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Increasing the legal retirement age: The impact on wages, worker flows and firm performance*

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Abstract

Many countries with pay-as-you-go pension systems have increased or plan to increase their legal retirement age (LRA) in order to address the financial consequences of ageing. Although the success of these policies is ultimately determined at the labour market, little is known about the demand-side implications of higher LRAs. Here, we identify the effect of LRA's upon firms by considering a legislative reform introduced in Portugal in 1994: women's LRA was gradually increased from 62 to 65 years while men's LRA stayed unchanged at 65. Using detailed matched employer-employee panel data and difference-in-differences matching methods, we analyse the effects of the reform in terms of a number of worker- and firm-level outcomes. After providing evidence of compliance with the law, we find that the wages and hours worked of older women (those required to work additional years) were virtually unchanged. However, firms employing older female workers significantly reduced their hirings, especially of younger female workers. Moreover, we find evidence that those firms lowered their output but not output per worker.

Keywords: Social security reform; older workers; matching estimators.

JEL Codes: J14, J26, J63.

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1 Introduction

Many pay-as-you-go pension systems across the world have been under financial pressure due to the combined effect of increased life expectancy and lower fertility rates. Several countries have responded or plan to respond by adjusting the legal retirement age (henceforth, LRA) - the age at which workers are entitled to retire.[1] Moreover, the age of retirement is also likely to be suject to further changes due to legislation against age discrimination (e.g. the recent directives issued by the European Union) which may eventually lead to the abolishment of mandatory retirement ages.

Although changes in pension systems have typically been studied from the point of view of their labour supply consequences, there are several reasons why adjustments in retirement age can also affect firm behaviour and labour demand. For instance, firms may offer incentives schemes in which wages are below productivity when workers start their careers, and then gradually increase at a faster pace than productivity (Lazear 1979). In the context of these incentive pay structures, an *ex post* increase in the mandatory retirement age would be detrimental to firms' profitability, particularly in a context of downward wage rigidity or of a relatively strict employment protection legislation.

Moreover, firms that are forced to retain workers for a period longer than initially expected may respond by decreasing their hirings of new staff as the older workers will need to be replaced only later. In this case, the net effect of higher mandatory retirement ages upon the sustainability of PAYG pension systems is weakened. On the one hand, social security contributions made by workers forced to postpone their retirement will increase while pension outlays will fall. On the other hand, social security deductions made by workers who are not hired will presumably fall while the payment of unemployment benefits may increase.

While it has been established that economic incentives play an important role in retirement decisions (e.g. Meghir & Whitehouse (1997) find that increased earnings in work delay job exit while increased social security benefits delay the return to work), very little is known about the implications of changes in the mandatory age of retirement, particularly at the firm level. The only related paper we can cite is Ichino et al. (2007), which argues that increasing the retirement age helps solve pension problems only if the employment prospects of the elderly

[1] For instance, Schwarz & Demirgue-Kunt (1999) describe the cases of 17 countries that have increased their mandatory retirement ages between 1992 and 1998. See also Burtless & Quinn (2002) for an analysis of the U.S. case.

remain intact. Their evidence suggests that, although displaced elderly workers initially lose out in terms of employment chances, later on there are no significant employability differences between older and younger displaced workers. However, an approach based on displacement cannot shed much light on the impact of increases in retirement age, particularly as the employment protection legislations of many countries restrict firings.[2]

Our evidence is based on a quasi-experiment which specifically involves an increase in the legal retirement age: a law introduced in Portugal in 1993 which increased the mandatory retirement age of women from 62 to 65 years while leaving the corresponding age for men unchanged at 65. Moreover, instead of focusing only on the specific case of older workers, we take a broader look of the labour market, namely by considering several aspects of the personnel policies of firms. Finally, we also examine the consequences of the reform in terms of some measures of firm performance.

Furthermore, we follow the affected individuals and their firms throughout time and compare them with suitable 'control' groups, based on detailed information about almost all wage earners and their firms in the private sector. Using treatment effects methods, most notably a combination of the difference-in-differences and matching approaches, we analyze the extent to which the extension of the legal retirement age changed the employment status, hours worked, and wages of women affected by legislative change. At the firm level, using similar methods, we study the effect of postponing the legal retirement age upon total hirings, separations, net job creation, and the hirings of different demographic groups. We also consider the effects upon firm performance.

The remaining of the paper is organized as follows. Section 2 sketches the Portuguese pension system, before and after the new law. The econometric methodologies, including the construction of treatment and control groups, are described in Section 3. We then present the data in Section 4, while Section 5 measures the compliance with the new law. Finally, Section 6 presents the results and Section 7 concludes.

2 The retirement law reform in Portugal

As in many other countries, the pension system in Portugal is of the defined-benefit type, as the amount of the pension awarded to an older individual depends on the number of years the

[2] See also Ashenfelter & Card (2002), who find that, in the US defined-contributions setting, the elimination of mandatory retirement for college and university faculty led to lower retirement rates.

individual worked and on some weighted average of the wages earned throughout the person's career. The amount of the pension therefore does not depend on the returns to financial assets over the period in which the worker made his or her contributions. Moreover, the funding of these pensions is typically carried out on a pay-as-you-go basis, in which current workers' contributions are used to pay the benefit of current retirees.[3]

In the early 1990s, the Portuguese pension system was facing the financial problems typical of defined-benefit pay-as-you-go systems. These problems arose due to population ageing, as a consequence of higher life expectancy and particularly low fertility rates, (in 1993, for instance, those aged 65 and above corresponded to 21.6% of the working population (*Annual Report* 1994)). As a response to these circumstances, the Portuguese government decided in 1993 to raise the mandatory retirement age of women from 62 to 65 years ('Decreto-Lei 329/93'), thus equalising the LRA for men and women.[4]

The law indicated that the new age of retirement for women would be implemented gradually, presumably to smooth the impact upon the first cohorts of older women that would be affected when the reform came into force. Specifically, the retirement age for women increased by six months every year, until it converged in 1999 to the level of men (see Table 1). For instance, while a woman born on 31st December 1931 would be entitled to retire on 31st December 1993 (on her 62nd birthday), a woman born one day later, on 1st January 1932, would only be entitled to receive her pension on 1st July 1994 (when 62 years and six months old). However, due to the gradual phasing in of the new retirement age, women born six months later, on 1st July 1932, would reach retirement age on 1st July 1995, i.e. when 63 years old.

There are two additional aspects in the pension system in Portugal that need to be taken into account. The first is that a LRA denotes the age at which a worker is entitled to claim old-age pension, provided that the worker contributed to the social security system for a sufficiently long period. At that point in time, the labour contract established between the employer and the employee is automatically terminated. However, workers are free to establish new labour contracts, with the same or different employers. Moreover, earnings received from

[3]The main alternative type of pension systems are of the fully-funded, defined-contribution type, when benefits are based on the value of individual accounts to individuals contribute over their working lives. This type tends to be riskier for individuals, as the value of the account will vary with fluctuations in interest rates. A new, hybrid system is the notional defined contribution type (see Barr & Diamond (2006) for a survey).

[4]The financial and insurance sectors were exempted from this provision of the law and are therefore removed from our empirical analysis. Moreover, the law also included other provisions, namely by making the formula that calculated the pension level less generous.

the new labour contract are not affected by the amount received from the old-age pension.

The second aspect to be taken into account is that, as in many other countries, the social security legislation in Portugal allowed for some exemptions from the 62 or the 65 years rule. Such exemptions, leading to early retirement, were typically observed for unemployed workers, workers in firms undergoing economic turbulence, and in jobs supposed to be particularly exhausting (e.g. air traffic controllers). These exemptions motivate our analysis of compliance (Section 5).

3 Identification and estimation methods

The feasibility of our evaluation exercise depends crucially on the suitability of the counterfactual groups that can be generated from the available data. We address this matter by carefully selecting units for the control group(s) and by using a combination of two methodologies typically proposed to tackle non-experimental settings: difference-in-differences and matching (Rosenbaum & Rubin 1983). In particular, we implement a difference-in-differences matching estimator (Heckman et al. 1997, 1998), which Smith & Todd (2005) show that may have the potential benefit of eliminating some sources of bias present in non-experimental settings, improving the quality of evaluation results significantly.

We take advantage of the characteristics of the dataset and of the new legal framework to construct treatment and control groups. In particular, we explore (i) the existence of data for the pre- and post-legislative periods; (ii) the source of variation that the gender-specific law introduced; and (iii) the availability of a rich set of covariates and of data originating from the same local labour market (Heckman et al. (1997)).

3.1 The treatment and control groups

In the limit, the new retirement law will have directly affected all women under the age of 62 and all firms that employed at least one such woman. Nonetheless, some specific groups of women were more likely to be affected by a firm's response to the new legal retirement age. Such women include those who would have reached the legal retirement age in year $t+1$ had the LRA remained at its value of year t. For instance, those aged [60; 60.5) by the end of 1992 would have presumably retired in 1994 under the previous age limit (62); however, due to the increase to 62.5 years, they will have had to postpone their retirement to 1995. We therefore assign such women to our treatment group.

The definition of the treatment group can also be extended above the first cohort of women affect by the new law. For example, women aged [55;60.5) by the end of 1992 had to postpone their retirement up to 1999, the year when the retirement age was equalized across genders. Finally, when analyzing the impact of the increase in the LRA on firm-level variables, we consider that a firm is a treated unit if it has at least one of these women among its staff.

A related choice that needs to be made concerns the years that correspond to the 'before' and 'after' periods. Recall that, starting in 1994, the women's new LRA was increased each year by six months until it reached (the men's LRA of) 65 years in 1999. Two obvious candidates for our 'before' period are the years of 1992 and 1993. We choose 1992 because the new law was already under discussion in 1993, which may have prompted individuals and firms to react in anticipation. On the other hand, the government policy was unknow in 1992 and, therefore, that year should not suffer from any anticipation effects. Therefore, the treatment group in the 'before' period includes all women aged [57.5; 60.5) or firms with women in this age range (see first column in Table 1).

Regarding the non-experimental control group, we adopt two definitions, depending on whether we are conducting an individual- or firm-level analysis. In the former case, we consider as our control the group formed by men in the same age group as the women included in the treatment group. As men's LRA was already 65 years when the new law came into effect in the 1994 to 1999 period, we can construct comparable control groups in this age-related dimension. Of course, this control group raises gender-related issues. These are, however, mitigated if we are willing to accept the time-invariance hypothesis of the D-in-D estimator (discussed in the next section). In other words, if the gender gap is constant over the analysis period, using men as control for women is less of an issue. Indeed, the data seems to support this hypothesis. Between 1991 and 1993, the log difference of worked hours between men and women was 0.098, 0.093 and 0.10, while the log difference of total remuneration was 0.39, 0.39 and 0.38; these values are also statistically (and economically) constant over time.

At the firm-level analysis (hirings, separations, net job creation and firm performance), we consider different control groups, based on whether firms employed in 1992 any women affected by the new law. For instance, one possible control group is made up of firms that do not employ any woman aged [60; 60.5) in 1992. In this case, the corresponding treatment group would be firms that employ at least one such woman in 1992. We then consider alternative treatment/control groups by broadening the range of ages that lead to the assignment of firms into each group. The broadest age range corresponds to firms that employ or do not employ any woman aged [55; 60.5) in 1992.

Besides any gender-related issues that may arise, the non-compulsory nature of the quasi-natural experiment may raise questions about selection into treatment status. These, as far as they are imputable to observables, can be handled by the matching causal inference methodology. To address differences between the two groups due to non-observable factors, we combine both D-in-D and matching strategies (Heckman et al. 1997, 1998), the so-called D-in-D matching estimator.

3.2 Econometric implementation

Let Y_{it}^{D} be the potential outcome of interest for individual i at time t had (s)he been in state D, where $D = 1$ if exposed to the program and 0 otherwise. Let treatment take place at time t. The fundamental identification problem lies in the fact that we do not observe, at time t, individual i in both states. Therefore, we cannot compute the individual treatment effect, $Y_{it}^{1} - Y_{it}^{0}$. One can, however, if provided with a convenient control group, estimate the average effect of the treatment on the treated.

The idea behind a D-in-D estimator is that we can use an untreated comparison group to identify temporal variation in the outcome that is not due to the treatment. However, in order to achieve identification of the general D-in-D estimator we need to assume

$$E[Y_{it}^{0} - Y_{it'}^{0} \mid D = 1] = E[Y_{it}^{0} - Y_{it'}^{0} \mid D = 0], \tag{1}$$

where t' is a time period before the program implementation. The assumption states that, over time, the outcome variable of treated individuals ($D = 1$), in the event that they had not been exposed to the treatment, would have evolved in the same fashion as actually observed for the individuals not exposed to the treatment ($D = 0$), known as the time invariance principle.

If the assumption expressed in (1) holds, the D-in-D estimate of the average treatment effect on the treated can be obtained by the sample analogs of

$$\widehat{\alpha}_{\text{D-in-D}} = \{E[Y_{it} \,|D = 1] - E[Y_{it} \,|D = 0]\} - \{E[Y_{it'} \,|D = 1] - E[Y_{it'} \,|D = 0]\}. \qquad (2)$$

The time invariance assumption can be too stringent if the treated and control groups are not balanced in covariates that are believed to be associated with the outcome variable. The D-in-D setup can be extended to accommodate a set of covariates and this is usually done in a linear way, which takes into account eligibility specific effects and time/aggregate effects. In the following model, $\widehat{\alpha}_D$ corresponds to the D-in-D estimate obtained on a sample of treatment and control units

$$Y_{it} = \lambda D + \tau_t + \theta' Z_{it} + \alpha_D D \tau_t + \varepsilon_{it}, \qquad (3)$$

where D is as before and represents the eligibility-specific intercept, defined over age and gender according to treatment rules, τ_t captures time/aggregate effects and equals 0 for the 'before' period and 1 for the 'after' period, and Z is a vector of covariates included to correct for differences in observed characteristics between individuals in treatment and control groups.

This estimator controls for both differences in the Zs and for time-specific effects, but it does not allow α_D to depend on Z and it does not impose common support on the distribution of the $Z's$ across the cells defined by the D-in-D approach (namely, before and after, and treatment and control). Additionally, this procedure might be inappropriate if the treatment has different effects for different groups in the population.

These pitfalls can be overcome by supplementing the D-in-D estimates with propensity score matching. The difference-in-differences matching (DDM) estimator adds to the simple D-in-D estimator the comparability on the observable covariates that characterizes the propensity score matching estimator.

The feasibility of the matching strategy relies on a rich set of observable individual characteristics, Z, to guarantee that the distribution of the individual characteristics important to each evaluation exercise is the same in the difference-in-differences cells. The matching process models the probability of participation and matches individuals with similar propensity scores. The time invariance assumption for the DDM estimator is

$$E[Y^0_{it} - Y^0_{it'} \mid p, D = 1] = E[Y^0_{it} - Y^0_{it'} \mid p, D = 0], \tag{4}$$

where $p = \Pr(D = 1|X)$ is the propensity score. When estimating the mean impact of the treatment on the treated the matching estimator requires a conditional mean independence assumption

$$E\left(Y^0_{it}|Z, D = 1\right) = E\left(Y^0_{it}|Z, D = 0\right) = E\left(Y^0_{it}|Z\right) \tag{5}$$

and also requires that there is a nonparticipant analogue for each participant which means that $\Pr(D = 1|Z) < 1$.

The DDM estimator takes two forms, depending on the nature of the data, namely, balanced panel data or repeated cross-sections. For the former case,

$$\widehat{\alpha}_{DDM} = E\left[(Y^1_t - Y^1_{t'}) - \widehat{E}\left(Y^0_t - Y^0_{t'}|P\right)\right], \tag{6}$$

where $\widehat{E}(Y|P)$ represents the expected outcome of individuals in the control group matched with those in the treatment group. In the case of the repeated cross-section, the DDM takes the form of

$$\widehat{\alpha}_{DDM} = E\left[Y^1_t - \widehat{E}\left(Y^0_t|P\right)\right] - E\left[Y^1_{t'} - \widehat{E}\left(Y^0_{t'}|P\right)\right], \tag{7}$$

where all variables are as above. We will use both estimators.

Our definition of the treatment group in the case of the firms motivates the additional estimation of the propensity score using Poisson regression rather than probit or logit models. Indeed, recall that the treatment unit is defined if the firm has one or more women immediately or soon to be affected by the new legislation.

Recall that the Poisson regression specifies that y_i is drawn from a Poisson distribution with parameter λ_i, which in turn is a function of regressors z_i. Formally,

$$\Pr(Y_i = y_i) = \frac{e^{-\lambda_i}\lambda_i^{y_i}}{y_i!}, \quad y_i = 0, 1, 2, \ldots, \tag{8}$$

where the λ_i is typically specified as $\log(\lambda_i) = \beta' z_i$. Estimation of the Poisson regression is achieved by maximum likelihood.

Therefore, the Poisson regression allows us to analyse our data with some advantages. It handles oversampling of zeros yielding more robust estimates of the propensity scores. Secondly, it allows us to match firms with a similar number of expected treatment and control units without performing (pre-estimation) exact matching on this univariate dimension. Thus, in practice, we apply the matching algorithm both to the probability that there is at least one older woman (the standard definition of propensity score), but also to the estimated expected value of older women in the each firm and to the probability of at least one older woman, as derived from Poisson regressions.

4 Data

We use two datasets in our analysis. To study the issues of labour income, working hours and worker flows, we use *Quadros de Pessoal*, a matched employer-employee data set. The impact on labour market transitions are analyzed with a quarterly employment survey, *Inquérito ao Emprego.*

4.1 *Quadros de Pessoal*

The main data source used in this paper is *Quadros de Pessoal* (QP), a longitudinal dataset matching firms and workers based in Portugal. The data are gathered every year by the Ministry of Employment and Social Security, based on a questionnaire that every establishment with wage-earners is legally obliged to fill in. Reported data cover all personnel working for the firm/establishment in a reference month (March, up to 1993, and October, from 1994).

Personnel on short-term leave (such as sickness, maternity, strike or holidays) are also included, whereas personnel on long-term leave (such as military service) are not reported. Civil servants, the self-employed and domestic service are not covered, and the coverage of agriculture is low given its low share of wage-earners. Reported data include the firm's location, industry, employment, sales, ownership, legal setting, and the worker's gender, age, skill, occupation, schooling, hiring date, earnings, work duration, etc.

The mandatory nature of the survey leads to an extremely high response rate. Given the nature of the dataset, which covers not just every company with wage-earners, but also all of its workers, problems commonly faced by panel data sets, such as under- or over-sampling of certain groups and panel attrition, are much attenuated. Also, employer-reported wage information is known to be subject to less measurement error than worker-reported data.

Each firm entering the database is assigned a unique identifying number, in such a way that it can be followed over time. The Ministry of Employment implements several checks to ensure that a firm that has already reported to the database is not assigned a different identification number. Similarly, each worker also has a unique identifier, based on his/her social security number.

4.2 *Inquérito ao Emprego*

Our second dataset is taken from the nationally representative Portuguese quarterly employment surveys *Inquérito ao Emprego* (IE) conducted by *Instituto Nacional de Estatística*, the Portuguese statistics agency. We use data for the period 1992(2)-2000(4). In addition to employment status, the employment survey contains information on the individual's age, gender, schooling, etc.

The survey has a quasi-longitudinal nature: One sixth of the sample rotate out of the sample each quarter, so that we can track transitions from employment for up to five quarters. Transition rates are then obtained simply by identifying those employed individuals in the survey, who move out of employment over the subsequent quarter. In this paper, we distinguish between two destinations: unemployment and economic inactivity (i.e., withdrawal from the labour force).

The main restrictions imposed on the data set were that the individual be employed at the time of the survey, aged older than 55, and resident in mainland Portugal. Finally, due to potential sample attrition, we ensured that individuals appearing in subsequent surveys with the same identifier were in fact the same individual. The resulting sample size is 229,066 individuals. Among them we were able to identify 1,167 transitions from employment into inactivity.

5 Measuring compliance with the new law

A common concern with the measurement of treatment effects is the effectiveness of the quasi-experiment. In other words, one needs to know how far reaching is the impact of the legislative change, particularly in a context in which early retirement is observed.

In order to evaluate the effect of the new law on the labour force status of affected women we use the *Inquérito ao Emprego* data and specify conventional logit models to estimate the probability of being employed and the probability of being inactive. Based on time of the survey and on the age and gender of the individuals, we defined a dummy variable identifying the women likely to be affected by the change in the legislation. More specifically, this variable takes value one for women aged 62 to 62.5 years in 1994, for women aged 62 to 63 in 1995, and so on up to 1999, when the dummy is one for women aged 62 to 65. We called this variable the 'Treatment Group'.

The estimation results are provided in Table 2 where it can be seen that the probability of being employed for the treated group of women increased sizeably. According to the logit estimates, the odds ratio associated with the treatment group is 1.313, meaning that it is 31.3 percent more likely for a women affected by the increase of the retirement to be employed. Symmetrically, the probability of being inactive decreased significantly among the treated women, where the decline is estimated to be around 27.9 percent. The overall picture from these two logit regressions is that the new retirement age rules had a visible impact in the labour force status of affected women.

We also provide a more complete picture of the labour market changes that emerge from postponing the retirement age by looking at transitions out of employment. Since the Portuguese employment survey has a quasi-longitudinal nature, one can track transitions between labour market states for about five sixths of the sample. In particular, one can spot transitions from employment into inactivity among individuals who are old enough to consider retirement. Based on the age of the individuals, one should expect to see an increase in the hazard rate for women affected by the change in the legislation. This is indeed what is obtained from the estimation of a Cox proportional hazards model, where the time of the implementation of the new law (the 'After' variable in the specification) is treated as a time-varying covariate. The indication provided in Table 3 is that the hazard rate more than tripled among the affected women.

6 Results

6.1 Impact on income, working hours, and absenteeism

In Table 4, we present a set of DDM estimates for the effect of the treatment on the treated for total income, working hours, and the probability of absence from work. The general result that emerges is that the impact of the increase of the mandatory age of retirement on these women's labour market outcomes are negligible. Neither income, nor working hours were affected by the extension of the working age. Also, the probability that a woman is an absentee, which could admittedly increase when requiring women to stay employed beyond their initial expectations, is not affected.

Before we discuss in more detail these DDM estimates, we shift our focus to the choice of the covariates used in the estimation of the propensity score and also to the plausibility of the assumption underlying the matching estimator. The choice of the variables in the specification of the probit model observed the basic principle that they should influence both the selection-into-treatment (to remain on the job) and the outcome variables. Thus, the variables included (see Table 5) are: potential experience and current job tenure and their quadratic terms, year dummies and (log) sales - to control for economy-wide and firm-specific shocks -, education level dummies, and sector of activity and regional dummies.[5] While the latter two sets of variables might influence more the outcome variable, clearly the other variables are simultaneously important in determining the decision to remain employed and the outcome variable.

The focus of Table 5 is, however, on the balancing properties of the matching procedure. For this purpose, we present a plethora of statistics, namely, the mean for the treatment and control groups for the unmatched and matched samples, the standardized bias measure suggested by Rosenbaum & Rubin (1985), and the joint significance tests and pseudo-R^2 of the propensity score (probit model) estimation (Sianesi 2004). This table illustrates the importance of matching and its success. While before the kernel-based matching procedure the treatment and control groups exhibited clear differences (e.g. tenure differed by about 2 years), after matching these are reduced to statistical insignificance. This is also confirmed by the reduction obtained in the standardized bias and, finally, by the joint statistical significance of the covariates and by the pseudo-R^2 of the propensity score in the unmatched and matched samples estimation procedures. As it can be seen in the last two rows of Table 5, the pseudo-R^2 in the propensity score estimation that used only the treated units and the corresponding matched control units falls to values close to zero. The F-test complements this information, corroborating the view that matching has successfully eliminated any systematic observable

[5]This table refers only to the propensity score matching procedure for the 'after' period. Similar testing schemes were conducted for the other components of the DDM estimator with overall results qualitatively identical. The full set of results is available from the authors upon request.

differences between the treated and control groups.

With regards to the DDM estimates, we present two estimates, depending on the use of unbalanced panel data (which we treat as repeated cross sections) or balanced panel data. The researcher has typically these two options, and the choice of one over the other hinges on the question to be answered. In the present case, as women had access to early retirement schemes, one cannot exclude the possibility that, faced with unexpected extensions of their careers, some of them opted for such retirement schemes. Thus, by opting for the balanced panel data, we are in fact looking exclusively over time at those who (as expected by the legislator) extended their careers. For the present case, Table 4 reports these alternative estimates and both are statistically not different from zero.

To check on the sensitivity of our point estimates to the definition of the non-experimental control group, we consider two alternative definitions of control units. The obvious choice to compare women would be other women. This, however, raises difficulties in the current setting because all women younger than 62 years were affected by the new legislation. Therefore, we have one obvious choice - women older than 62 in 1993 - and a less obvious and, indeed potentially endogenous - younger women who did not have to postpone retirement in the 1994-1999 transition period. The last column of Table 4 presents the results. We find that the conclusions do not depend on the choice of the control group. Neither income, nor working hours were affected by the postponing of the retirement age. The same is true for the probability of being absent from work.

6.2 Impact on personnel policies

We consider three different matching variables: the propensity score (from a probit regression), the predicted number of women affected by the new legislation (from a Poisson regression - see Section 3.2), and the estimated probability that a firm employed at least one woman affected (again, from a Poisson regression). Our models are run in within-firm differences, by taking as the dependent variable the difference between the value of the variable for each year and the value of the same variable in 1992.

The matching method used is kernel matching.[6] We also impose the common support. The propensity score (and the alternative matching variables based on Poisson regressions)

[6] We have checked the robustness of the results using nearest neighbour matching and the results (available upon request) are very similar.

are estimated using a very large set of variables: a cubic in firm size (measured in terms of the number of workers), five dummies for firm size ranges, a quadratic in the share of women in the workforce, a cubic in the average total pay per worker, a cubic in the average total number of hours worked, a quadratic in the percentage of workers that are men aged 60 or more, the shares of voting rights held by domestic (non public) and foreign investors, 57 industry dummies and 29 region dummies. The sample is also restricted to firms with 100 or less employees, as we found that some large firms that employed a very large number of older women were very difficult to match;[7] i.e. it is difficult to find large firms that do not employ at least one woman affected by the new LRA.

We assess the treatment effects in terms of three main different labour market variables: hirings, separations, and net job creation. Net job creation at year t is defined as the difference in the total number of workers between year t and year $t-1$ in each firm. Hirings at year t are defined as the number of workers that are hired since year t up to year $t-1$. Separations are defined as the difference between hirings and net job creation.[8] Finally, in order to shed more light into any possible patterns resulting from the change in retirement age, we also decompose the total level of hirings into four groups of workers. These groups are defined according to the gender and the age (25 or younger; and older than 25) of the worker hired.

Because we are interested in understanding the net impact of the reform, all variables (hirings, separations and net job creation) are considered in a cumulative way, when they refer to the 'after' period. Specifically, each variable results from summing the flow of the year under analysis and the flow of the same variable for all years in the treatment period up to that year under analysis. For instance, when we refer to the impact on hirings in 1997, we are comparing the sum of hirings in 1995, 96 and 97 in the treated group with the same sum for the control group. Moreover, because the analysis is based on a difference-in-differences approach, each sum of flows is subtracted by the level of that flow in 1992.

We then consider three different periods over which we carry out this aggregation of flows: only 1995 (the first year that falls exclusively during the 'after' period), 1995 to 1997 and 1995 to 1999. These periods have been chosen in order to establish a correspondence between the

[7] This restriction eliminates only less than 2% of our sample, given the relatively low average size of firms in Portugal. We also checked that our results are robust to other cut-off thresholds (results available upon request).

[8] Our method of counting worker flows based on annual data implies that we may underestimate hirings and separations, as we cannot track workers that are hired after the census month in year $t-1$ and that then separate before the census month in year t. However, these short-term flows are not important from our point of view in this paper.

different criteria that assign firms to either the treatment or the control groups. As mentioned before, strictly speaking all firms with at least one female employee in 1992 will be (directly) affected by the increase of the mandatory age of retirement, to the extent that, under the new law, such firms will be forced to retain those workers for a longer period than expected when the worker was hired.

Tables 6 and 7 present the results concerning the impact of the higher LRA in terms of firm-level job and worker flows. As mentioned before, we consider three different matching variables. Moreover, for the benefit of robustness, we also consider two different sample definitions: all firms present in each year since 1991 until 1999 (Table 6) and only firms present in all years since 1991 until 1999 (Table 7). The advantage of the first definition (which leads to a decreasing sample size over time, due to firm exits) is that its effects will be more representative of the entire economy. On the other hand, the advantage of the second definition is that it rules out any possible impacts of compositional changes in the pool of firms analysed, as the same firms are followed over time.

In the two tables, the column indicating the period to which the estimate refers also indicates the criterion adopted to define the control and the treatment groups. The correspondence between the period range examined and the definition of the treatment group is designed to allow us to study the impact on worker flows over the same period in which the law was binding in terms of preventing the older women employees from retiring. For instance, estimates for the period 1995-99 are also based on a treatment group made up of firms that employed in 1992 at least one woman aged 55 to 60 (while the control group corresponds to firms that employed zero women aged 55 to 60 in 1992). Similarly, estimates for 1995-97 are based on treatment group firms employing at least one woman aged 57-60 in 1992. Finally, estimates for 1995 only are based on treatment groups firms employing at least one woman aged 59-60 in 1992. Also, recall that each number under the column 'ATT' corresponds to a separate estimate from a different matching analysis.

The main result that emerges from these tables is that hirings and separations fall significantly for the treated firms. The findings are also remarkably similar for different samples and matching methods. For instance, when considering the impact of the new law upon cumulative hirings over the 1995-99 period, the estimated effects range from -2.39 (SE=0.54; Propensity Score - Probit, continuing firms - see the bottom third of Table 7) and -3.51

(SE=0.48; Predicted N, all firms - see the top third of Table 6). A similar comparison in the case of cumulative separations, again over the 1995-99 period, indicates a range of estimates from -1.54 (Propensity Score - Probit, continuing firms) to -2.42 (Predicted N, all firms). Similar narrow ranges of significant estimates can be documented for cumulative net job creation levels (from -0.80 to -0.99) and for different time periods.

Another important result from Tables 6 and 7 is that wider periods of analysis and wider definitions of the treatment group translate into bigger effects. For instance, in Table 6, the effect on hirings under Predicted N matching goes from -0.04 in 1995 to -1.12 in 1995-97 to -3.51 in 1995-99. In order to place these and other estimates in context, it is important to mention that, in each treated firm in 1992, the number of older women (old enough to assign the firm to the treatment group) is between around 1 (for the 1995-97 period) and around 1.5 (for the 1995-99 period) across different sample definitions (all firms or only continuing firms). Taking also into account the range of estimates documented for hirings in 1995-97 and 1995-99, we conclude that the impact of the new law was of decreasing hirings by between one and two workers for each older worker retained in the firm.

In terms of separations, the range of relative effects (change in separations per retained worker) is broadly similar, although of a smaller magnitude and thus closer to the one-to-one relationship expected given the earlier evidence that the law was binding. Moreover, given the identity connecting hirings, separations and net job creation, the smaller decrease in separations than in hirings implies that net job creation will fall in the treatment group with respect to the control group, as documented in our results. We also find that, the longer the time range considered (e.g. 1995-1999 vs. 1995 only), the stronger the relative impact of the law in terms of decreased hirings, separations, or net job creation.

The only results in the two tables that are not significant are those that refer to the year of 1995 only. In other words, when the treatment group is made up of firms with at least one woman aged 59-60 in 1992, there are no significant differences in the 1995 hirings, separations or net job creation levels between the treatment and the control groups. This result is most likely due to the narrowness of the treatment group and, to a smaller extent, to the fact that, in that case, the 'after' period is only one year after the year in which the new law came into force.

We also carry out balancing tests for our estimates, in order to check some of the assumptions underlying the matching method. Table 8 presents the results for the specification underpinning the treatment group based on women aged 57-60 and the dependent variable referring the cumulative impacts over the 1995-1997 period. We see that the matched sample leads to a much greater equality of the observables across the treatment and the control groups. In the few cases that the t-test of the equality of the means of the two groups is rejected at the standard levels of significance, the economic difference between the two groups is particularly small. We also checked the results for the remaining specifications and all results are very similar (available upon request).

Finally, as mentioned above, we decompose the effect of the law upon hirings in terms of four different demographic groups (female workers aged 25 or less, male workers aged 25 or less, female workers aged 26 or more, male workers aged 26 or more) that may have been affected differently. A subset of the results are presented in Tables A.1 to A.6.[9]

The results indicate that the negative effect upon hirings is concentrated upon younger workers and, in particular, upon younger women. For instance, Table A.1 (based on Propensity Score - Probit matching and a treatment group of firms employing at least one woman aged 57-60 in 1992), indicates that the total effect upon cumulative hirings from 1995 to 1997 was -1.01 (SE=0.27). This figure is decomposed into an effect upon hirings of women aged 25 or less of -0.46 (SE=0.08) while the effect upon hirings of men aged 25 or less is -0.28 (SE=0.07). The equivalent effects upon women and men older than 25 are, respectively, -0.15 (SE=0.12) and -0.12 (SE=0.10).

6.3 Impact on firm performance

As argued in the Introduction, it is possible that firms' performance is negatively affected by the additional constraint imposed on their personnel policies when the LRA is increased. We assess this hypothesis by extending the framework we used for job and worker flows, considering now the impact of the treatment in terms of different measures of firm performance. These measures are: sales, "net" sales (sales minus the wage bill), sales per worker and "net" sales per worker. Unlike before, each one of these variables is now measured in a single period (the

[9]These tables also provide additional information with respect to Tables 6 and 7, namely the effects across the different dependent variables, sample definitions and matching methods, for all time period ranges, from 1995 to 1995-1999 - and not only the time period 'directly' related to the definition of the treatment group. This breadth in terms of the time periods covered comes at the cost of considering only one definition of the treatment group, namely that of firms with at least one woman aged 57-60 in 1992. However, we have checked that the same patterns hold for the remaining definitions of the treatment group.

last year of the range of years considered for the definition of the treatment/control groups).

Again, we consider three different matching variables: the propensity score (from a probit regression and from a Poisson regression) and the predicted number of women affected by the new legislation (from a Poisson regression). Tables 9 and 10 present the results, first for all firms and then only for firms that can be followed over each given period.

We find that there are relatively large and very significant effects in terms of sales and "net" sales. The figures, across the different specifications and estimation methods, range between -6pp and -9pp for the 1995 and 1995-1997 periods and -12 to -14 for the 1995-1999 periods, always with t-ratios above 2. However, when considering the effects in terms of sales per worker, we find no significant differences between the treatment and the control groups. Moreover, the estimates are also particularly small. The absence of a negative effect in terms of sales per worker is related to the relative decrease in firm size observed in the firms affected by the increase in the LRA.

6.4 Discussion

One possible explanation why the effect upon separations can be slightly higher than the average number of women retained in firms due to the new law is related to the evidence about lower hirings in treated firms. To the extent that new employer-employee matches are more likely to dissolve, firms that are hiring fewer workers will also be likely to exhibit fewer separations in the following periods.

A similar argument may also be useful in explaining the fact that the ratio between retained workers and the reduction in hirings may exceed one (it can be as big as two in some cases). If employers may anticipate that a relatively high share of new hires will not become good matches, then employers may decide to hire more than one worker for each vacancy. This would generate the result documented, as fewer vacancies related to older workers retained by the new law lead to a more than proportional decline in hirings. Another possible explanation for this apparently high ratio between retained workers and hirings concerns the possible scale effects induced by the higher costs faced by firms. As firing costs increase and/or firms are forced to pay wages above productivity for a longer period than initially expected, firms may be forced to hire fewer workers.

The different levels of human capital of older and new workers may also play some role in a more than proportional relationship between affected workers and the reduction in hirings. To the extent that the former are more qualified than the latter, firms may typically need to hire more than one new worker for each older worker they need to replace.

7 Conclusions

Increasing the mandatory retirement age has been considered an important policy to address the financial sustainability difficulties of pay-as-you-go, defined-benefit pension systems that follow population ageing. Although the success of any such policy is essentially determined at the labour market, our paper is the first to examine how firms adjust their personnel policies when forced to retain their older workers longer than initially expected.

We present quasi-experimental evidence on such response by firms, by examining the impact of a 1993 Portuguese law that increased the retirement age of women while leaving unchanged the retirement age of men. Using matched employer-employee panel data and difference-in-differences matching methods, we compare firms that, before the law was announced, employed women old enough to be (immediately or soon) affected by the new law with otherwise very similar firms but that did not employ any such women.

After checking that firms did indeed comply with the law, we find that the monthly wages and the hours of the affected women were virtually unchanged. Moreover, we also find that 'treated' firms did significantly reduce their worker flows (hirings and separations). In our preferred specifications, the results indicate that firms hire approximately one to two fewer workers for each older worker that is retained due to the higher mandatory retirement age. Moreover, we also find that younger workers and, in particular, younger women are the demographic groups most affected by the lower level of total hirings.

The result about lower hirings suggests that the contribution of higher retirement ages to the sustainability of pensions may be weaker than previously assumed, at least over the short run. On the one hand, the period over which older workers contribute to the pension system is extended while the period over which they collect payments is diminished. However, on the other hand, the contributions from younger workers to the pension system may fall and unemployment benefit payments may rise.

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Table 1: Treatment groups: Before and after the new retirement age

	Treatment groups by age sets (Before=1992)						
Year:	1992	1994	1995	1996	1997	1998	1999
LRA:	62	62.5	63	63.5	64	64.5	65
	[57.5, 58)						[64.5, 65)
	[58, 58.5)					[64, 64.5)	
	[58.5, 59)				[63.5, 64)		
	[59, 59.5)			[63, 63.5)			
	[59.5, 60)		[62.5, 63)				
	[60, 60.5)	[62, 62.5)					

Notes: (1) Treatment group: The set of individuals (women) who would have retired in year t if the legal retirement age (LRA) had remained at its value of year $t-1$. For example, women in the age group [60, 60.5) in 1992 would have retired in 1994 if the LRA had remained at 62 years; (2) Before period: the 'before' is always set to 1992, when the women's LRA was 62 years and no legislative change was expected.

Table 2: labour Force Status: 1992-2000 or 1992 and 2000 only (Logit results)

Regressor	Labour Force Status: Employment	Labour Force Status: Inactivity	Labour Force Status: Employment	Labour Force Status: Inactivity
Gender (Female=1)	-1.242	1.333	-1.289	1.355
	(0.010)	(0.010)	(0.021)	(0.021)
Age Group	-0.047	0.072	-0.108	0.139
	(0.028)	(0.028)	(0.061)	(0.061)
Treated Group	0.272	-0.327	0.389	-0.420
	(0.031)	(0.031)	(0.067)	(0.068)
Number of observations	229,066	229,066	49,701	49,701
Wald test	28,553.6	31,525.4	6,158.3	6,636.2

Source: *Inquérito ao Emprego*. The specification includes 17 age and 8 year dummies (pr one year dummy in the case of the last two columns). Standard errors in parenthesis.

Table 3: Transition from Employment into Inactivity (Cox Hazard Model with Time-Varying Covariates)

Regressor	
Gender (Male=1)	0.309
	(0.069)
Age Group	0.338
	(0.298)
Age Group × Female	-0.311
	(0.177)
Age Group × After	-0.220
	(0.210)
Age Group × After × Female	0.724
	(0.309)
Number of observations	1,167
Wald test	47.3

Source: *Inquérito ao Emprego*. The specification includes 8 year dummies. Standard errors in parenthesis.

Table 4: labour market outcomes: Impact on postponed women retirees' total income, working hours and probability of working

	D-in-D			
			Matching	
Variable	unrest.[1]	rest.[2]	c. section[3]	panel[4]
Log earnings	0.008	-0.015	0.005	0.008
	(0.013)	(0.010)	(0.015)	(0.011)
	52,120	52,120	53,570	10,204
	0.08	0.44	-	-
Log hours	-0.033	-0.028	-0.026	0.009
	(0.007)	(0.007)	(0.010)	(0.009)
	50,628	50,628	52,166	9,823
	0.04	0.08	-	-
Pr(Absentee)[5]	-	-	-0.011	-
	-	-	(0.009)	-
	-	-	66,811	-
	-	-	-	-

Notes: The values reported for each pair variable and estimator are point estimate, standard error, number of observations and R^2. (1) The D-in-D unrestricted estimator does not control for confounding factors; (2) The OLS D-in-D restricted estimator is based on a linear specification, controlling for observable characteristics; (3) DDM estimator with kernel matching on the propensity score with repeated cross-section data; (4) DDM estimator with kernel matching on the propensity score with balanced panel data. The set of variables used with the estimation of the propensity score and in the restricted OLS D-in-D estimator are reported in **??**. (5) It refers to the probability that a employee although registered in QP is reported as having worked zero hours, and (s)he is taken as absentee.

Table 5: Balancing properties of the kernel based propensity score matching for the unbalanced panel data in the after period

		Unbalanced panel data (as repeated cross-section)				
				After		
		Mean		t-test		Reduction
Variable	Sample	Treated	Control	p-value$^{(1)}$	% bias$^{(2)}$	|bias|
Experience	Unmatched	52.44	52.19	0.000		
	Matched	52.43	52.34	0.163	3	62.6
Experience^2	Unmatched	2759.20	2734.40	0.000		
	Matched	2758.40	2749.30	0.162	3	63.2
Tenure	Unmatched	15.58	17.80	0.000		
	Matched	15.60	15.51	0.704	0.8	95.6
Tenure^2	Unmatched	381.64	484.89	0.000		
	Matched	382.44	382.51	0.994	0	99.9
Total sales	Unmatched	7.02	7.78	0.000		
	Matched	7.03	7.09	0.227	-2.5	92.1
Education:						
High school	Unmatched	0.03	0.03	0.093		
	Matched	0.03	0.04	0.739	-0.8	73.9
College	Unmatched	0.03	0.04	0.001		
	Matched	0.03	0.03	0.402	-1.7	71.2
Year dummies:						
1994	Unmatched	0.17	0.18	0.191		
	Matched	0.17	0.16	0.471	1.5	33.6
1995	Unmatched	0.20	0.20	0.603		
	Matched	0.20	0.20	0.765	-0.6	29.5
1996	Unmatched	0.14	0.15	0.006		
	Matched	0.14	0.14	0.746	-0.7	86
1997	Unmatched	0.18	0.17	0.071		
	Matched	0.18	0.18	0.824	0.5	84.6
1998	Unmatched	0.15	0.14	0.246		
	Matched	0.15	0.15	0.818	-0.5	75.1
1999	Unmatched	0.17	0.16	0.107		
	Matched	0.17	0.17	0.909	-0.2	91.1
Observations:						
On common support		4,324	13,259			
Off common support		12	0			
		Unmatched	Matched			
|Bias| summary statistics:						
Mean		8.82	1.01			
Std. Dev.		11.34	0.95			
Maximum		56.93	3.70			
Minimum		0.34	0.01			
Pseudo $R^{2(3)}$		0.135	0.002			
Joint F-test, p-value		0.000	0.998			

Notes: The table does not exhaustively list all variables included in the probit model used to estimate the propensity scores; we omit from the table the balancing property of sector of activity and regional dummy variables. (1) The p-value of the t-test for the equality of means in the treated and control groups, both before and after matching. (2) Bias is the standardized bias as suggested by Rosenbaum & Rubin (1985) reported together with the achieved percentage reduction in |bias|. (3) Pseudo R^2 from the probit model estimation of the propensity scores, including all variables reported above, before and after the matching process (Sianesi 2004).

Table 6: Average treatment effects on hirings, separations, and net job flows (all firms)

Matching	*Flow*	*Year*	*ATT*	*SE(ATT)*	*Treated*	*Control*
Predicted N - Poisson						
	H	1995	-0.04	0.18	1394	75675
	H	1995-97	-1.23	0.26	3083	54395
	H	1995-99	-3.51	0.48	4015	40214
	S	1995	-0.30	0.22	1394	75675
	S	1995-97	-0.85	0.27	3083	54395
	S	1995-99	-2.42	0.39	4015	40214
	NJC	1995	0.26	0.24	1394	75675
	NJC	1995-97	-0.38	0.20	3083	54395
	NJC	1995-99	-1.09	0.25	4015	40214
Propensity Score - Poisson						
	H	1995	-0.04	0.18	1394	75675
	H	1995-97	-1.10	0.28	3110	54395
	H	1995-99	-2.86	0.49	4052	40214
	S	1995	-0.30	0.22	1394	75675
	S	1995-97	-0.75	0.28	3110	54395
	S	1995-99	-1.89	0.41	4052	40214
	NJC	1995	0.26	0.24	1394	75675
	NJC	1995-97	-0.35	0.21	3110	54395
	NJC	1995-99	-0.97	0.25	4052	40214
Propensity Score - Probit						
	H	1995	-0.06	0.18	1396	75341
	H	1995-97	-1.01	0.27	3107	54323
	H	1995-99	-2.59	0.49	4048	40197
	S	1995	-0.28	0.22	1396	75341
	S	1995-97	-0.66	0.27	3107	54323
	S	1995-99	-1.64	0.40	4048	40197
	NJC	1995	0.22	0.25	1396	75341
	NJC	1995-97	-0.36	0.21	3107	54323
	NJC	1995-99	-0.95	0.25	4048	40197

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 59-60, 57-60 or 55-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively. *H, S, NJC* indicate, respectively, total hirings, separations, net job creation. The matching variable is the predicted number of affected women from a Poisson regression; is equivalent to the propensity score but the estimates are obtained from the Poisson regression; refers to the conventional propensity score method.

Table 7: Average treatment effects on hirings, separations, and net job flows (continuing firms)

Matching	*Flow*	*Year*	*ATT*	*SE(ATT)*	*Treated*	*Control*
Predicted N - Poisson						
	H	1995	-0.31	0.24	663	34735
	H	1995-97	-0.90	0.32	1944	33433
	H	1995-99	-3.30	0.53	3329	32037
	S	1995	0.03	0.26	663	34735
	S	1995-97	-0.49	0.28	1944	33433
	S	1995-99	-2.31	0.42	3329	32037
	NJC	1995	-0.34	0.32	663	34735
	NJC	1995-97	-0.42	0.23	1944	33433
	NJC	1995-99	-0.99	0.26	3329	32037
Propensity Score - Poisson						
	H	1995	-0.33	0.24	663	34735
	H	1995-97	-0.80	0.34	1965	33433
	H	1995-99	-2.66	0.54	3359	32037
	S	1995	0.03	0.26	663	34735
	S	1995-97	-0.36	0.29	1965	33433
	S	1995-99	-1.86	0.43	3359	32037
	NJC	1995	-0.36	0.32	663	34735
	NJC	1995-97	-0.44	0.23	1965	33433
	NJC	1995-99	-0.80	0.27	3359	32037
Propensity Score - Probit						
	H	1995	-0.38	0.25	664	34584
	H	1995-97	-0.86	0.33	1964	33392
	H	1995-99	-2.39	0.54	3358	32021
	S	1995	0.07	0.27	664	34584
	S	1995-97	-0.37	0.29	1964	33392
	S	1995-99	-1.54	0.43	3358	32021
	NJC	1995	-0.45	0.34	664	34584
	NJC	1995-97	-0.50	0.23	1964	33392
	NJC	1995-99	-0.85	0.27	3358	32021

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 59-60, 57-60 or 55-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively. *H, S, NJC* indicate, respectively, total hirings, separations, net job creation. The matching variable is the predicted number of affected women from a Poisson regression; is equivalent to the propensity score but the estimates are obtained from the Poisson regression; refers to the conventional propensity score method.

Table 8: Balancing properties of the kernel based propensity score matching (all firms)

Variable	Sample	Mean Treated	Control	t-test	Reduction |bias|	% bias$^{(2)}$	*t*-test p-value$^{(1)}$
Firm Size	Unmatched	22.874	94.189	69.6		52.25	0.000
	Matched	22.779	21.028	9.1	87.0	2.91	0.004
Firm Size^2	Unmatched	10.974	26.209	53.2		45.28	0.000
	Matched	10.869	99.991	5.5	89.6	1.72	0.086
Firm Size^3	Unmatched	71.494	13.458	43.8		39.05	0.000
	Matched	70.425	65.676	3.6	91.8	1.09	0.275
Wages	Unmatched	56376	47278	27.4		15.25	0.000
	Matched	56320	54679	4.9	82.0	1.89	0.058
Wages^2	Unmatched	4.4e+07	3.3e+07	13.6		6.93	0.000
	Matched	4.3e+07	4.2e+07	2.4	82.5	1.01	0.310
Wages^3	Unmatched	4.7e+10	3.7e+10	2.8		1.23	0.219
	Matched	4.6e+10	4.4e+10	0.6	77.3	0.35	0.725
Men 60+	Unmatched	.06051	.0388	21.6		10.96	0.000
	Matched	.06052	.0598	0.7	96.7	0.29	0.769
(Men 60+)^2	Unmatched	.01222	.01323	-1.9		-0.84	0.402
	Matched	.01223	.01354	-2.5	-30.3	-1.36	0.175
Female	Unmatched	.54384	.36865	56.3		27.93	0.000
	Matched	.54368	.51388	9.6	83.0	4.13	0.000
Female^2	Unmatched	.37134	.25394	35.8		19.05	0.000
	Matched	.37123	.35002	6.5	81.9	2.65	0.008
Domestic Capital	Unmatched	769.62	667.85	22.8		11.81	0.000
	Matched	770.29	737.66	7.3	67.9	3.00	0.003
Foreign Capital	Unmatched	93.674	54.861	4.8		2.98	0.003
	Matched	93.795	95.058	-0.2	96.7	-0.05	0.957
Hours	Unmatched	132.55	129.43	7.0		3.59	0.000
	Matched	132.57	130.9	3.7	46.6	1.54	0.124
Hours^2	Unmatched	192.79	190.02	2.8		1.42	0.156
	Matched	192.84	190.57	2.3	18.0	0.95	0.340
Hours^3	Unmatched	292.15	294.55	-1.2		-0.61	0.544
	Matched	292.27	289.91	1.2	2.0	0.50	0.616

Notes: The table does not exhaustively list all variables included in the probit model used to estimate the propensity scores; we omit from the table the balancing property of sector of activity and regional dummy variables. (1) The *p*-value of the *t*-test for the equality of means in the treated and control groups, both before and after matching. (2) Biasis the standardized bias as suggested by Rosenbaum & Rubin (1985) reported together with the achieved percentage reduction in |bias|.

Table 9: Average treatment effects on different measures of firm performance (all firms)

Matching	*Flow*	*Year*	*ATT*	*SE(ATT)*	*Treated*	*Control*
Predicted N - Poisson						
	Sales	1995	-8.19	3.07	1293	79462
	Sales	1995-97	-8.49	2.68	3364	65553
	Sales	1995-99	-11.19	2.72	5324	57310
	Net sales	1995	-7.99	2.95	1293	79462
	Net sales	1995-97	-7.58	2.54	3364	65553
	Net sales	1995-99	-9.19	2.55	5324	57310
	Sales pw	1995	-0.35	0.23	1307	79086
	Sales pw	1995-97	0.04	0.18	3392	65278
	Sales pw	1995-99	-0.09	0.17	5379	57084
	Net sales pw	1995	-0.35	0.23	1307	79086
	Net sales pw	1995-97	0.04	0.18	3392	65278
	Net sales pw	1995-99	-0.08	0.16	5379	57084
Prop. Score - Poisson						
	Sales	1995	-8.21	3.08	1295	79819
	Sales	1995-97	-8.75	2.72	3367	65634
	Sales	1995-99	-12.23	2.72	5312	57330
	Net sales	1995	-7.97	2.96	1295	79819
	Net sales	1995-97	-6.98	2.57	3367	65634
	Net sales	1995-99	-10.09	2.55	5312	57330
	Sales pw	1995	-0.33	0.24	1309	79439
	Sales pw	1995-97	0.10	0.18	3395	65354
	Sales pw	1995-99	-0.03	0.17	5368	57104
	Net sales pw	1995	-0.34	0.24	1309	79439
	Net sales pw	1995-97	0.10	0.18	3395	65354
	Net sales pw	1995-99	-0.03	0.16	5368	57104
Prop. Score - Probit						
	Sales	1995	-8.16	3.08	1295	79819
	Sales	1995-97	-8.47	2.69	3342	65634
	Sales	1995-99	-12.63	2.74	5310	57330
	Net sales	1995	-7.89	2.97	1295	79819
	Net sales	1995-97	-7.13	2.55	3342	65634
	Net sales	1995-99	-10.26	2.58	5310	57330
	Sales pw	1995	-0.33	0.24	1309	79439
	Sales pw	1995-97	0.09	0.18	3376	65354
	Sales pw	1995-99	-0.04	0.17	5359	57104
	Net sales pw	1995	-0.34	0.24	1309	79439
	Net sales pw	1995-97	0.10	0.18	3376	65354
	Net sales pw	1995-99	-0.04	0.17	5359	57104

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects and estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 59-60, 57-60 or 55-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively. *Net sales* are total sales minus the wage bill. The matching variable "number of women" is the predicted number of affected women from a Poisson regression; "Pr($t \geq 1$ woman)" is equivalent to the propensity score but the estimates are obtained from the Poisson regression; "propensity score" refers to the conventional propensity score method.

Table 10: Average treatment effects on different measures of firm performance (continuing firms)

Matching	*Flow*	*Year*	*ATT*	*SE(ATT)*	*Treated*	*Control*
Predicted N - Poisson						
	Sales	1995	-9.44	3.45	805	48814
	Sales	1995-97	-6.37	2.94	2436	47328
	Sales	1995-99	-12.23	2.89	4222	45559
	Net sales	1995	-11.95	3.28	781	46923
	Net sales	1995-97	-6.81	2.60	2341	45501
	Net sales	1995-99	-10.94	2.57	4060	43797
	Sales pw	1995	-0.26	0.23	781	46923
	Sales pw	1995-97	0.03	0.18	2341	45501
	Sales pw	1995-99	-0.08	0.17	4060	43797
	Net sales pw	1995	-0.27	0.23	781	46923
	Net sales pw	1995-97	0.05	0.18	2341	45501
	Net sales pw	1995-99	-0.06	0.17	4060	43797
Prop. Score - Poisson						
	Sales	1995	-9.41	3.46	807	49015
	Sales	1995-97	-7.00	3.01	2445	47377
	Sales	1995-99	-13.29	2.89	4211	45574
	Net sales	1995	-11.88	3.28	783	47114
	Net sales	1995-97	-6.11	2.64	2350	45547
	Net sales	1995-99	-11.79	2.57	4050	43812
	Sales pw	1995	-0.24	0.23	783	47114
	Sales pw	1995-97	0.07	0.19	2350	45547
	Sales pw	1995-99	-0.02	0.17	4050	43812
	Net sales pw	1995	-0.26	0.23	783	47114
	Net sales pw	1995-97	0.12	0.19	2350	45547
	Net sales pw	1995-99	-0.01	0.17	4050	43812
Prop. Score - Probit						
	Sales	1995	-9.39	3.46	807	49015
	Sales	1995-97	-6.55	2.97	2422	47377
	Sales	1995-99	-13.68	2.92	4209	45574
	Net sales	1995	-11.76	3.29	783	47114
	Net sales	1995-97	-5.80	2.61	2328	45547
	Net sales	1995-99	-11.79	2.58	4042	43812
	Sales pw	1995	-0.25	0.23	783	47114
	Sales pw	1995-97	0.08	0.18	2328	45547
	Sales pw	1995-99	-0.01	0.17	4042	43812
	Net sales pw	1995	-0.26	0.23	783	47114
	Net sales pw	1995-97	0.12	0.18	2328	45547
	Net sales pw	1995-99	-0.01	0.17	4042	43812

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment on the treated in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects and estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 59-60, 57-60 or 55-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively. *Net sales* are total sales minus the wage bill. The matching variable "number of women" is the predicted number of affected women from a Poisson regression; "Pr($t \geq 1$ woman)" is equivalent to the propensity score but the estimates are obtained from the Poisson regression; "propensity score" refers to the conventional propensity score method.

Table A.1: Average treatment effect on hirings, separations, and net job flows (all firms). Matching variable: Propensity score. Treatment group: firms that employed at least one woman aged 57-60 in 1992

Flow	*Year*	*ATT*	*SE(ATT)*	*Women*	*Treated*	*Control*
Hirings						
	1995	0.25	0.09	1.17	4118	72842
	1995-96	-0.27	0.16	1.17	3558	61875
	1995-97	-1.01	0.27	1.18	3107	54323
	1995-98	-1.22	0.41	1.18	2728	47777
	1995-99	-1.65	0.55	1.18	2355	41862
Separations						
	1995	-0.27	0.13	1.17	4118	72842
	1995-96	-0.40	0.18	1.17	3558	61875
	1995-97	-0.66	0.27	1.18	3107	54323
	1995-98	-0.90	0.37	1.18	2728	47777
	1995-99	-0.89	0.50	1.18	2355	41862
Net Job Creation						
	1995	0.52	0.14	1.17	4118	72842
	1995-96	0.13	0.16	1.17	3558	61875
	1995-97	-0.36	0.21	1.18	3107	54323
	1995-98	-0.32	0.25	1.18	2728	47777
	1995-99	-0.76	0.28	1.18	2355	41862
Hirings (men 25 or younger)						
	1995	-0.03	0.03	1.17	4118	72842
	1995-96	-0.14	0.05	1.17	3558	61875
	1995-97	-0.28	0.07	1.18	3107	54323
	1995-98	-0.35	0.11	1.18	2728	47777
	1995-99	-0.47	0.14	1.18	2355	41862
Hirings (women 25 or younger)						
	1995	0.13	0.03	1.17	4118	72842
	1995-96	-0.13	0.06	1.17	3558	61875
	1995-97	-0.46	0.08	1.18	3107	54323
	1995-98	-0.72	0.11	1.18	2728	47777
	1995-99	-1.03	0.14	1.18	2355	41862
Hirings (men older than 25)						
	1995	0.10	0.03	1.17	4118	72842
	1995-96	0.01	0.06	1.17	3558	61875
	1995-97	-0.12	0.10	1.18	3107	54323
	1995-98	-0.06	0.16	1.18	2728	47777
	1995-99	-0.01	0.22	1.18	2355	41862
Hirings (women older than 25)						
	1995	0.05	0.04	1.17	4118	72842
	1995-96	-0.01	0.07	1.17	3558	61875
	1995-97	-0.15	0.12	1.18	3107	54323
	1995-98	-0.09	0.18	1.18	2728	47777
	1995-99	-0.13	0.24	1.18	2355	41862

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 57-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively.

Table A.2: Average treatment effect on hirings, separations, and net job flows (continuing firms). Matching variable: Propensity score. Treatment group: firms that employed at least one woman aged 57-60 in 1992

Flow	*Year*	*ATT*	*SE(ATT)*	*Women*	*Treated*	*Control*
Hirings						
	1995	0.05	0.13	1.19	1964	33392
	1995-96	-0.22	0.21	1.19	1964	33392
	1995-97	-0.86	0.33	1.19	1964	33392
	1995-98	-1.32	0.47	1.19	1964	33392
	1995-99	-1.50	0.58	1.19	1964	33392
Separations						
	1995	-0.20	0.14	1.19	1964	33392
	1995-96	-0.40	0.20	1.19	1964	33392
	1995-97	-0.37	0.29	1.19	1964	33392
	1995-98	-0.74	0.40	1.19	1964	33392
	1995-99	-0.65	0.53	1.19	1964	33392
Net Job Creation						
	1995	0.25	0.16	1.19	1964	33392
	1995-96	0.17	0.19	1.19	1964	33392
	1995-97	-0.50	0.23	1.19	1964	33392
	1995-98	-0.58	0.26	1.19	1964	33392
	1995-99	-0.85	0.29	1.19	1964	33392
Hirings (men 25 or younger)						
	1995	-0.13	0.04	1.19	1964	33392
	1995-96	-0.20	0.06	1.19	1964	33392
	1995-97	-0.32	0.09	1.19	1964	33392
	1995-98	-0.48	0.12	1.19	1964	33392
	1995-99	-0.57	0.14	1.19	1964	33392
Hirings (women 25 or younger)						
	1995	0.05	0.04	1.19	1964	33392
	1995-96	-0.19	0.06	1.19	1964	33392
	1995-97	-0.54	0.10	1.19	1964	33392
	1995-98	-0.83	0.13	1.19	1964	33392
	1995-99	-0.98	0.15	1.19	1964	33392
Hirings (men older than 25)						
	1995	0.11	0.05	1.19	1964	33392
	1995-96	0.14	0.08	1.19	1964	33392
	1995-97	0.03	0.13	1.19	1964	33392
	1995-98	-0.01	0.19	1.19	1964	33392
	1995-99	-0.01	0.23	1.19	1964	33392
Hirings (women older than 25)						
	1995	0.02	0.07	1.19	1964	33392
	1995-96	0.03	0.10	1.19	1964	33392
	1995-97	-0.03	0.15	1.19	1964	33392
	1995-98	0.01	0.21	1.19	1964	33392
	1995-99	0.06	0.26	1.19	1964	33392

Notes: Source: *Quadros de Pessoal.* *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 57-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively.

Table A.3: Average treatment effect on hirings, separations, and net job flows (all firms). Matching variable: Poisson probability. Treatment group: firms that employed at least one woman aged 57-60 in 1992

Flow	*Year*	*ATT*	*SE(ATT)*	*Women*	*Treated*	*Control*
Hirings						
	1995	0.34	0.09	1.17	4120	72948
	1995-96	-0.33	0.17	1.17	3561	61965
	1995-97	-1.10	0.28	1.18	3110	54395
	1995-98	-1.26	0.41	1.18	2731	47835
	1995-99	-1.60	0.56	1.18	2358	41910
Separations						
	1995	-0.29	0.13	1.17	4120	72948
	1995-96	-0.46	0.18	1.17	3561	61965
	1995-97	-0.75	0.28	1.18	3110	54395
	1995-98	-0.96	0.37	1.18	2731	47835
	1995-99	-0.80	0.51	1.18	2358	41910
Net Job Creation						
	1995	0.62	0.14	1.17	4120	72948
	1995-96	0.13	0.17	1.17	3561	61965
	1995-97	-0.35	0.21	1.18	3110	54395
	1995-98	-0.31	0.25	1.18	2731	47835
	1995-99	-0.80	0.29	1.18	2358	41910
Hirings (men 25 or younger)						
	1995	-0.03	0.03	1.17	4120	72948
	1995-96	-0.16	0.05	1.17	3561	61965
	1995-97	-0.29	0.07	1.18	3110	54395
	1995-98	-0.34	0.11	1.18	2731	47835
	1995-99	-0.43	0.14	1.18	2358	41910
Hirings (women 25 or younger)						
	1995	0.15	0.04	1.17	4120	72948
	1995-96	-0.15	0.06	1.17	3561	61965
	1995-97	-0.50	0.08	1.18	3110	54395
	1995-98	-0.76	0.11	1.18	2731	47835
	1995-99	-1.06	0.14	1.18	2358	41910
Hirings (men older than 25)						
	1995	0.13	0.04	1.17	4120	72948
	1995-96	0.01	0.06	1.17	3561	61965
	1995-97	-0.10	0.10	1.18	3110	54395
	1995-98	-0.01	0.16	1.18	2731	47835
	1995-99	0.04	0.22	1.18	2358	41910
Hirings (women older than 25)						
	1995	0.09	0.04	1.17	4120	72948
	1995-96	-0.03	0.07	1.17	3561	61965
	1995-97	-0.21	0.12	1.18	3110	54395
	1995-98	-0.15	0.18	1.18	2731	47835
	1995-99	-0.16	0.24	1.18	2358	41910

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 57-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively.

Table A.4: Average treatment effect on hirings, separations, and net job flows (continuing firms). Matching variable: Poisson probability. Treatment group: firms that employed at least one woman aged 57-60 in 1992

Flow	*Year*	*ATT*	*SE(ATT)*	*Women*	*Treated*	*Control*
Hirings						
	1995	0.09	0.13	1.19	1965	33433
	1995-96	-0.20	0.21	1.19	1965	33433
	1995-97	-0.80	0.34	1.19	1965	33433
	1995-98	-1.21	0.48	1.19	1965	33433
	1995-99	-1.37	0.59	1.19	1965	33433
Separations						
	1995	-0.21	0.14	1.19	1965	33433
	1995-96	-0.39	0.20	1.19	1965	33433
	1995-97	-0.36	0.29	1.19	1965	33433
	1995-98	-0.69	0.41	1.19	1965	33433
	1995-99	-0.57	0.53	1.19	1965	33433
Net Job Creation						
	1995	0.29	0.16	1.19	1965	33433
	1995-96	0.19	0.19	1.19	1965	33433
	1995-97	-0.44	0.23	1.19	1965	33433
	1995-98	-0.51	0.26	1.19	1965	33433
	1995-99	-0.80	0.29	1.19	1965	33433
Hirings (men 25 or younger)						
	1995	-0.13	0.04	1.19	1965	33433
	1995-96	-0.19	0.06	1.19	1965	33433
	1995-97	-0.30	0.09	1.19	1965	33433
	1995-98	-0.43	0.12	1.19	1965	33433
	1995-99	-0.51	0.14	1.19	1965	33433
Hirings (women 25 or younger)						
	1995	0.07	0.04	1.19	1965	33433
	1995-96	-0.18	0.06	1.19	1965	33433
	1995-97	-0.54	0.10	1.19	1965	33433
	1995-98	-0.83	0.13	1.19	1965	33433
	1995-99	-0.99	0.15	1.19	1965	33433
Hirings (men older than 25)						
	1995	0.10	0.05	1.19	1965	33433
	1995-96	0.14	0.08	1.19	1965	33433
	1995-97	0.06	0.13	1.19	1965	33433
	1995-98	0.05	0.19	1.19	1965	33433
	1995-99	0.06	0.24	1.19	1965	33433
Hirings (women older than 25)						
	1995	0.04	0.07	1.19	1965	33433
	1995-96	0.04	0.10	1.19	1965	33433
	1995-97	-0.03	0.15	1.19	1965	33433
	1995-98	0.01	0.21	1.19	1965	33433
	1995-99	0.07	0.26	1.19	1965	33433

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 57-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively.

Table A.5: Average treatment effect on hirings, separations, and net job flows (all firms). Matching variable: Poisson N. Treatment group: firms that employed at least one woman aged 57-60 in 1992

Flow	*Year*	*ATT*	*SE(ATT)*	*Women*	*Treated*	*Control*
Hirings						
	1995	0.27	0.09	1.16	4092	72948
	1995-96	-0.37	0.16	1.16	3532	61965
	1995-97	-1.23	0.26	1.17	3083	54395
	1995-98	-1.43	0.39	1.17	2706	47835
	1995-99	-1.83	0.53	1.17	2334	41910
Separations						
	1995	-0.19	0.12	1.16	4092	72948
	1995-96	-0.41	0.18	1.16	3532	61965
	1995-97	-0.85	0.27	1.17	3083	54395
	1995-98	-1.05	0.36	1.17	2706	47835
	1995-99	-1.11	0.48	1.17	2334	41910
Net Job Creation						
	1995	0.46	0.14	1.16	4092	72948
	1995-96	0.04	0.16	1.16	3532	61965
	1995-97	-0.38	0.20	1.17	3083	54395
	1995-98	-0.38	0.24	1.17	2706	47835
	1995-99	-0.72	0.28	1.17	2334	41910
Hirings (men 25 or younger)						
	1995	-0.04	0.03	1.16	4092	72948
	1995-96	-0.16	0.04	1.16	3532	61965
	1995-97	-0.30	0.07	1.17	3083	54395
	1995-98	-0.33	0.11	1.17	2706	47835
	1995-99	-0.41	0.14	1.17	2334	41910
Hirings (women 25 or younger)						
	1995	0.16	0.03	1.16	4092	72948
	1995-96	-0.15	0.06	1.16	3532	61965
	1995-97	-0.51	0.08	1.17	3083	54395
	1995-98	-0.76	0.11	1.17	2706	47835
	1995-99	-1.06	0.14	1.17	2334	41910
Hirings (men older than 25)						
	1995	0.09	0.03	1.16	4092	72948
	1995-96	-0.01	0.06	1.16	3532	61965
	1995-97	-0.15	0.10	1.17	3083	54395
	1995-98	-0.08	0.15	1.17	2706	47835
	1995-99	-0.04	0.21	1.17	2334	41910
Hirings (women older than 25)						
	1995	0.05	0.04	1.16	4092	72948
	1995-96	-0.04	0.07	1.16	3532	61965
	1995-97	-0.28	0.11	1.17	3083	54395
	1995-98	-0.26	0.17	1.17	2706	47835
	1995-99	-0.32	0.22	1.17	2334	41910

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 57-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively.

Table A.6: Average treatment effect on hirings, separations, and net job flows (continuing firms). Matching variable: Poisson N. Treatment group: firms that employed at least one woman aged 57-60 in 1992

Flow	*Year*	*ATT*	*SE(ATT)*	*Women*	*Treated*	*Control*
Hirings						
	1995	0.08	0.13	1.18	1944	33433
	1995-96	-0.24	0.20	1.18	1944	33433
	1995-97	-0.90	0.32	1.18	1944	33433
	1995-98	-1.34	0.46	1.18	1944	33433
	1995-99	-1.53	0.57	1.18	1944	33433
Separations						
	1995	-0.15	0.14	1.18	1944	33433
	1995-96	-0.40	0.19	1.18	1944	33433
	1995-97	-0.49	0.28	1.18	1944	33433
	1995-98	-0.79	0.39	1.18	1944	33433
	1995-99	-0.73	0.51	1.18	1944	33433
Net Job Creation						
	1995	0.23	0.16	1.18	1944	33433
	1995-96	0.16	0.19	1.18	1944	33433
	1995-97	-0.42	0.23	1.18	1944	33433
	1995-98	-0.55	0.26	1.18	1944	33433
	1995-99	-0.79	0.29	1.18	1944	33433
Hirings (men 25 or younger)						
	1995	-0.13	0.04	1.18	1944	33433
	1995-96	-0.19	0.06	1.18	1944	33433
	1995-97	-0.29	0.09	1.18	1944	33433
	1995-98	-0.42	0.12	1.18	1944	33433
	1995-99	-0.49	0.14	1.18	1944	33433
Hirings (women 25 or younger)						
	1995	0.07	0.04	1.18	1944	33433
	1995-96	-0.18	0.06	1.18	1944	33433
	1995-97	-0.52	0.09	1.18	1944	33433
	1995-98	-0.82	0.13	1.18	1944	33433
	1995-99	-0.99	0.15	1.18	1944	33433
Hirings (men older than 25)						
	1995	0.08	0.05	1.18	1944	33433
	1995-96	0.10	0.08	1.18	1944	33433
	1995-97	0.00	0.13	1.18	1944	33433
	1995-98	-0.01	0.18	1.18	1944	33433
	1995-99	-0.01	0.23	1.18	1944	33433
Hirings (women older than 25)						
	1995	0.05	0.07	1.18	1944	33433
	1995-96	0.03	0.10	1.18	1944	33433
	1995-97	-0.10	0.14	1.18	1944	33433
	1995-98	-0.09	0.20	1.18	1944	33433
	1995-99	-0.04	0.24	1.18	1944	33433

Notes: Source: *Quadros de Pessoal*. *ATT* refers to the average treatment effect in terms of the worker flow considered and at the year under analysis. *t(ATT)* denotes analytical standard errors. Estimation is done using by fixed effects where estimates are based on the difference in the accumulated level of the worker flows since 1995 until the year under analysis and the base year, 1992. *Women* is the number of women aged 57-60 in 1992. *Treatment* and *Control* indicates the number of firms assigned to the treatment and control groups respectively.