implying a lower level of supervision and responsibilities for employees in this occupational category.

VI. Concluding Remarks

The causal effect of privatization on wages remains an important and controversial topic among policymakers and economists alike. The frequent opposition from public opinion and trade unions towards privatization programs makes this particular topic a challenging issue for policy makers. The resistance usually arises from the fear of adverse labour strategies, including either displacement or wage reductions. For economists, on the other hand, the topic creates additional interest, for at least two different reasons. First, different theoretical approaches produce ambiguous predictions regarding the wage effects of privatization. Second, there is the habitual missing data problem inherent in the evaluation of causal effects in observational studies. In contrast with active labour market policies, though, privatization has not been the target of a lively discussion from an evaluation standpoint, and therefore deserves further scrutiny.

The purpose of this article has been to investigate the effects of privatization on wages in the Portuguese banking industry. In particular, we were interested in testing if earlier findings on privatization wage effects (Monteiro, 2004) are robust to the selection of methodology. Following earlier analysis, we focus on the effects of privatization on employees who remained within the firm after the reform, by comparing wages of those employees with employees in public firms. We have done this by implementing two variants of matching estimators in two different contexts: cross-sectional and longitudinal samples.

In general, the results point to an overall confirmation of previous findings. Indeed, our results, obtained from Quadros de Pessoal for the period between 1989 and 1997, generally show a negative (positive) short-run (long-run) effect of privatization on relative wage growth for both men and women retained in the privatized firms. When the wage effects are broken down to account for the heterogeneity of the effects, a persistent positive pattern prevails, irrespective of 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 also imply that, rather than education, seniority and experience still count for much in this particular labour market.

Our results have at least two important policy implications. First, privatization seems to be a gender neutral policy, given the similarity of the effects by gender, both in terms of trend and intensity. Thus, our results appear to contradict Gary Becker’s prediction about the relationship between market structure and discrimination. Nevertheless, more research is clearly needed to assess if privatization affects men and women differently. Second, the evidence presented so far also shows that the fear of wage cuts following privatization – as is often argued by labour unions – seems to be unfounded. Indeed, wage losses – if they occur – are only temporary, as the long-term dynamics seem to confirm the law of one price in the labour market.

27 See Rosen (1992) or Wolfram (1998) for a theoretical and empirical survey on executive pay levels.
Acknowledgements
I thank two anonymous referees, Mark Stewart, Odd Rune Straume, Ian Walker and participants at several seminars and conferences for valuable comments. I am indebted to the Ministério do Trabalho e da Solidariedade for allowing the availability of data from Quadros de Pessoal. Financial support was provided by the Ministério da Ciência e Tecnologia under the grant BD/SFRH/2000/1291.

References
Ministério da Segurança Social e do Trabalho (MSST), Departamento de Estatística do Trabalho, Emprego e Formação Profissional (DETEFP), *Greves Anual, Informação Estatística (Síntese)*, various years, Lisbon, Portugal.
Ministério da Segurança Social e do Trabalho (MSST), Departamento de Estudos, Prospectiva e Planeamento (DEPP), Coleção Relatórios e Análises, Série Regulamentação do Trabalho. Aumento Médio Ponderado Intertabelas e Aumentos Intertabelas Deflacionados, various years, Lisbon, Portugal.


### Appendix

Table 9. Results from the participation probit for men and women when t = 2

<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th></th>
<th>Women</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>SE</td>
<td>Coefficient</td>
<td>SE</td>
</tr>
<tr>
<td>Constant</td>
<td>0.288</td>
<td>0.454</td>
<td>0.087</td>
<td>0.144</td>
</tr>
<tr>
<td>Tenure^{1}</td>
<td>0.011</td>
<td>0.006</td>
<td>0.027*</td>
<td>0.010</td>
</tr>
<tr>
<td>Tenure^{2}</td>
<td>-0.001*</td>
<td>0.0001</td>
<td>-0.002*</td>
<td>0.0003</td>
</tr>
<tr>
<td>Experience</td>
<td>-0.073*</td>
<td>0.008</td>
<td>-0.053*</td>
<td>0.008</td>
</tr>
<tr>
<td>Experience^{2}</td>
<td>0.001*</td>
<td>0.0001</td>
<td>0.001*</td>
<td>0.0001</td>
</tr>
<tr>
<td>Education vs. less than 4 years of schooling</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Primary (4)</td>
<td>2.297*</td>
<td>0.449</td>
<td>0.646*</td>
<td>0.103</td>
</tr>
<tr>
<td>Preparatory (6)</td>
<td>2.495*</td>
<td>0.449</td>
<td>0.987*</td>
<td>0.117</td>
</tr>
<tr>
<td>Lower secondary (9)</td>
<td>2.642*</td>
<td>0.449</td>
<td>1.383*</td>
<td>0.119</td>
</tr>
<tr>
<td>Upper secondary (12)</td>
<td>1.867*</td>
<td>0.449</td>
<td>0.903*</td>
<td>0.121</td>
</tr>
<tr>
<td>University (16)</td>
<td>2.130*</td>
<td>0.451</td>
<td>1.002*</td>
<td>0.143</td>
</tr>
<tr>
<td>Number of months since last promotion</td>
<td>0.007*</td>
<td>0.0002</td>
<td>0.008*</td>
<td>0.0003</td>
</tr>
<tr>
<td>Low skilled vs. high skilled</td>
<td>-0.109*</td>
<td>0.022</td>
<td>-0.163*</td>
<td>0.043</td>
</tr>
<tr>
<td>Privatization date vs. 1989</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1991</td>
<td>-1.692*</td>
<td>0.043</td>
<td>-0.759*</td>
<td>0.043</td>
</tr>
<tr>
<td>1992</td>
<td>-1.782*</td>
<td>0.047</td>
<td>-0.814*</td>
<td>0.052</td>
</tr>
<tr>
<td>1993</td>
<td>-1.560*</td>
<td>0.046</td>
<td>-0.747*</td>
<td>0.051</td>
</tr>
<tr>
<td>1994</td>
<td>-0.991*</td>
<td>0.045</td>
<td>0.122*</td>
<td>0.049</td>
</tr>
<tr>
<td>LR chi-squared</td>
<td>6 439</td>
<td>0.000a</td>
<td>2 494</td>
<td>0.000a</td>
</tr>
<tr>
<td>Pseudo R^2</td>
<td>0.209</td>
<td>0.178</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fraction correctly predicted (cut-off = 0.5)</td>
<td>70.50</td>
<td>68.32</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sample size</td>
<td>22 994</td>
<td>10 110</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: Own computations based on Quadros de Pessoal, MSST (1989–1997).

Notes: * denotes significant at the 1% level.

*p-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatization.
Table 10. Results from the participation probit for men and women when $t = 3$

<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th></th>
<th>Women</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>SE</td>
<td>Coefficient</td>
<td>SE</td>
</tr>
<tr>
<td>Constant</td>
<td>0.177</td>
<td>0.519</td>
<td>0.098</td>
<td>0.177</td>
</tr>
<tr>
<td>Tenure</td>
<td>0.044*</td>
<td>0.008</td>
<td>0.050*</td>
<td>0.011</td>
</tr>
<tr>
<td>Tenure$^2$</td>
<td>-0.002*</td>
<td>0.0002</td>
<td>-0.004*</td>
<td>0.0003</td>
</tr>
<tr>
<td>Experience</td>
<td>-0.083*</td>
<td>0.001</td>
<td>-0.045*</td>
<td>0.011</td>
</tr>
<tr>
<td>Experience$^2$</td>
<td>0.002*</td>
<td>0.0001</td>
<td>0.001*</td>
<td>0.0001</td>
</tr>
<tr>
<td>Education vs. less than 4 years of schooling</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Primary (4)</td>
<td>2.476*</td>
<td>0.513</td>
<td>0.355</td>
<td>0.137</td>
</tr>
<tr>
<td>Preparatory (6)</td>
<td>2.926*</td>
<td>0.513</td>
<td>0.933*</td>
<td>0.150</td>
</tr>
<tr>
<td>Lower secondary (9)</td>
<td>3.093*</td>
<td>0.513</td>
<td>1.502*</td>
<td>0.153</td>
</tr>
<tr>
<td>Upper secondary (12)</td>
<td>2.216*</td>
<td>0.513</td>
<td>0.922*</td>
<td>0.155</td>
</tr>
<tr>
<td>University (16)</td>
<td>2.578*</td>
<td>0.516</td>
<td>1.119*</td>
<td>0.176</td>
</tr>
<tr>
<td>Number of months since last promotion</td>
<td>0.010*</td>
<td>0.0003</td>
<td>0.010*</td>
<td>0.0004</td>
</tr>
<tr>
<td>Low skilled vs. high skilled</td>
<td>-0.131*</td>
<td>0.027</td>
<td>-0.179*</td>
<td>0.047</td>
</tr>
<tr>
<td>Privatization date vs. 1989</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1991</td>
<td>-1.629*</td>
<td>0.063</td>
<td>-0.586*</td>
<td>0.056</td>
</tr>
<tr>
<td>1992</td>
<td>-2.282*</td>
<td>0.065</td>
<td>-1.148*</td>
<td>0.058</td>
</tr>
<tr>
<td>1993</td>
<td>-1.884*</td>
<td>0.065</td>
<td>-0.849*</td>
<td>0.058</td>
</tr>
<tr>
<td>1994</td>
<td>-1.582*</td>
<td>0.064</td>
<td>-0.411*</td>
<td>0.056</td>
</tr>
<tr>
<td>LR chi-squared</td>
<td>5 777</td>
<td>0.000a</td>
<td>2 642</td>
<td>0.000a</td>
</tr>
<tr>
<td>Pseudo $R^2$</td>
<td>0.252</td>
<td></td>
<td>0.234</td>
<td></td>
</tr>
<tr>
<td>Fraction correctly predicted (cut-off = 0.5)</td>
<td>76.71</td>
<td></td>
<td>73.81</td>
<td></td>
</tr>
<tr>
<td>Sample size</td>
<td>18 492</td>
<td></td>
<td>8 189</td>
<td></td>
</tr>
</tbody>
</table>

Source: Own computations based on Quadros de Pessoal, MSST (1989–1997).
Notes: * denotes significant at the 1% level.

$p$-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatization.

Table 11. Results from the participation probit for men and women when $t = 4$

<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th></th>
<th>Women</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>SE</td>
<td>Coefficient</td>
<td>SE</td>
</tr>
<tr>
<td>Constant</td>
<td>-5.514</td>
<td></td>
<td>0.986*</td>
<td>0.217</td>
</tr>
<tr>
<td>Tenure</td>
<td>0.074*</td>
<td>0.007</td>
<td>0.082*</td>
<td>0.012</td>
</tr>
<tr>
<td>Tenure$^2$</td>
<td>-0.002*</td>
<td>0.0002</td>
<td>-0.004*</td>
<td>0.0004</td>
</tr>
<tr>
<td>Experience</td>
<td>-0.107*</td>
<td>0.009</td>
<td>-0.102*</td>
<td>0.012</td>
</tr>
<tr>
<td>Experience$^2$</td>
<td>0.002*</td>
<td>0.0001</td>
<td>0.002*</td>
<td>0.0002</td>
</tr>
<tr>
<td>Education vs. less than 4 years of schooling</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Primary (4)</td>
<td>6.125*</td>
<td>0.108</td>
<td>-0.352**</td>
<td>0.186</td>
</tr>
<tr>
<td>Preparatory (6)</td>
<td>6.533*</td>
<td>0.102</td>
<td>-0.247</td>
<td>0.193</td>
</tr>
<tr>
<td>Lower secondary (9)</td>
<td>6.453*</td>
<td>0.096</td>
<td>0.032</td>
<td>0.197</td>
</tr>
<tr>
<td>Upper secondary (12)</td>
<td>5.796*</td>
<td>0.089</td>
<td>-0.508**</td>
<td>0.198</td>
</tr>
<tr>
<td>University (16)</td>
<td>5.743*</td>
<td>0.094</td>
<td>-0.899*</td>
<td>0.215</td>
</tr>
<tr>
<td>Number of months since last promotion</td>
<td>0.006*</td>
<td>0.0005</td>
<td>0.003*</td>
<td>0.0007</td>
</tr>
<tr>
<td>Low skilled vs. high skilled</td>
<td>-0.316*</td>
<td>0.028</td>
<td>-0.553*</td>
<td>0.054</td>
</tr>
<tr>
<td>Privatization date vs. 1989</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1991</td>
<td>0.729*</td>
<td>0.027</td>
<td>0.630*</td>
<td>0.043</td>
</tr>
<tr>
<td>1992</td>
<td>0.273*</td>
<td>0.033</td>
<td>0.302*</td>
<td>0.048</td>
</tr>
<tr>
<td>1993</td>
<td>-0.183*</td>
<td>0.039</td>
<td>-0.477*</td>
<td>0.068</td>
</tr>
<tr>
<td>LR chi-squared</td>
<td>2 426</td>
<td>0.000a</td>
<td>970</td>
<td>0.000a</td>
</tr>
<tr>
<td>Pseudo $R^2$</td>
<td>0.121</td>
<td></td>
<td>0.113</td>
<td></td>
</tr>
<tr>
<td>Fraction correctly predicted (cut-off = 0.5)</td>
<td>68.10</td>
<td></td>
<td>72.63</td>
<td></td>
</tr>
<tr>
<td>Sample size</td>
<td>14 490</td>
<td></td>
<td>6 965</td>
<td></td>
</tr>
</tbody>
</table>

Source: Own computations based on Quadros de Pessoal, MSST (1989–1997).
Notes: * denotes significant at the 1% level.

$p$-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatization.
### Table 12. Propensity scores by time and gender

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td><strong>t = 1</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Privatized</td>
<td>0.5537567</td>
<td>0.2340450</td>
<td>0.0131557</td>
<td>0.9887109</td>
</tr>
<tr>
<td>Public</td>
<td>0.2869447</td>
<td>0.2071896</td>
<td>0.0018966</td>
<td>0.9815215</td>
</tr>
<tr>
<td><strong>Women</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Privatized</td>
<td>0.4816485</td>
<td>0.220088</td>
<td>0.0129936</td>
<td>0.9554378</td>
</tr>
<tr>
<td>Public</td>
<td>0.2371310</td>
<td>0.1924759</td>
<td>0.0006537</td>
<td>0.9402833</td>
</tr>
<tr>
<td></td>
<td><strong>t = 2</strong></td>
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<td></td>
<td></td>
</tr>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Privatized</td>
<td>0.6999680</td>
<td>0.2265299</td>
<td>0.1025044</td>
<td>0.9988026</td>
</tr>
<tr>
<td>Public</td>
<td>0.4588628</td>
<td>0.1831909</td>
<td>0.0026444</td>
<td>0.9919025</td>
</tr>
<tr>
<td><strong>Women</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Privatized</td>
<td>0.6045986</td>
<td>0.2280892</td>
<td>0.0085209</td>
<td>0.9891979</td>
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**Source:** Own computations based on Quadros de Pessoal, MSST (1989–1997).

### Table 13. Indicators of covariate balancing, before and after matching, by time and gender

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<tr>
<th></th>
<th>Probit pseudo $R^2$</th>
<th>$Pr &gt; \chi^2$</th>
<th>Median absolute bias before</th>
<th>Median absolute bias after</th>
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**Source:** Own computations based on Quadros de Pessoal, MSST (1989–1997).