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ABSTRACT

Employment Consequences of Restrictive Permanent Contracts: Evidence from Spanish Labour Market Reforms*

Temporary employment contracts allowing unrestricted dismissals were introduced in Spain in 1984 and quickly came to account for most new jobs. In 1997, however, the Spanish government attempted to reduce the incidence of temporary employment by reducing payroll taxes and dismissal costs for permanent contracts. In this Paper, we exploit the fact that recent reforms apply only to certain demographic groups to set up a natural experiment research design to study the effects of contract regulations on employment and worker flows. Using data from the Spanish Labor Force Survey, we find that the reduction of payroll taxes and dismissal costs increased the employment of young men and women on permanent contracts, although the effects for young women are marginally significant. The results suggest a moderately elastic response of permanent employment to non-wage labour costs. We also find positive effects on the transitions from unemployment and temporary employment into permanent employment for young and older workers and from permanent employment to non-employment only for older men, suggesting that the reform had little effect on dismissals.

JEL Classification: J23, J32, J38, J63 and J65
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I. Introduction

The European unemployment crisis has motivated extensive debate about the role of labor market institutions in exacerbating unemployment. Concern with possible adverse effects of inflexibility has stimulated research and calls for reform. While a role for institutions is superficially appealing, the evidence for their importance has been mixed (see, e.g., Nickell and Layard (1999) for a recent survey) and the interpretation of results remains controversial. One reason the causal effect of institutional changes has been difficult to establish is the lack of sharp changes or reforms that can be used for measurement. Most institutional changes in the European context have been either gradual or so widespread that it is difficult to identify control groups that can be used to establish a non-reform baseline for comparison.

A second important feature of most reforms to date, and consequently of efforts to evaluate these reforms, is that they are "reforms at the margin" which fail to introduce a fundamental liberalization. In fact, some reforms may simply add further distortions. The most important example of this is the introduction of temporary contracts, a common liberalization strategy in Western Europe. Rather than reducing dismissal costs for permanent contracts, these reforms introduced temporary employment contracts that are not subject to dismissal costs. Allowing the use of temporary contracts without dismissal costs is, however, not equivalent to reducing dismissal costs on permanent contracts. The introduction of this new type of contract may increase the wages of permanent workers and have undesirable consequences for output, employment, and segmentation of the labor market.[1]

[1] See, for example, Blanchard and Landier (2002); Cahuc and Postel-Vinay (2002); Dolado, Garcia-Serrano, and Jimeno (2002); Hunt (2000); Garcia-Fontes and Hopenhayn (1996); Jimeno and Toharia (1993, 1996); Bertola and Ichino (1995); Bentolila and Dolado (1994); and Bentolila and Saint-Paul (1992) for theoretical and empirical analyses on the effects of temporary contracts.

In this paper, we asses the impact of a recent reform in the Spanish labor market. A study of the recent Spanish experience is especially compelling because, in contrast with the majority of Continental reforms, Spain's 1997 Reform bill, extended in 2001, marks a sharp change for some groups (i.e., young workers, older workers, the long-term unemployed, women under-represented in their occupations, and disabled workers), while leaving other groups unaffected. This presents an opportunity to set up a treatment-control design that may provide more reliable estimates of reform effects than past efforts. A second unique feature of recent Spanish reforms is that, unlike previous "reforms at the margin," they led to sharp reductions in payroll taxes and dismissal costs for permanent contracts. Consequently, these reforms may provide a better estimate of the elasticity of permanent employment with respect to non-wage labor costs.

The theoretical section of the paper presents a model with temporary and permanent contracts to illustrate the impact of reduced payroll taxes and dismissal costs on employment. The model is similar to Blanchard and Landier (2002), but it endogenizes dismissals and introduces payroll taxes. In our model, a reduction in dismissal costs for permanent contracts increases conversions of temporary into permanent employment, but it also increases dismissals of permanent workers so the net effect on permanent employment is ambiguous. In contrast, a reduction in payroll taxes increases conversions but leaves dismissals unchanged, so its net effect is to increase permanent employment. Our theoretical discussion shows why the 1997 reform is more likely than previous reforms to have increased employment levels.

The empirical analysis examines the impact of the 1997 reform on employment and worker flows using data from the Spanish labor force survey from the second

quarter of 1987 to the fourth quarter of 2000. The Spanish LFS collects basic individual and family information, as well as labor market information, including type of employment contract. In addition, the LFS has a rotating panel structure that allows us to estimate quarterly transition probabilities.

Our results suggest the reform increased permanent employment probabilities for young workers, although the effects for young women are not always highly significant. The results for young men are robust to controls for common macro shocks for all age groups, for province-specific trends, and for age-specific cyclical effects. Results for older workers and young women show smaller and insignificant or marginally significant effects. The results suggest a moderately elastic response of permanent employment to non-wage labor costs for young men, not out of line with previous estimates of labor demand elasticities reported by Hamermesh (1993). The estimates also show increased quarterly transition probabilities from non-employment to permanent employment for young and older men, although the results for older men are not significant at conventional levels, and from temporary to permanent employment for young men and women during the reform years. On the other hand, transition probabilities from permanent employment to non-employment increased for older men, accounting for weak net employment effects for this group. An implication of these findings is that costly permanent contracts and, especially, high payroll taxes have partly contributed to slack employment growth in Spain. The results also suggest that reducing the costs of permanent employment may be of special value for younger workers.

The paper is organized as follows. Section II describes the institutional framework and the Spanish labor market reforms. Section III presents a theoretical

analysis of reductions in payroll taxes and dismissal costs for permanent contracts introduced by recent reforms. Section IV explains the natural experiment research design used to evaluate the impact of the 1997 reform. Section V describes the data and presents estimates of the effects of the reform on employment levels, accessions, conversions, and separations. We conclude in Section VI.

II. The Spanish Labor Market Reforms

The Spanish labor market has been marked by substantial changes in employment protection legislation over the last two decades. Following the transition to democracy in 1978, Spain introduced labor legislation which maintained many restrictions on dismissals first put into practice during the Franco years. This legislation established that firms could dismiss workers for "personal reasons," in which case the firm had to prove the worker's incompetence or absenteeism, and for "economic reasons," in which case the firm had to prove its need to reduce employment due to technological, organizational, or productive causes. Dismissals justified by "economic reasons" required advance notice.

Workers dismissed for "personal reasons" could appeal to labor courts. The severance payment awarded depended on whether judges ruled the dismissal as "fair" or "unfair." A dismissal was ruled as "fair" if the employer was able to prove the worker's incompetence or absenteeism and "unfair" otherwise. In case of fair dismissals, firms had to pay 20 days pay per year of seniority, with a maximum of 12 months. In the case of unfair dismissals, firms had to pay 45 days pay per year of seniority, with a maximum of 42 months. Severance payments for "economic reasons" were the same as for fair dismissals under "personal reasons." In practice, this legislation turned out to be very stringent because judges ruled dismissals as unfair in the majority of cases.

Moreover, approval for dismissals under "economic reasons" was often granted only when there was an agreement between employers and workers, which was achieved in most cases by raising severance payments above the legally established amounts.

The Spanish government introduced the first reform designed to reduce dismissal costs in 1984. Since an across-the-board reduction of dismissal costs was politically impossible, the reform liberalized the use of temporary contracts. Temporary contracts required lower severance payments than permanent contracts when the contract terminated at term. In particular, temporary workers were entitled to severance pay of 12 days per year of seniority, which could not be appealed in labor courts.

As a result of the 1984 reform, the proportion of employees under temporary contracts increased from 10% during the 1980's to over 30% in the early 1990's. Between 1985 and 1994, over 95% of all new hires were employed through temporary contracts and the conversion rate from temporary to permanent contracts was only around 10%.[2] The main concern with the liberalization of temporary contracts after 1984 was that it generated segmentation between unstable low-paying jobs and stable high-paying jobs, without appearing to reduce unemployment.

Shifting direction in light of these concerns, in 1994 new regulations limited the use of temporary employment contracts to seasonal jobs.[3] In practice, however, employers continued to hire workers under temporary contracts for all types of jobs and not just for seasonal jobs. In addition, the 1994 reform slightly relaxed dismissal conditions for permanent contracts. In particular, the definition of fair dismissals was widened by including additional "economic reasons" for dismissals. In practice,

[2] See Güell-Rottlan and Petrongolo (2000) and Bover and Gómez (1999).
[3] In the case of workers over 45 years of age, temporary contracts could continued to be used for all types of jobs until 1995. After 1995, however, the use of temporary contracts for the over 45 age group, as for the rest of workers, was limited to seasonal jobs.

approval for dismissals under "economic reasons" continued to be granted mainly when there was an agreement between employers and workers and labor courts continued to rule most dismissals as unfair, so that dismissal costs on permanent contracts did not change much.

The perceived ineffectiveness of the 1994 reform led to a new reform in 1997, which was eventually extended in 2001. As with the 1994 reform, the goal of the 1997 and 2001 reforms was to reduce the use of temporary contracts. However, rather than trying to limit the use of temporary contracts by further possibly ineffective regulation, the new reform increased the incentives for firms to hire workers in certain population groups using permanent contracts. In particular, the 1997 reform reduced dismissal costs for unfair dismissals by about 25% and payroll taxes between 40% and 90% for newly signed permanent contracts and for conversions of temporary into permanent contracts after the second quarter of 1997 for workers under 30 years of age, over 45 years of age, the long-term unemployed, women under-represented in their occupations, and disabled workers.

Key provisions of the 1997 reform are summarized in Table 1. Severance payments for unfair dismissals of newly signed contracts of workers in affected groups were reduced from 45 to 33 days pay per year of seniority and the maximum was reduced from 42 to 24 months. In addition, given the high payroll tax rate in Spain (i.e., 28.3% of the salary), the reform reduced payroll taxes between 40% and 90% for workers in these population groups hired under permanent contracts.[4] Table 1 shows that payroll tax reductions went from 40% for workers under 30 years of age and for

[4] Payroll taxes are generally high in all Continental Europe (with Denmark being an exception) and have often being pointed as an explanation for high unemployment in Europe. Laroque and Salanie (2002); Kramarz and Philippon (2001); and Fougére, Kramarz, and Magnac (2000) study the consequences of high payroll taxes in France.

long-term unemployed, to between 70% and 90% for disabled workers. Table 1 shows that in some cases payroll taxes were also reduced after the second year of employment.[5]

The research value of the 1997 reform is partly due to the fact that the new regulations affected different groups of workers differently. In particular, the 1997 reform changed payroll taxes and dismissal costs over time differently for different population groups: younger and older workers, the long-term unemployed, women under-represented in their occupations, and disabled workers. Our estimation strategy exploits the temporal as well as the cross-section variation to evaluate the impact of the reduction in payroll taxes and dismissal costs on employment levels and flows.[6]

The 1997 reform led to a sharp and sustained increase in the number of permanent contracts for workers in some affected groups. This can be seen in Figures 1 and 2, which plot the total number of newly signed permanent contracts and conversions of temporary into permanent contracts for men and women, respectively. The figures show that the number of newly signed permanent contracts increased sharply for young workers and older men, and to a lesser extent for older women, after the second quarter of 1997, but remained roughly constant for the long-term unemployed and disabled workers. On the other hand, the number of regular permanent contracts (i.e., contracts not subject to reductions in payroll taxes and dismissal costs) initially decreased in 1997 and then increased but at a lower rate than for younger workers. The figures also show a marked rise in the number of conversions of

[5] The 2001 reform which became effective in January 2001 essentially extended the 1997 reform, but applied the lower subsidies for contracts signed in 1999 mentioned in Table 1.

[6] Other studies exploiting the differential impact of labor market regulations for different groups of workers in Europe include: Blundell et. al. (2003); Abowd, Kramarz, and Margolis (1999); and Hunt (1995). Autor, Donohue III, and Schwab (2002); Kugler and Saint-Paul (2002), Acemoglu and Angrist (2001); and Oyer and Schaeffer (2000) exploit differential dismissal costs for different groups of workers in the U.S. to study their impact on employment and worker flows.

temporary into permanent contracts after the second quarter of 1997 for both men and women. The sharp rise in conversions and new permanent contracts for young and older workers after the second quarter of 1997 suggests the reform affected these groups of workers.

III. Theoretical Consequences of the Reform

A simple dynamic model illustrates the effects of reductions in payroll taxes and dismissal costs for permanent contracts, such as those introduced by the 1997 reform, when there are both temporary and permanent contracts in the economy. Our model is similar to Blanchard and Landier's (2002) but allows for endogenous permanent dismissals and payroll taxes. Our theoretical discussion shows why the 1997 reform is more likely than previous reforms to have increased employment levels.

Firms have a discount factor r, and they create and fill vacancies using temporary and permanent contracts. There is a cost K of creating a vacancy, which can be filled instantaneously by hiring workers from the pool of the unemployed (i.e., the matching technology is such that there are "workers waiting at the gate").

All jobs are initially filled with temporary contracts, which have constant productivity $\varepsilon_o \geq 0$. Match-specific productivity of permanent jobs is a random variable, ε, with distribution G on $[0,\varepsilon_m]$, where, $\varepsilon_m \geq \varepsilon_o$. Both temporary and permanent jobs are subject to productivity shocks with instantaneous probability λ, where the new match-specific productivity, ε', is drawn from the distribution G. Temporary jobs hit by shocks are either terminated or converted into permanent jobs, while permanent jobs hit by shocks are either terminated or continued. While temporary jobs are not subject to dismissal costs, permanent jobs are subject to dismissal costs, F, which are assumed to be pure waste. Both temporary and permanent jobs are subject to payroll taxes. Payroll

taxes for temporary and permanent jobs are a fraction s_T and s_P of wages w_T and w_P, respectively. The values of temporary and permanent jobs are $J_T(\varepsilon_o)$ and $J_P(\varepsilon)$ and are given by the following Bellman equations:

$$rJ_T(\varepsilon_o) = \varepsilon_o - (1+s_T)w_T(\varepsilon_o) + \lambda E(J_P(\varepsilon') - J_T(\varepsilon_o) \mid \varepsilon' \geq \underline{\varepsilon}),$$

$$rJ_P(\varepsilon) = \varepsilon - (1+s_P)w_P(\varepsilon) + \lambda E(J_P(\varepsilon') - J_P(\varepsilon) \mid \varepsilon' \geq \bar{\varepsilon}) + \lambda(J_T(\varepsilon_o) - J_P(\varepsilon) - F)G(\bar{\varepsilon}),$$

where $\underline{\varepsilon}$ is the threshold match-specific productivity at which firms are indifferent between ending temporary jobs and converting temporary into permanent jobs, and $\bar{\varepsilon}$ is the threshold match-specific productivity at which firms are indifferent between dismissing and retaining workers under permanent contracts.

The labor force is normalized to 1. Individuals are infinitely lived, risk-neutral and have a discount factor r. Workers employed in temporary and permanent jobs receive wages w_T and w_P and a fraction of benefits b financed by firms' payroll contributions for temporary and permanent jobs, $s_T w_T$ and $s_P w_P$ (where b=1 implies a perfect link between benefits and contributions). Workers dismissed from permanent jobs and whose temporary jobs end enter unemployment. Unemployed workers have zero utility and they must start with temporary jobs before moving up to permanent jobs. The arrival rate of temporary jobs is $\varphi = h/u$, where h are total hires and u unemployment. The value to a worker of being employed in a temporary job with productivity ε_o, of being employed in a permanent job with productivity ε, and of being unemployed are $W_T(\varepsilon_o)$, $W_P(\varepsilon)$, and U, and are given by the following Bellman equations:

$$rW_T(\varepsilon_o) = (1+bs_T)w_T(\varepsilon_o) + \lambda E(W_P(\varepsilon') - W_T(\varepsilon_o) \mid \varepsilon' \geq \underline{\varepsilon}) + \lambda(U - W_T(\varepsilon_o))G(\underline{\varepsilon}),$$

$$rW_P(\varepsilon) = (1+bs_P)w_P(\varepsilon) + \lambda E(W_P(\varepsilon') - W_P(\varepsilon) \mid \varepsilon' \geq \bar{\varepsilon}) + \lambda(U - W_P(\varepsilon))G(\bar{\varepsilon}),$$

$$rU = \varphi(W_T(\varepsilon_o) - U).$$

Free entry implies that the number of vacancies is determined by zero net profits, $J_T(\varepsilon_o) = K$. Moreover, since the value of permanent jobs increases with the match-specific productivity, ε, the conversion threshold, $\underline{\varepsilon}$, above which temporary jobs are converted into permanent jobs and the dismissal threshold, $\bar{\varepsilon}$, below which permanent workers are dismissed are given by the following equations:

$$J_P(\underline{\varepsilon}) = J_T(\varepsilon_o) = K \tag{1}$$

$$J_P(\bar{\varepsilon}) = J_T(\varepsilon_o) - F \tag{2}$$

Wages in both types of jobs are set by symmetric Nash bargaining, with continuous renegotiation. The Nash-bargaining conditions for temporary and permanent jobs are:

$$J_T(\varepsilon_o) - K = W_T(\varepsilon_o) - U, \tag{3}$$

$$J_P(\varepsilon) - J_T(\varepsilon_o) + F = W_P(\varepsilon) - U \tag{4}$$

Substituting the free-entry condition into equation (3) implies that the value of being employed in a temporary job is equal to the value of being unemployed, $W_T(\varepsilon_o) = U$, and both are equal to zero. Integrating equation (4) over $\underline{\varepsilon}$ and ε_m, yields $E(J_P(\varepsilon') - W_P(\varepsilon') | \varepsilon' \geq \underline{\varepsilon}) = (K-F)(1-G(\underline{\varepsilon}))$. Using this together with the fact that the value of being unemployed is zero and with the Bellman equation for a temporary job yields the temporary wage,

$$w_T(\varepsilon_o) = [\varepsilon_o - rK - \lambda F(1-G(\underline{\varepsilon}))] / [2 + (1+b)s_T].$$

Similarly, integrating (4) over $\underline{\varepsilon}$ and ε_m, yields $E(J_P(\varepsilon') - W_P(\varepsilon') | \varepsilon' \geq \bar{\varepsilon}) = (K-F)(1-G(\bar{\varepsilon}))$. This together with the fact that the value of being unemployed is zero and with the Bellman equation for a permanent job yields the permanent wage,

$$w_P(\varepsilon) = [\varepsilon - r(K-F)] / [2 + (1+b)s_P].$$

There is a unique wage for temporary jobs, since they all have the same level of productivity, ε_o. On the other hand, wages in permanent jobs depend on the match-specific productivity, ε.

Substituting wages and the free-entry condition into the value of a permanent job, and evaluating at the conversion and dismissal thresholds yields two equations which define the conversion and dismissal thresholds implicitly,

$$(r+\lambda)K = \{[(1+bs_P)\,\underline{\varepsilon} + r(K-F)]/[2 + (1+b)s_P]\} + \lambda E(\,J_P(\varepsilon')\,|\,\varepsilon' \geq \bar{\varepsilon}\,) + \lambda(K-F)G(\,\bar{\varepsilon}\,) \tag{5}$$

$$(r+\lambda)(K-F) = \{[(1+bs_P)\,\bar{\varepsilon} + r(K-F)]/[2+(1+b)s_P]\} + \lambda E(\,J_P(\varepsilon')\,|\,\varepsilon' \geq \bar{\varepsilon}\,) + \lambda(K-F)G(\,\bar{\varepsilon}\,) \tag{6}$$

Subtracting (6) from (5) yields

$$\underline{\varepsilon} - \bar{\varepsilon} = [2 + (\,1+b\,)s_P]\,(\,r+\lambda\,)F\,/\,[\,1+bs_P\,].$$

Substituting the permanent wage into the value of a permanent job, and then integrating by parts and using the threshold conditions (1) and (2), yields the individual thresholds,

$$\bar{\varepsilon} = (\,r+\lambda\,)(\,K-F\,) - \lambda\varepsilon_m\,/\,r + \lambda(\,G(\,\varepsilon_m\,)-G(\,\bar{\varepsilon}\,))\,/\,r,$$

$$\underline{\varepsilon} = (\,r+\lambda\,)K - \lambda\varepsilon_m\,/\,r + \lambda(\,G(\,\varepsilon_m\,)-G(\,\bar{\varepsilon}\,))\,/\,r + (\,r+\lambda\,)(\,1+s_P\,)F\,/\,(\,1+bs_P\,),$$

Comparative statics on these thresholds show that a reduction in dismissal costs reduces the difference between the conversion and dismissal thresholds both because the conversion threshold falls and because the dismissal threshold increases. A reduction in payroll taxes for permanent jobs also reduces the difference between the conversion and dismissal thresholds as long as the link between benefits and contributions is not perfect. In this case, however, only the conversion threshold falls.

Given the values of the two productivity thresholds, we can derive the steady-state values of unemployment, temporary employment and permanent employment. The

flow out of unemployment has to equal the flow into unemployment as well as the flow into temporary jobs, so $\varphi u = \lambda[e_T G(\underline{\varepsilon}) + e_P G(\bar{\varepsilon})] = \lambda e_T$. Using the steady state conditions and the identity $u + e_T + e_P = 1$, yields the steady state values of unemployment, temporary employment and permanent employment,

$$u = [\lambda G(\bar{\varepsilon})] / [\lambda G(\bar{\varepsilon}) + \varphi(G(\bar{\varepsilon}) + \lambda(1 - G(\underline{\varepsilon})))],$$

$$e_T = [\varphi G(\bar{\varepsilon})] / [\lambda G(\bar{\varepsilon}) + \varphi(G(\bar{\varepsilon}) + \lambda(1 - G(\underline{\varepsilon})))],$$

$$e_P = [\varphi(1 - G(\underline{\varepsilon}))] / [\lambda G(\bar{\varepsilon}) + \varphi(G(\bar{\varepsilon}) + \lambda(1 - G(\underline{\varepsilon})))].$$

For given φ, unemployment and temporary employment increase with $\underline{\varepsilon}$ and $\bar{\varepsilon}$, while permanent employment decreases with $\underline{\varepsilon}$ and $\bar{\varepsilon}$. Consequently, a reduction in dismissal costs has an ambiguous effect on permanent employment and a reduction in payroll taxes increases permanent employment if the link between benefits and contributions is not perfect.

As in Blanchard and Landier (2002), which looks at the effect of reducing dismissal costs for temporary contracts, our theoretical discussion suggests the reduction in dismissal costs for permanent contracts increases hiring and dismissal and has ambiguous effects on unemployment. On the other hand, while Blanchard and Landier (2002) find that reducing temporary dismissal costs reduces permanent conversions and increases the permanent-temporary wage differential, here a reduction in permanent dismissal costs increases permanent conversions and reduces the wage differential. Thus, unlike previous reforms affecting temporary contracts only, the 1997 reform should reduce labor market segmentation. Moreover, our model shows that the 1997 reform was more likely than previous reforms to increase permanent employment because of the reduction in payroll taxes introduced by this reform.

IV. Identification Strategy

Our goal in this paper is to identify the impact of reduced payroll taxes and dismissal costs on permanent contracts. To this end, we compare treated groups under 30 and over 45 years of age with the control group of middle-aged workers before and after the 1997 reform. We concentrate on contrasts by age group since other treated groups - the long-term unemployed and women under-represented in certain occupations - may be self-selected. While self-selection is not as much of a concern for disabled workers, unfortunately our data does not allow us to distinguish disabled workers. Moreover, as shown in Figures 1 and 2 above, the greatest impact of the reform appears to have been on the two affected age groups.

The identification strategy is illustrated in Figures 3 and 4, which plot permanent employment probabilities for men and women by age group relative to the base period, first quarter of 1997, for the same years as Figures 1 and 2 (i.e., 1995-2000). The figures show that permanent employment probabilities started to increase after the implementation of the reform (i.e., second quarter of 1997) and that the increase was greatest for younger workers. Since the reform was introduced during an expansion, Figures 5 and 6 plot the permanent employment probabilities for men and women for the entire period for which we have data (i.e., 1987 to 2000), which spans another expansion in the late 1980's and a recession in the early 1990's. As before, these figures show the increase in permanent employment probabilities for the young after the second quarter of 1997, but they also show higher permanent employment probabilities for the young during the expansion of the late 1980's.[7] The figures highlight the importance of proper control for cyclical effects, especially because the young appear to

[7] Similarly, Bover, Arellano and Bentolila (2002) find that favorable business conditions in Spain increase the hazard of leaving unemployment.

benefit disproportionately during expansions. On the other hand, the figures show similar permanent employment probabilities during the two expansions, even though the expansion of the late 1980's was stronger than the expansion of the late 1990's in terms of GDP growth.

To control for age-specific cyclical effects, we compare permanent employment of treated and control individuals during the expansionary reform period with the permanent employment of treated and control individuals during an earlier expansionary period. This estimator uses the period without reform to check for the possibility that expansions have differential effects on younger and older workers.[8] In addition, this strategy is implemented in samples limited to narrower age groups, concentrated around the affected age groups, to check for substitution of control for treated workers. It is possible that workers not covered by the reform are being substituted for under 30 and over 45 year olds. If this were the case, then we should find much larger effects in the restricted samples. For example, the sample for the young is restricted to the 20-39 age group. Since the 20-29 age group and the 30-39 age group are likely to be close substitutes, restricting the sample in this way is an important robustness check.

The following logit model is used to implement the estimation strategy:

$$\Pr[e_{it}=1 \mid X_{it}, d_i] = \Lambda[\alpha_t + \beta' d_i + \gamma' X_{it} + \delta'(d_i \times R_t)], \tag{7}$$

where $e_{it}=1$ if employed with a permanent contract and 0 otherwise; d_i is a vector of dummies for treated groups, α_t is a year effect, and X_{it} includes covariates affecting individual i at time t, including quarter dummies and, in some specifications, province-specific trends. The group dummies capture differential permanent employment rates of the treated groups before and after the reform, while the quarter and year effects capture

the impact of seasonal and macro shocks affecting workers in both treated and control groups.[9] The province-specific trends control for factors affecting employment differentially in different provinces over time, including EU active labor market programs introduced in some Spanish regions.[10] R_t is a dummy for reform years, so that δ, the vector of reform/treatment group interactions, captures the effects of interest.

Specifications that control for age-specific cyclical effects include age group interactions with an expansion variable, E_t. We use two expansion variables: an expansion dummy which equals 1 in 1987-91 and 1995-1999 and zero otherwise, and GDP growth. That is, the estimating equation is modified to be

$$\Pr[e_{it}=1 \mid X_{it}, di] = \Lambda[\alpha_t + \beta' d_i + \gamma' X_{it} + \delta_E'(d_i \times E_t) + \delta_R'(d_i \times R_t)]. \tag{8}$$

Here, as before, the impact of the reform is captured by the vector of reform/treatment group interactions, δ_R, which now measures the reform impact relative to the pre-treatment expansion. The age-specific cyclical effect is captured by the vector of expansion/treatment group interactions, δ_E.

Finally, transition probabilities from non-employment to permanent employment, from temporary employment to permanent employment, and from permanent employment to non-employment, were estimated by fitting equations (7) and (8) conditional on the relevant labor market state. That is, all parameters are free to

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[8] This strategy is in the spirit of the falsification test by Angrist and Krueger (1999) which uses the "Failed Mariel Boatlift" to examine the impact of immigration on the Miami labor market.

[9] In addition, including quarter and year effects helps to control for cohort effects.

[10] We include interactions of province dummies with a time trend because active labor market programs were introduced differentially across provinces during the 1990's. However, in contrast to the sharp timing of the 1997 reform which was introduced after the second quarter of 1997, active labor market policies were introduced at various points during the 1990's. In addition, the inclusion of province-specific trends helps to control for the serial correlation problem in differences-in-differences inference pointed out by Bertrand, Duflo, and Mullainathan (2001). Other specifications also include sector-specific trends to control for factors potentially affecting employment differentially in different sectors over time, such as skilled-biased technical change. We do not report them here, since the results are very close to those with province-specific trends.

vary with employment status in period t-1. As with the models for employment levels, some of the specifications for transitions control for age-specific cyclical effects by allowing differential transition probabilities for treated groups during the expansions of the late 1980's and 1990's.

V. Estimates of the Impact of the 1997 Reform

A. Data and Descriptive Statistics

Our data comes from the Spanish Labor Force Survey (LFS) from the second quarter of 1987 to the fourth quarter of 2000.[11] The LFS has information on basic individual and family information, including information about sex, age, province of residence, education, marital status, and whether the person is a household head or not. The LFS also includes labor force information including employment status, occupation, sector, tenure and type of contract in the current and previous jobs.[12] We exclude individuals in the military, workers employed in agriculture, as well as employers, coop members, family workers and the self-employed from our sample. Our samples include men and women between 16 and 65 years of age to focus on young and older workers affected by the reform.

The LFS has a rotating panel structure that follows individuals for a maximum of six quarters, replacing one-sixth of the sample every quarter. In practice, there is attrition and not everyone is followed for six quarters. Jiménez and Peracchi (2002)

[11] The LFS underwent a number of methodological changes in 1995. Prior to 1995 the LFS sampled randomly out of the 1980 population Census, while after 1995 the LFS sampled randomly out of the 1991 population Census. Most importantly, prior to 1995, individuals between 25 and 45 years of age were under-sampled because of problems with the sampling framework which was corrected after 1995. These methodological changes have reduced the figures on aggregate unemployment estimated with the LFS, but as shown in Figures 2-6, they do not appear to have affected estimates of individual employment probabilities for those in particular age groups.

[12] The Spanish LFS does not have earnings information, so we cannot study the effect of payroll taxes and dismissal costs on wages. The presence of downward wage rigidities in the Spanish context, however, probably implies that most adjustments take place through quantities rather than through prices.

report an attrition rate of about 20% in the rotating panel, which is close to that found for similar data sets in other countries.[13] To identify transitions, we match individual records from one quarter to the next using the personal identification number of the individual. We restrict ourselves to matches with the same sex in consecutive quarters.

The impact of the 1997 reform on employment levels is evaluated by looking at employment probabilities. The effects on worker flows are evaluated by looking at transition probabilities.

Table 2 presents descriptive statistics by age group for the periods before and after the reform. The table shows lower permanent employment probabilities for young men and women and middle-aged men after the reform, probably reflecting the fact that the pre-reform period includes the strong expansion of the late 1980's. On the other hand, permanent employment probabilities are higher for middle-aged women and older men and women after the reform. Simple comparisons of means also indicate lower transitions during the post-reform period. However, as shown in the regressions below, controlling for year effects and other covariates shows a different picture. Men and women are also older, more educated, less likely to be married, and have shorter tenures during the reform period. In contrast, men are less likely and women more likely to be head of household during the reform period.

B. Employment Effects

Table 3 reports logit marginal effects estimated using equations (7) and (8). The dependent variable is a discrete variable which takes the value of 1 if the person is employed with a permanent contract and 0 if the person is non-employed (either unemployed or out of the labor force). The controls in these logits are head of

[13] Acemoglu and Angrist (2001) report an attrition rate of around 29% in the CPS. Also, similar to what has been found for other countries, Jiménez and Peracchi (2002) find little evidence that attrition

household and marital status dummies, four schooling groups, tenure, seven occupation groups, 10 sector groups, year effects, quarter effects, 15 province-specific trends, and under 30 and over 45 age groups. The effects of interest are captured by the interactions of the under 30 and over 45 age groups with the reform dummy.[14] The marginal effects of these interactions capture the change in permanent employment probabilities of younger and older relative to middle-aged workers during the reform years.[15] Panels A and B show the results for men and women, respectively. The results show a large and statistically significant increase in permanent employment probabilities for young relative to middle-aged workers after the 1997 reform became effective, although the results for young women are only marginally significant. The reported standard errors allow for clustering by year-age group to control for common random effects within these cells.[16] For example, Column (1) shows that the probability of permanent employment increased by 0.0222 for younger men and by 0.016 for young women relative to middle-aged workers during the reform years. In contrast, the results for older workers are insignificant.

Column (2) controls for age-specific cyclical effects by including interactions of the under 30 and over 45 age groups with an expansion dummy. The results show positive and significant effects for both young men and women of 0.0211 and 0.019. In contrast, the results for older workers remain insignificant. However, since the

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generates important selection biases in estimating quarterly transition probabilities.

[14] The age group and post-reform dummies are defined by the age and quarter at the time of hiring. Thus, for employment probabilities and transitions from non-employment and temporary to permanent employment, the age groups are constructed using the current age and year/quarter of survey. In contrast, for transitions from permanent to non-employment, the age groups are constructed using the age at the time of the accession (i.e., current age – tenure) and the year/quarter at the time of accession (i.e., current year/quarter – tenure).

[15] Standard errors for marginal effects were calculated using the delta method.

[16] As is typical in data with a group structure like ours, adjusting for group clustering seems much more important than adjusting for the fact that the rotation group structure means that some individuals are followed through time (see, e.g., studies using the CPS). Since the two-way adjustment is complex, we

expansion of the late 1980's was stronger in terms of GDP growth, this control for age-specific cyclical effects may provide a lower bound of the effect of the reform. The next column, instead, controls for age-specific cyclical effects by interacting age groups with GDP growth. As before, the results show significant positive effects on young men and women, but the effects on older workers remain insignificant.

The next two columns limit the sample to narrower age groups to check for substitution effects. Column (4) uses the 20-29 age group as the treated and the 30-39 age group as the control for young workers, while Column (5) uses the 45-54 age group as the treated and the 35-44 age group as the control for older workers. The results in Columns (4) and (5) of Panel A show an increase in permanent employment probabilities of 0.016 (i.e., 2.8%) for young men but no effect for older men. These results based on narrower age groups are smaller than those found for the full sample, suggesting no substitution effects for men. The results in Columns (4) and (5) of Panel B show an increase of 0.018 (i.e., 7.2%) in the permanent employment probabilities for young relative to middle-aged women during the reform years, but no effect for older women.[17] The larger effect for young women in the restricted sample suggests there may be some substitution of middle-aged for younger women. On the other hand, the results for young women are marginally significant even when using the full sample.[18]

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report standard errors correcting only for the former. The latter increases standard errors by only about a third, with little effect on significance levels, while group-clustering more than triples the standard errors.

[17] The control group for women may not be as good because the group of middle-aged women may include women under-represented in their occupations who may have benefited from the reform. To check for this possibility, we divided women into occupations that were and were not covered by the reform, then replicated the differences-in-differences strategy on those groups. We did not find differential effects of the reform for women in different occupations. However, there are some problems with this strategy. First, occupations are not well-defined for those not working. Second, occupations can only be identified at the 1-digit level in the Labor Force Survey, but under-represented occupations are classified at the 3-digit level. Finally, as pointed out above, women may self-select into occupations.

[18] We also looked at the effect of the reform on the probability of employment under temporary contracts. Our theoretical framework suggests that the reduction in dismissal costs should have ambiguous effects on temporary employment. On the other hand, the reduction in payroll taxes should had generated substitution of temporary for permanent jobs and should had reduced temporary employment. In analysis

C. Effects on Worker Flows

Table 4 reports logit marginal effects from models for transitions from non-employment to permanent employment. The dependent variable is a discrete variable which takes the value of 1 if the person transited from non-employment to permanent employment from one quarter to the next and 0 if the person continues to be non-employed the next quarter.[19] As before, Panel A reports the results for men and Panel B for women. The results show increased transitions from non-employment to permanent employment for young relative to middle-aged workers after the 1997 reform became effective. For example, Column (1) in Panel A shows an increase in the relative transition probabilities from non-employment to permanent employment of 0.0447 or 46.2% for younger men during the reform years. Column (1) in Panel B also shows an increase in the relative probability of transiting from non-employment to permanent employment of 0.0096 or 16.7% (with a p-value of 0.187) for younger women during the reform years. The results for older workers are insignificant.

The rest of the columns in Table 4 report results which control for age-specific cyclical effects. Column (2) in Panel A shows a smaller effect on the probability of transiting from non-employment to permanent employment of 0.0374 or 38.7% for young men and a bigger effect of 0.0301 or 12.2% for older men (with a p-value of 0.188) after controlling for age-specific cyclical effects using an expansion dummy. The transition from non-employment to permanent employment falls to 0.0374 or 38.7% for younger men. Interacting age groups with GDP growth instead of the expansion dummy in Column (3) yields a slightly larger effect for young men of 0.0445 or 46%, but no effect on older men. Results on the restricted samples of men in

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parallel to that in Table 3, we found no differential effects for young men and women or older men but a negative effect for older women, although the effect for older women is not always significant.

Columns (4) and (5) of Panel A show a smaller but significant effect of 0.0344 or 35.6% for young men, but no effect for older men. Thus, the results based on the restricted sample suggest no substitution of middle-aged for younger and older men. The results for young women in Panel B continue to show an increase in the transitions from non-employment to employment of between 16.5% and 24% depending on the way of controlling for age-specific cyclical effects. Moreover, the results controlling for age-specific cyclical effects on the restricted sample of young women show a significant increase in the transitions from non-employment to employment of 14.4%. On the other hand, the results for older women are insignificant. The results on the restricted samples for women again suggest possible substitution of middle-aged for young women, indicating that these results must be interpreted cautiously.[20]

Table 5 reports logit marginal effects from models for transitions from temporary to permanent employment. The results in Panel A show a statistically significant increase in the transitions from temporary to permanent employment for younger relative to middle-aged men during the reform years. The results without controlling for age-specific cyclical effects suggest an increase of about 0.0307 or 36.7%, while the results which control for age-specific cyclical effects indicate an increase of between 0.0253 and 0.0303 (or between 30.2% and 36.2%). On the contrary, the results for older men show negative though mostly insignificant effects on the transitions from temporary to permanent employment. The results in Panel B also show a significant increase in temporary to permanent transition probabilities for young women but not for older women. The results without controlling for age-specific

[19] The controls for transition probabilities are as in the permanent employment probability specifications.
[20] Results for transitions from non-employment to temporary employment show positive effects on younger workers and no effects on older workers, although the effects on younger workers are not always significant. However, comparing these effects with the effects on transitions into permanent employment

cyclical effects suggest an increase of about 0.0226 or 26.2%, while the results which control for age-specific cyclical effects indicate an increase of between 0.02 and 0.0227 (or between 23% and 26.3%). The results on the restricted samples are smaller for young workers than those in the full sample and insignificant for older workers, suggesting no substitution effects.

Table 6 reports logit marginal effects from models for transitions from permanent employment to non-employment. The results in Panel A show a rise in the transitions from permanent employment to non-employment for older relative to middle-aged men during the reform years of between 7.5% and 8.7%, with and without controlling for age-specific cyclical effects. The results on the restricted sample of older men are similar to those in the full sample, suggesting no substitution in dismissals towards older men. Evidence of increased flows from non-employment to permanent employment for older men in Table 4 and the increased flows from permanent employment to non-employment in this table appear to cancel out, explaining the weak net effect on permanent employment. In contrast, the results on the full sample suggest no change in the transition from permanent employment to non-employment for young men during the reform years. Similarly, the effects for young and older women are insignificant. [21]

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in Table 4 indicates a significant increase in the transitions into permanent employment relative to transitions into temporary employment for young men, but no effect on young women.

[21] Only the results on the restricted sample for young men suggest a significant increase in transitions from permanent employment to non-employment. In contrast, results of logits for transitions from temporary employment to non-employment show no effects on any of the treated groups. Part of the reason why we may not be finding effects of the reform on the permanent separations of young men and women and older women is that we are mostly looking at the effect of the reform during the expansion of the late 1990's. While 2000 marks the beginning of the recent recession, we do not observe a full cycle after the reform.

D. Economic Interpretation of Magnitudes

Elasticities of permanent employment with respect to non-wage labor costs can be estimated by dividing the percent change in net employment from Table 3 by the percent change in employment costs due the 1997 reform. We concentrate on young men because the results for older workers and young women show insignificant or marginally significant effects, suggesting little response to the reform by these workers.

The 1997 reform reduced dismissal costs from 45 to 33 days pay or, equivalently, a reduction of 26.7%. In addition, the reform reduced the uniform payroll tax rate of 28.3% of the salary of young workers by 40% for contracts signed in 1997 and 1998 during the first two years of the contract, and by 35% and 25% for contracts signed after 1999 during the first and second years of the contract, respectively. To estimate the percent change in total costs implied by the reform, we need to multiply the changes in dismissal costs and payroll taxes by the fraction of expected dismissal costs and payroll taxes in total labor costs. Expected quarterly costs for unfair dismissals are equal to the probability of an unfair dismissal times the estimated costs of unfair dismissals. While we do not have the probability of a dismissal, Table 2 reports separation rates by age (i.e., 3.3% for young men). The probability of ruling a dismissal unfair in Spain is 0.72.[22] Costs for unfair dismissals can be estimated based on the following formula:

$$\text{Dismissal Costs} = (45/365) \times \text{Yearly Salary} \times \text{Tenure in Years.}$$

[22] This number is estimated using data from the Spanish Ministry of Justice by Galdón-Sánchez and Güell (2000) as the percent of cases declared as unfair dismissals out of all cases taken to court. Since there may be a fraction of cases not taken to court, then this number may be lower and the elasticity estimated above would then be higher.

Mean salaries from the Survey of Salary Structure for 1995 indicate a yearly salary of 3,830 Euros for young men.[23] From the LFS we get mean tenure for young men of 2.16 years in 1995. Combining these numbers, we get quarterly expected dismissal cost of 24.23 Euros for young men.[24]

Payroll tax costs are easier to obtain. The payroll tax rate is 28.3%, implying an average quarterly payroll tax cost of 271 Euros for young men. Consequently, dismissal costs and payroll taxes account for about 1.9% and 21.6% of total labor costs for young men, respectively. Multiplying these figures by the corresponding percent changes in dismissal costs and payroll taxes gives the percent change in total labor costs as a result of the reform. Using the larger payroll tax reductions of 40% for young workers, the percent reductions in total labor costs implied by the reform for young men was of 9.2%. Using the smaller payroll tax reductions of 30% for young workers applied during the second year of the contract, the percent reductions in total labor costs implied by the reform for young men was of 7%. Of the total labor cost reduction for young men induced by the reform, 92.7% and 94.4% can be attributed to the smaller and larger payroll tax reductions, respectively. This means that while payroll tax reductions were smaller in absolute terms for younger than for older workers, the payroll tax reductions were relatively more important for younger workers. Although payroll tax reductions for older workers ranged between 50% and 60%, they only accounted for between 47% and 51% of the total reduction in non-wage labor costs because of the relatively high dismissal costs for this group of workers.

[23] Average salaries are low because they include very young workers many of whom still live at home in Spain.

[24] This means we do no have to consider the change in the maximum payment of dismissal costs from 42 to 24 months, since it is never binding.

Results in Table 3 that control for age-specific cyclical effects suggest the reform increased permanent employment probabilities between 0.016 or 2.8% and 0.0222 or 3.9% for young men. These results imply elasticities for young men of between –0.31 and –0.43 using the payroll tax reduction of 40% and of between –0.4 and –0.56 using the payroll tax reduction of 30%. The results suggest a moderately elastic employment response of young men to changes in non-wage labor costs, but an inelastic response of older workers. These elasticities are well in line with the mean labor demand elasticity of –0.45 reported by Hamermesh (1993). The results are also consistent with the findings reported in Hamermesh (1993) that the demand for labor for young workers and, more generally, for less-skilled workers is more elastic.

Our results are also in line with positive but moderate effects of employment subsidies for low-wage workers found in studies for the U.S. and Canada. For example, Bishop (1981) finds that the New Jobs Tax Credit introduced in 1977 in the U.S. increased employment and Gera (1987) finds that the Employment Tax Credit Program introduced in Canada in 1978 and 1981 had positive effects on job creation. Also, while earlier studies of U.S. subsidy programs to employers hiring disadvantaged youth found that these programs had low take-up rates and could even stigmatize the targeted group, Katz (1998) presents recent evidence on the Targeted Jobs Tax Credit program suggesting a modest impact on the demand for labor for disadvantaged youth.

VI. Conclusion

Natural experiments that can be used to assess the consequences of employment contract regulations in Europe are rare. This paper uses the Spanish labor market reform of 1997, which reduced payroll taxes and dismissal costs, to set up a research design based on the fact that the reform applied differently to different age groups. Our

theoretical framework suggests the reduction in dismissal costs should increase conversions and dismissals, with an ambiguous effect on employment. On the other hand, the reduction in payroll taxes should increase conversions and, thus, permanent employment. Estimates using the Spanish Labor Force Survey suggest that the reform increased permanent employment probabilities for young relative to middle-aged workers. The results for young men are robust to controls for common macro shocks for all age groups, for province-specific trends, and for age-specific cyclical effects. The results also show increases in the relative transitions from non-employment to permanent employment for young and older men, although the results for older men are not significant at conventional levels, and from temporary to permanent employment for young men and women during the reform period. On the other hand, relative transitions from permanent employment to non-employment increase only for older men. Results based on restricted age-groups, suggest that the reform is not simply generating substitution of middle-aged for younger and older men but there is some evidence of substitution of middle-aged for younger women.

Our results suggest that the reduction in dismissal costs and payroll taxes had a positive effect on the hiring margin of young workers with little effect on dismissals, but increased dismissals and hiring for older men. This explains why the reform seems to have had a positive net effect on permanent employment for young workers but not for older workers. This is probably because of the relative importance of payroll tax reductions for young workers, which suggests positive effects on hiring and net employment when the benefit-contribution linkage is not perfect.

The estimated elasticities suggest a moderately elastic response of permanent employment to non-wage labor costs for younger men for whom the payroll tax

reduction was relatively more important. The weaker effects on young women probably reflect the importance of other considerations when hiring women, such as fertility decisions and the existence of mandatory maternity leave for women under permanent contracts in Spain. These results are in line with previous estimates of labor demand elasticities and with previous findings of larger elasticities for younger workers. Our findings are also consistent with the positive, but moderate, employment effect of subsidy programs and demonstration projects in the U.S. and Canada. On balance, the results reported here support the view that the high non-wage labor costs and lack of flexibility associated with permanent contracts have contributed to reduce employment levels in Spain, especially for young workers. Nonetheless, given the moderate response of employment to payroll taxes and dismissal costs, non-wage labor costs alone are unlikely to explain the bulk of youth unemployment. Other factors such as insurance by families and the state as well as the little stigma attached to unemployment may contribute to explain persistent and high unemployment among young workers in Europe.

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Figure 1: Number of New Permanent Contracts for Men in Population Groups affected by the 1997 Reform

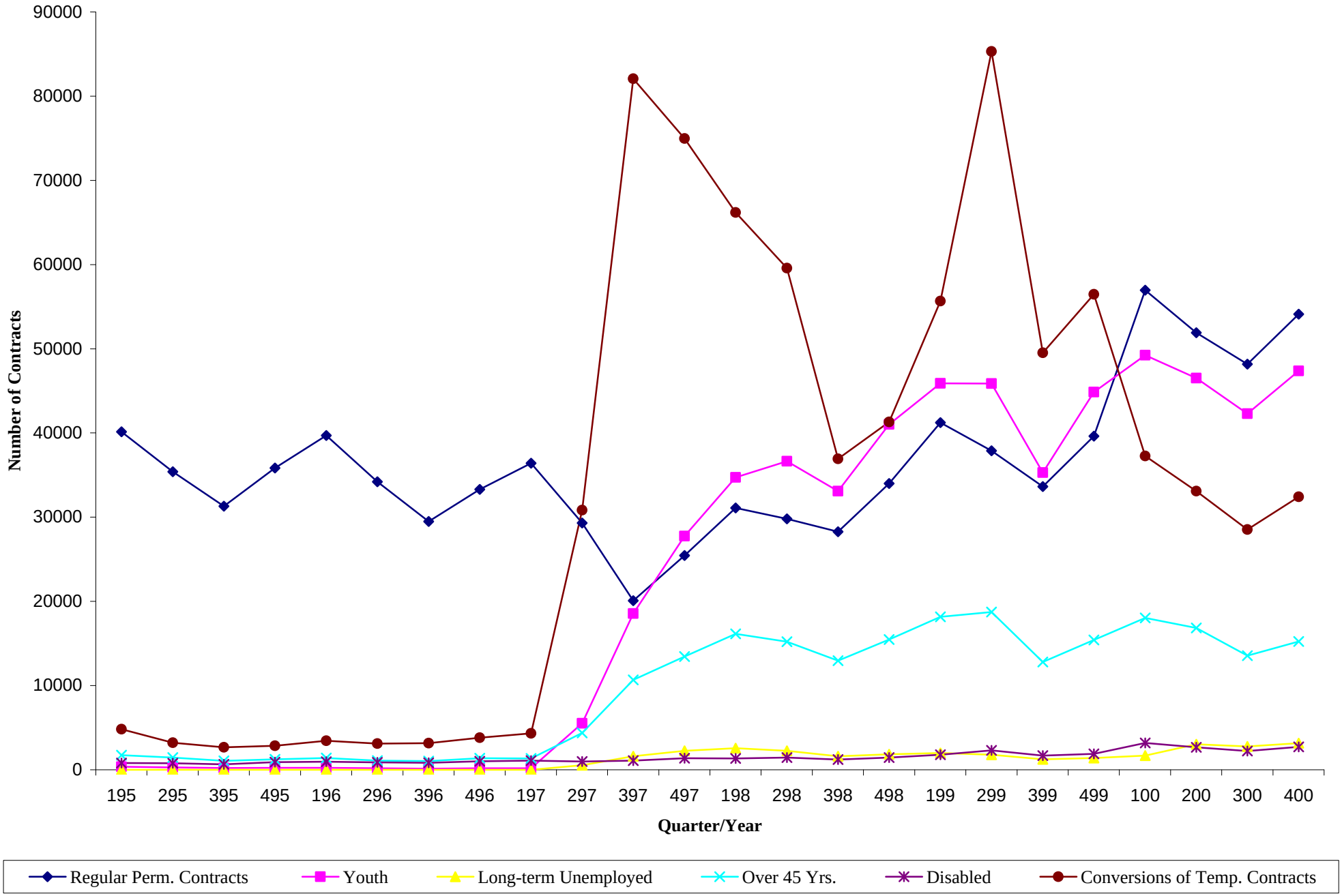

Figure 2: Number of New Permanent Contracts for Women in Population Groups affected by the 1997 Reform

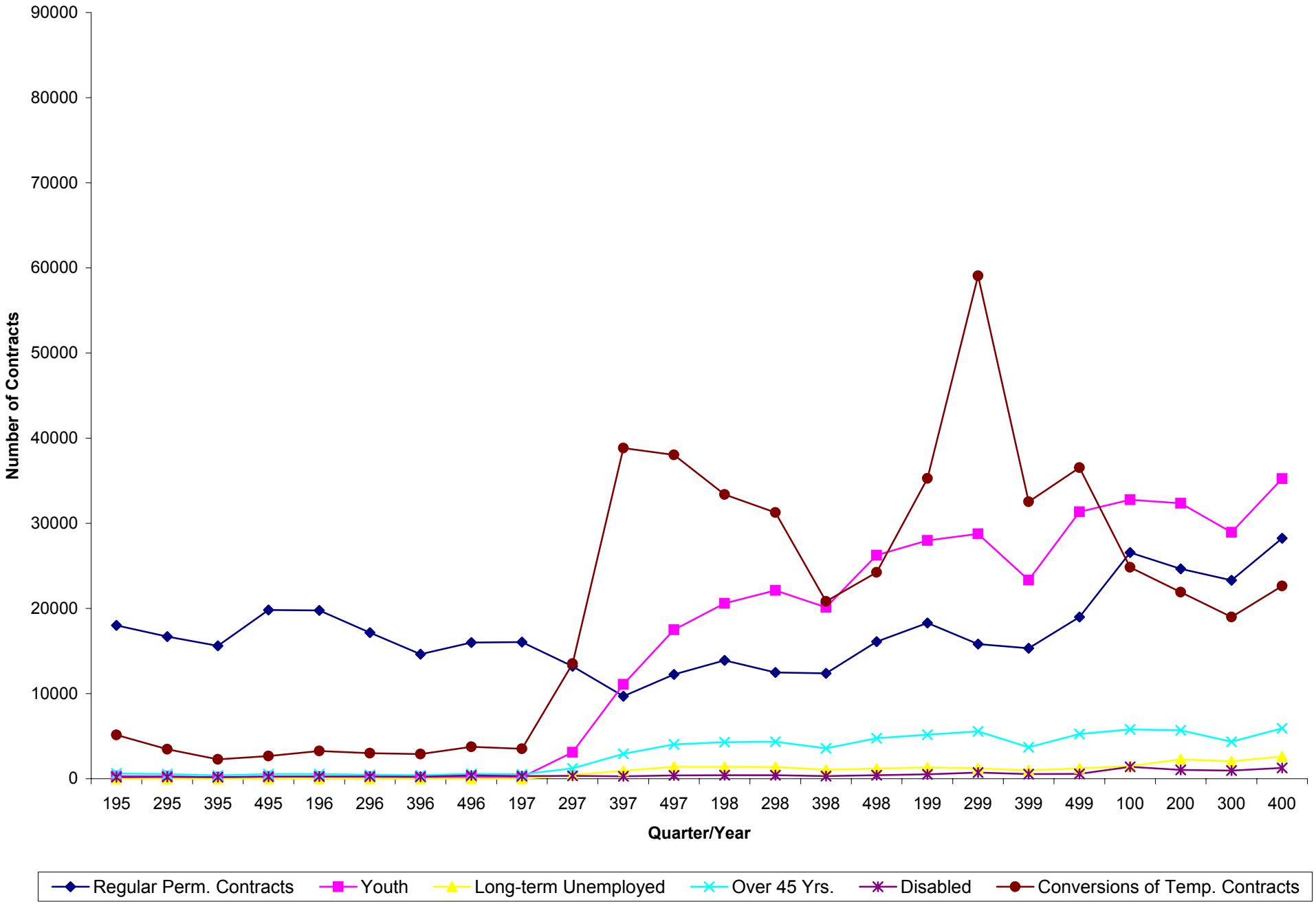

Figure 3: Permanent Employment Probabilities for Men by Age Group Normalized by First Quarter of 1997, 1995-2000

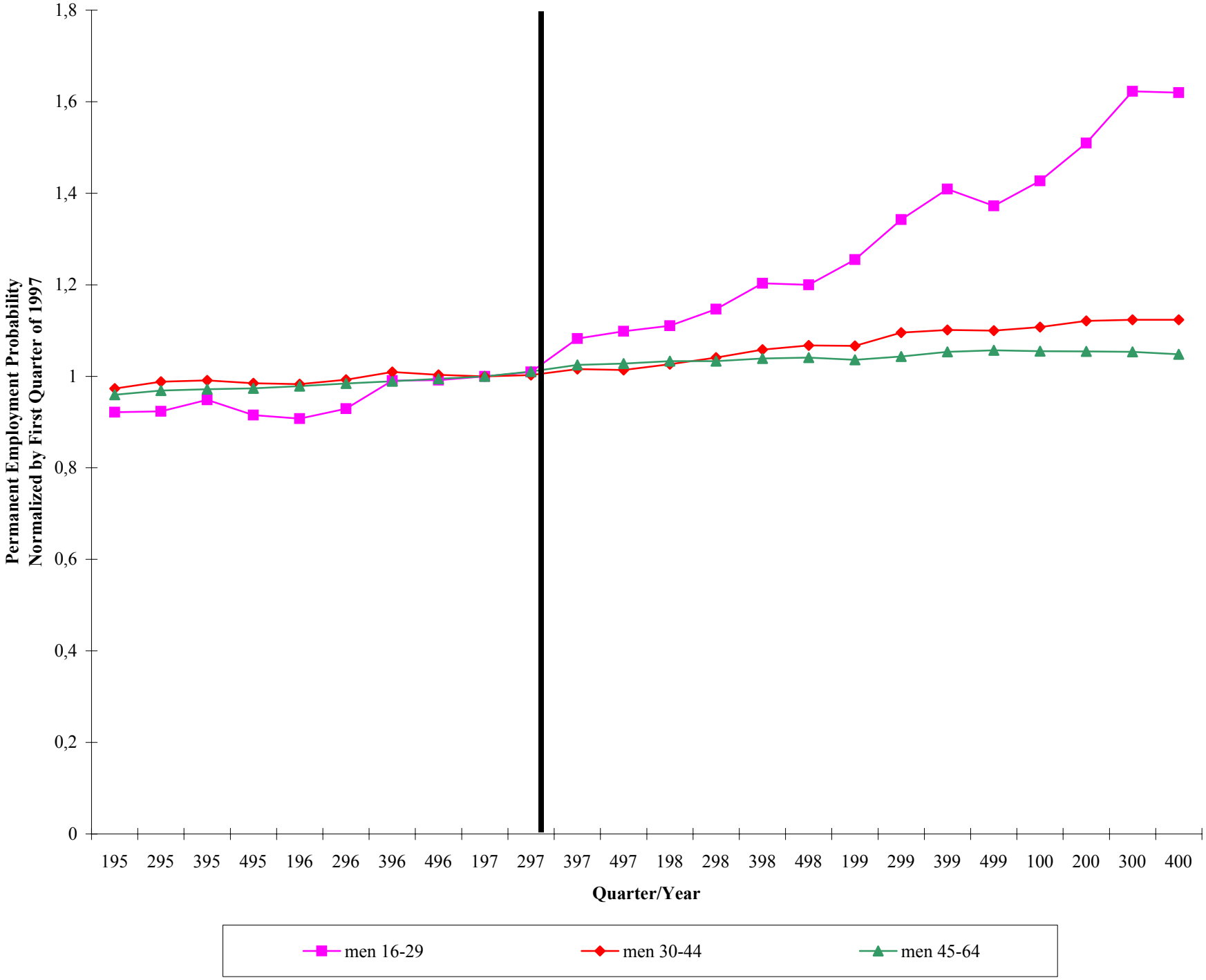

Figure 4: Permanent Employment Probabilities for Women by Age Group Normalized by First Quarter of 1997, 1995-2000

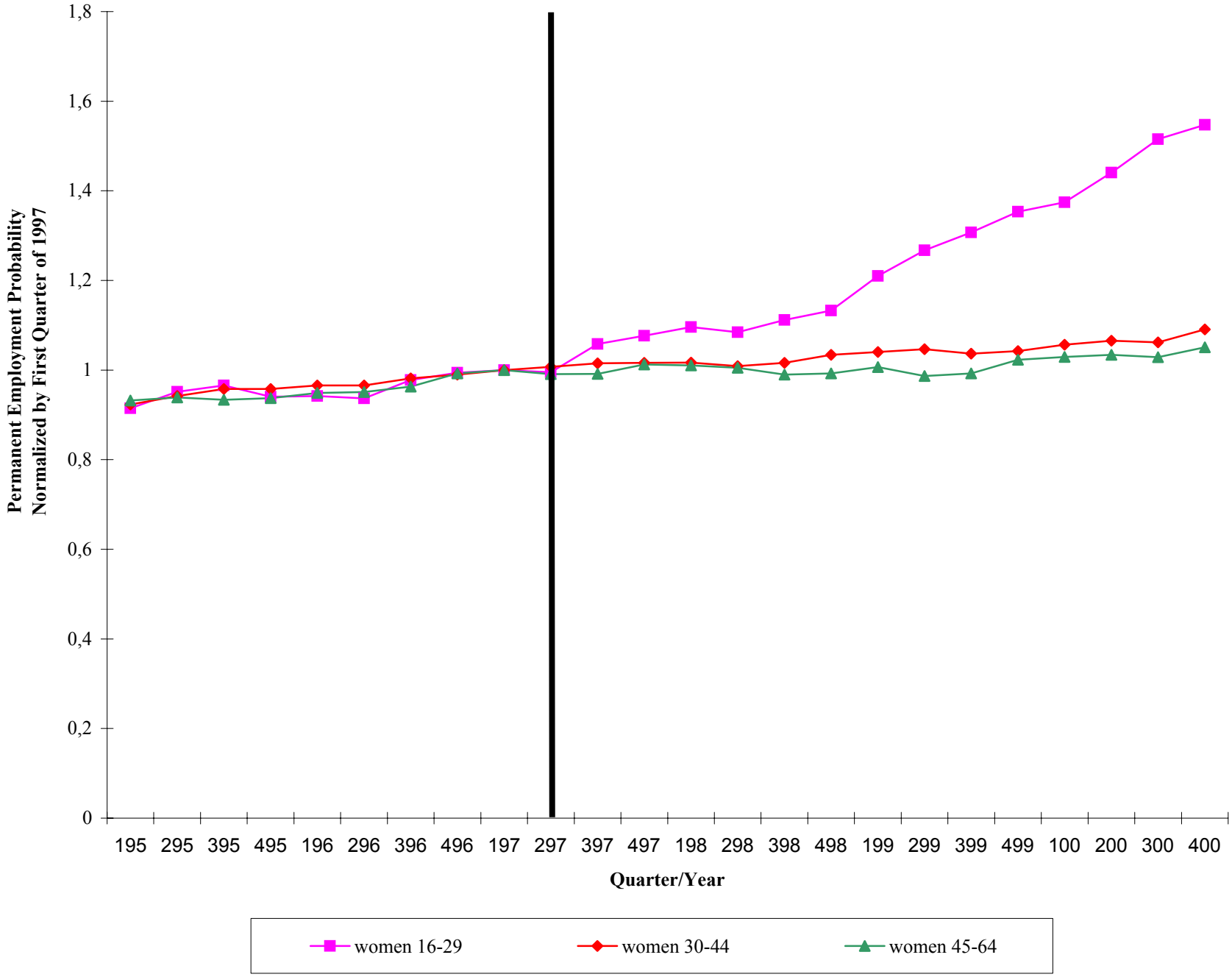

Figure 5: Permanent Employment Probabilities for Men by Age Group Normalized by First Quarter of 1997, 1987-2000

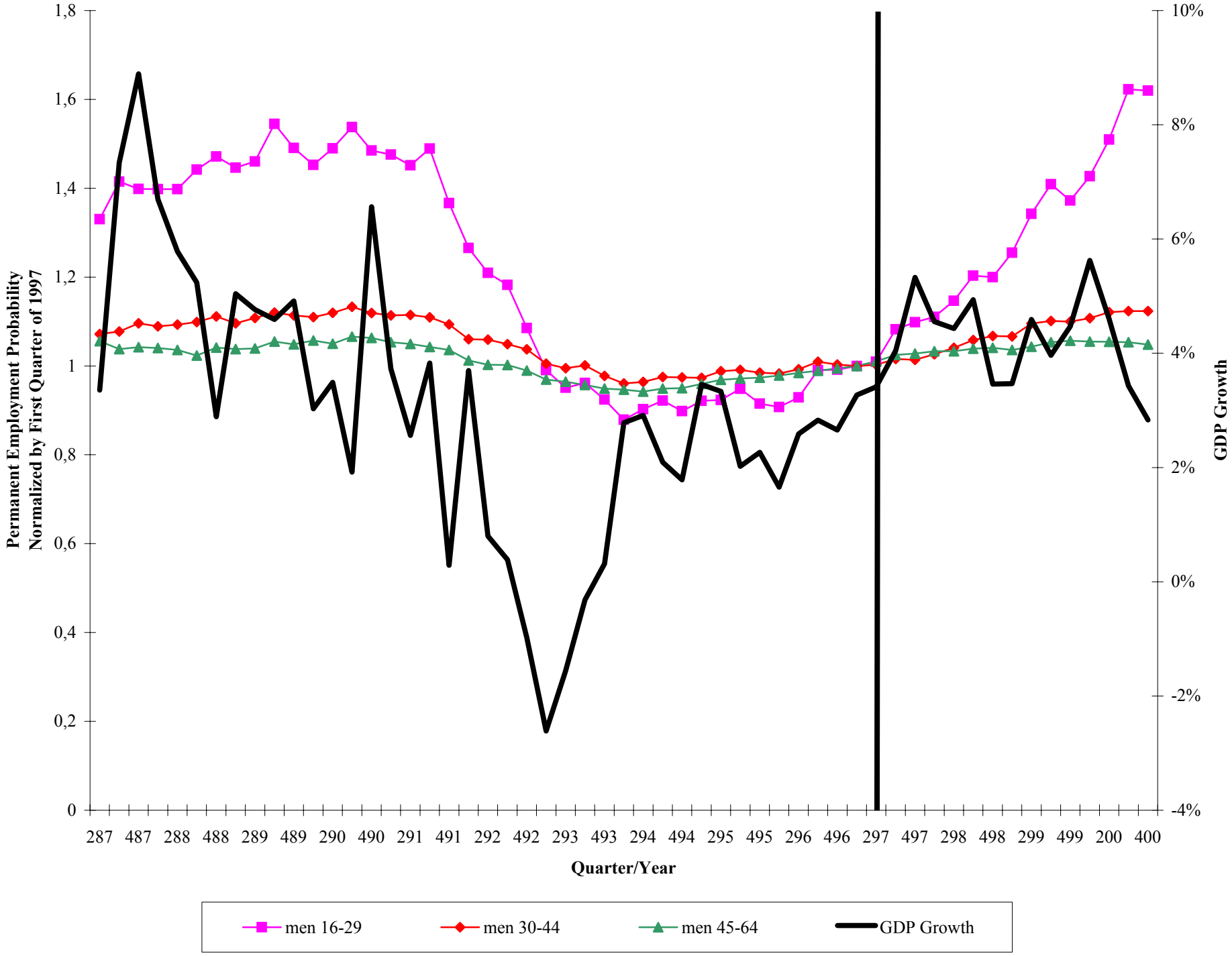

Figure 6: Permanent Employment Probabilities for Women by Age Group Normalized by First Quarter 1997, 1987-2000

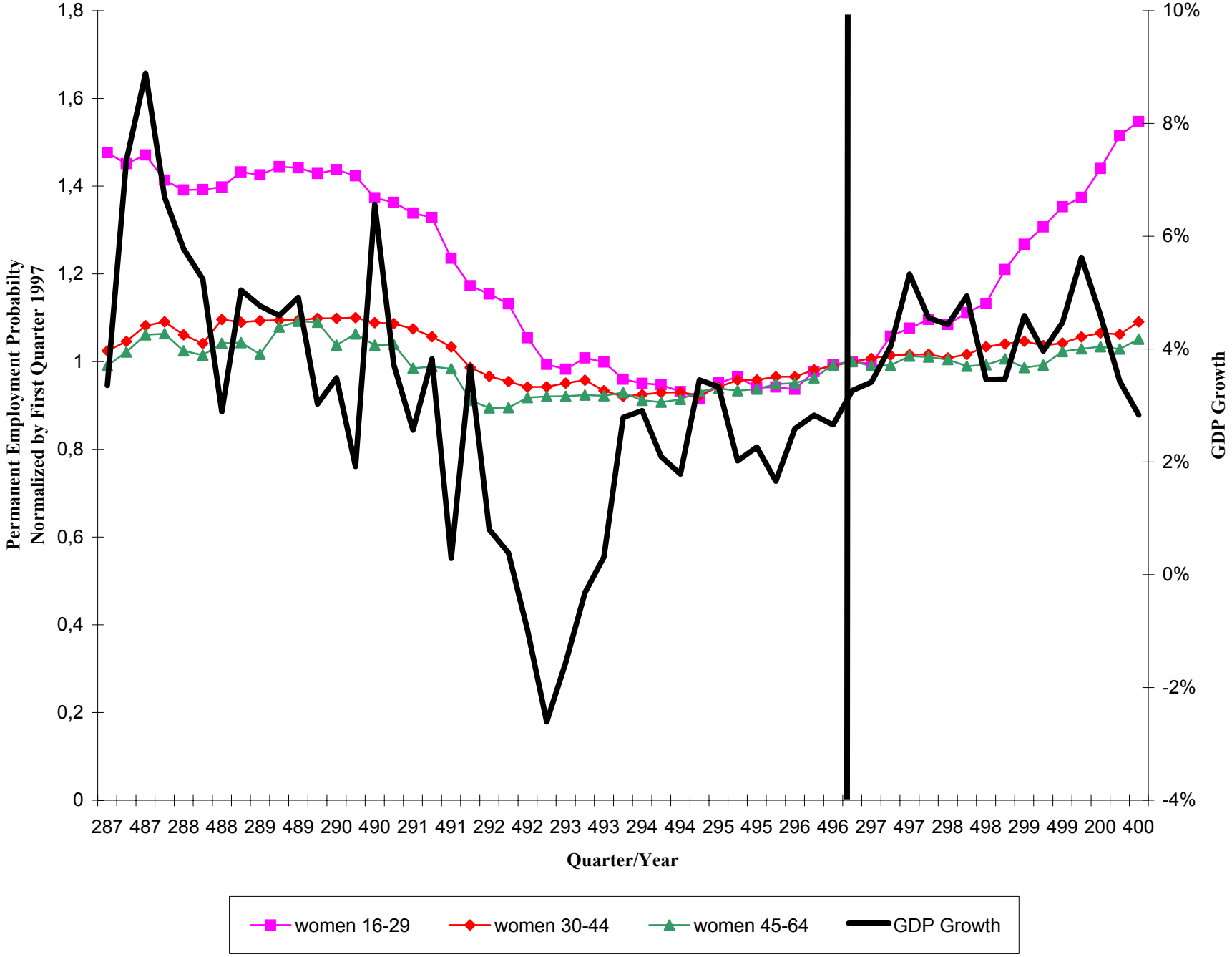

Table 1: Labor Market Reforms after 1997:
Reductions in Payroll Taxes and Dismissal Costs for Permanent Contracts

	Dismissal costs under existing permanent contracts	**Dismissal costs under new permanent contracts**	**Payroll tax reductions for newly hired workers under permanent contracts in 1997-1998**	**Payroll tax reductions for newly hired workers under permanent contracts in 1999**
Unemployed aged 30-44 years	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	None	None
Young unemployed workers (under 30 years of age)	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	40% of employer contributions for 24 months	35% of employer contributions for 12 months, 25% for another 12 months
Unemployed workers above 45 years of age	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	60% of employer contributions for 24 months, 50% thereafter	45% of employer contributions for 12 months, 40% for another 12 months
Long-term unemployed (over 1 year of registered unemployment)	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	40% of employer contributions for 24 months	40% of employer contributions for 12 months, 30% for another 12 months
Workers employed under temporary contracts	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	50% employer contributions for 24 months, 20% for another 12 months	None
Women hired under temporary contracts or long-term unemployed hired in occupations with low weight of female employment	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	60% employer contributions for 24 months, 20% for another 12 months	45% employer contributions for 24 months, 40% for another 12 months
Workers hires under training contracts	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	50% employer contributions for 24 months, 20% for another 12 months	25% employer contributions for 24 months
Workers above 45 years of age hired under temporary contracts	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	60% employer contributions for 24 months, 20% for another 12 months	60% employer contributions for 24 months, 20% for another 12 months
Disabled workers	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 45 days' wages per year of seniority with a maximum of 42 months' wages	Fair dismissals: 20 days' wages per year of seniority with a maximum of 12 months' wages Unfair dismissals: 33 days' wages per year of seniority with a maximum of 24 months' wages	70%-90% for the whole employment spell	70%-90% for the whole employment spell

Table 2: Descriptive Statistics by Age Group, Before and After the 1997 Reform

	Age 16-29		Age 30-44		Age 45-65	
Variable	Pre-Reform	Post-Reform	Pre-Reform	Post-Reform	Pre-Reform	Post-Reform
			A. MEN			
Permanent Employment Probability	0.5709	0.5657	0.8369	0.8329	0.7931	0.8012
Non-employment to Permanent Employment Transition Probability	0.0967	0.0765	0.4048	0.352	0.2476	0.2446
Temporary to Permanent Employment Transition Probability	0.0837	0.0551	0.1031	0.0521	0.0997	0.0416
Permanent Employment to Non-employment Transition Probability	0.0329	0.0202	0.0128	0.0079	0.0241	0.017
Age	23.77	24.59	36.33	37.15	52.63	52.76
	(3.38)	(3.24)	(4.34)	(4.24)	(5.59)	(5.34)
Tenure (in months)	31.67	28.43	117.37	112.95	212.44	219.54
	(37.01)	(33.48)	(87.2)	(87.75)	(137.72)	(136.95)
% Head of Household	21.33	15.64	79.8	75.06	93.52	91.58
% Married	23.82	16.28	82.01	77.93	91.27	91.58
% No Education	1.91	0.87	4.24	1.76	16.03	9.56
% Primary Education	42.95	11.23	46.51	20.84	56.58	44.73
% Secondary Education	34.15	55.63	24.27	42.65	10.78	22.31
% Technical Education	15.61	22.67	13.65	16.62	7.34	7.49
% University Education	5.38	9.58	11.32	18.13	9.27	15.91
N	189,440	29,061	344,099	62,340	330,233	60,956
			B. WOMEN			
Permanent Employment Probability	0.2483	0.2276	0.5192	0.5316	0.4579	0.4873
Non-employment to Permanent Employment Transition Probability	0.0575	0.0484	0.1502	0.1424	0.0963	0.1169
Temporary to Permanent Employment Transition Probability	0.0864	0.058	0.1036	0.0602	0.1383	0.0701
Permanent Employment to Non-employment Transition Probability	0.0518	0.0457	0.0223	0.0201	0.032	0.0265
Age	22.36	23.12	35.99	36.9	52.8	52.41
	(3.74)	(3.68)	(4.32)	(4.24)	(5.78)	(5.44)
Tenure (in months)	27.68	23.77	97.87	95.06	170.23	173.64
	(34.48)	(31.32)	(84.84)	(86.49)	(134.71)	(134.68)
% Head of Household	2.09	2.86	10.22	13.01	20.89	21.64
% Married	22.37	17.86	76.06	76.4	72.26	74.56
% No Education	1.54	0.94	5.17	3.24	22.97	16.56
% Primary Education	34.14	6.67	41.27	19.9	51.37	43.26
% Secondary Education	36.11	53.33	23.9	39.08	10.21	19.93
% Technical Education	17.18	20.54	13.77	14.62	5.48	5.1
% University Education	11.03	18.52	15.91	23.15	9.97	15.15
N	171,155	29,631	226,127	53,043	139,751	32,905

Notes: The table reports means, probabilities, and percentages for the indicated age group. Standard deviations are in parentheses where appropriate.

Table 3: Permanent Employment Probabilities

Regressors	Full Sample (1)	Full Sample (2)	Full Sample (3)	Restricted Age Groups (4)	Restricted Age Groups (5)
			A. MEN		
Age < 30	-0.0404*	-0.0473*	-0.0406*	0.042	-
	(0.0159)	(0.02)	(0.0163)	(0.0075)	
Age >= 45	-0.1909*	-0.2103*	-0.1917*	-	-0.0935
	(0.027)	(0.0344)	(0.0273)		(0.0089)
Age < 30 x Reform	0.0222*	0.0211*	0.0222*	0.0158*	-
	(0.0101)	(0.0092)	(0.0101)	(0.0073)	
Age >= 45 x Reform	0.0043	-0.0091	0.0044	-	0.0057
	(0.0143)	(0.0129)	(0.0141)		(0.009)
Age < 30 x Expansion Dummy	-	0.0117	-	-	-
		(0.0187)			
Age >= 45 x Expansion Dummy	-	0.0364	-	-	-
		(0.0281)			
Age < 30 x GDP Growth	-	-	0.0323	-0.0684	-
			(0.0936)	(0.1325)	
Age >= 45 x GDP Growth	-	-	0.0492	-	0.0278
			(0.137)		(0.1099)
N	711,989	711,989	711,989	325,066	365,216
			B. WOMEN		
Age < 30	-0.0501*	-0.0481*	-0.05*	-0.0032	-
	(0.0124)	(0.0189)	(0.0126)	(0.0096)	
Age >= 45	-0.0321^{+}	-0.0444	-0.0327^{+}	-	-0.0154*
	(0.0194)	(0.029)	(0.0194)		(0.0065)
Age < 30 x Reform	0.016^{+}	0.019*	0.0161^{+}	0.0177**	-
	(0.01)	(0.0076)	(0.01)	(0.009)	
Age >= 45 x Reform	-0.004	-0.0143	-0.0042	-	0.0066
	(0.0141)	(0.0129)	(0.014)		(0.0059)
Age < 30 x Expansion Dummy	-	-0.0043	-	-	-
		(0.022)			
Age >= 45 x Expansion Dummy	-	0.0232	-	-	-
		(0.0328)			
Age < 30 x GDP Growth	-	-	0.0038	0.065	-
			(0.1009)	(0.1258)	
Age >= 45 x GDP Growth	-	-	0.0778	-	0.1008
			(0.2464)		(0.176)
N	465,739	465,739	465,739	275,030	208,793

Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by year/age group. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, education, tenure, and occupation, and province-specific trends. The last row indicates whether and how age-specific cyclical effects are controlled for. The first column does not control for age-specific cyclical effects. The second column controls for age-specific cyclical effects by interacting age groups with an expansion dummy which takes the value of 1 for the years 1987-90 and 1996-2000. Columns (3)-(5) control for age-specific cyclical effects by interacting age groups with GDP growth. The first three columns use the entire sample, while the last two columns restrict the sample to age groups which allow for more comparable treatment and control groups. The sample in Column (4) is restricted to the 20-39 age group and the sample in Column (5) is restricted to the 35-54 age group. *Significant at 1% level, **Significant at 5% level, $^{+}$Significant at 10% level.

Table 4: Transition Probabilities from Non-employment to Permanent Employment

	Full Sample			Restricted Age Groups	
Regressors	(1)	(2)	(3)	(4)	(5)
			A. MEN		
Age < 30	-0.0034	-0.0095	-0.0088	-0.0054	-
	(0.0133)	(0.0206)	(0.0137)	(0.0108)	
Age >= 45	-0.1208*	-0.1114*	-0.1219*	-	-0.0515*
	(0.0212)	(0.0312)	(0.0221)		(0.0146)
Age < 30 x Reform	0.0447*	0.0374*	0.0445*	0.0344*	-
	(0.015)	(0.014)	(0.0148)	(0.0156)	
Age >= 45 x Reform	0.0209	0.0301	0.0203	-	0.0229
	(0.0218)	(0.0229)	(0.022)		(0.019)
Age < 30 x Expansion Dummy	-	0.0123	-	-	-
		(0.0229)			
Age >= 45 x Expansion Dummy	-	-0.0196*	-	-	-
		(0.0415)			
Age < 30 x GDP Growth	-	-	0.4632*	0.3936**	-
			(0.1909)	(0.2022)	
Age >= 45 x GDP Growth	-	-	0.1266	-	0.0675
			(0.2613)		(0.3377)
N	138,039	138,039	138,039	70,484	44,509
			B. WOMEN		
Age < 30	-0.0221*	-0.0148*	-0.022*	-0.0184*	-
	(0.0046)	(0.006)	(0.0048)	(0.0029)	
Age >= 45	0.008	0.0152	0.002	-	0.0018
	(0.0074)	(0.0093)	(0.0087)		(0.0067)
Age < 30 x Reform	0.0096	0.014**	0.0095	0.0083*	-
	(0.0072)	(0.0063)	(0.0071)	(0.0038)	
Age >= 45 x Reform	-0.0061	-0.0016	-0.0078	-	-0.0035
	(0.0083)	(0.0106)	(0.0084)		(0.0067)
Age < 30 x Expansion Dummy	-	-0.0134**	-	-	-
		(0.0072)			
Age >= 45 x Expansion Dummy	-	-0.0135	-	-	-
		(0.0149)			
Age < 30 x GDP Growth	-	-	0.0034	-0.039	-
			(0.1619)	(0.2008)	
Age >= 45 x GDP Growth	-	-	0.4863*	-	0.5434
			(0.2116)		(0.2265)
N	153,541	153,541	153,541	102,844	52,431

Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by year/age group. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, education, tenure, and occupation, and province-specific trends. The last row indicates whether and how age-specific cyclical effects are controlled for. The first column does not control for age-specific cyclical effects. The second column controls for age-specific cyclical effects by interacting age groups with an expansion dummy which takes the value of 1 for the years 1987-90 and 1996-2000. Columns (3)-(5) control for age-specific cyclical effects by interacting age groups with GDP growth. The first three columns use the entire sample, while the last two columns restrict the sample to age groups which allow for more comparable treatment and control groups. The sample in Column (4) is restricted to the 20-39 age group and the sample in Column (5) is restricted to the 35-54 age group. *Significant at 1% level, **Significant at 5% level, $^{+}$Significant at 10% level.

Table 5: Transition Probabilities from Temporary to Permanent Employment

Regressors	Full Sample (1)	Full Sample (2)	Full Sample (3)	Restricted Age Groups (4)	Restricted Age Groups (5)
			A. MEN		
Age < 30	-0.0195*	-0.013**	-0.0199*	-0.017*	-
	(0.0029)	(0.0066)	(0.0028)	(0.003)	
Age >= 45	-0.0018	-0.0096	-0.0025	-	-0.0016
	(0.0061)	(0.0095)	(0.0019)		(0.0023)
Age < 30 x Reform	0.0307*	0.0253*	0.0303*	0.0302*	-
	(0.0048)	(0.005)	(0.0048)	(0.0045)	
Age >= 45 x Reform	-0.0123+	-0.0083	-0.129**	-	-0.0035
	(0.0067)	(0.0068)	(0.0067)		(0.0048)
Age < 30 x Expansion Dummy	-	0.0039	-	-	-
		(0.0071)			
Age >= 45 x Expansion Dummy	-	0.0086	-	-	-
		(0.0096)			
Age < 30 x GDP Growth	-	-	0.077	0.0964	-
			(0.076)	(0.073)	
Age >= 45 x GDP Growth	-	-	0.1195	-	0.0908
			(0.0819)		(0.1171)
N	176,337	176,337	176,337	130,350	54,234
			B. WOMEN		
Age < 30	-0.0203*	-0.023*	-0.0202*	-0.0166*	-
	(0.0029)	(0.0053)	(0.0032)	(0.0031)	
Age >= 45	0.0229*	0.0176*	0.0231*	-	0.0195*
	(0.0033)	(0.0069)	(0.0034)		(0.0027)
Age < 30 x Reform	0.0226*	0.0219*	0.0227*	0.02*	-
	(0.0072)	(0.007)	(0.0072)	(0.0071)	
Age >= 45 x Reform	-0.0105	-0.0126	-0.0104	-	-0.0034
	(0.0074)	(0.0074)	(0.0074)		(0.0051)
Age < 30 x Expansion Dummy	-	0.0039	-	-	-
		(0.0054)			
Age >= 45 x Expansion Dummy	-	0.0078	-	-	-
		(0.0077)			
Age < 30 x GDP Growth	-	-	-0.016	0.0142	-
			(0.0618)	(0.0698)	
Age >= 45 x GDP Growth	-	-	-0.0292	-	-0.0671
			(0.0876)		(0.086)
N	153,471	153,471	153,471	114,643	39,481

Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by year/age group. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, education, tenure, and occupation, and province-specific trends. The last row indicates whether and how age-specific cyclical effects are controlled for. The first column does not control for age-specific cyclical effects. The second column controls for age-specific cyclical effects by interacting age groups with an expansion dummy which takes the value of 1 for the years 1987-90 and 1996-2000. Columns (3)-(5) control for age-specific cyclical effects by interacting age groups with GDP growth. The first three columns use the entire sample, while the last two columns restrict the sample to age groups which allow for more comparable treatment and control groups. The sample in Column (4) is restricted to the 20-39 age group and the sample in Column (5) is restricted to the 35-54 age group. *Significant at 1% level, **Significant at 5% level, +Significant at 10% level.

Table 6: Transition Probabilities from Permanent Employment to Non-employment

Regressors	Full Sample (1)	Full Sample (2)	Full Sample (3)	Restricted Age Groups (4)	Restricted Age Groups (5)
			A. MEN		
Age < 30	0.0098*	0.0094*	0.0099*	-0.0009	-
	(0.0016)	(0.0016)	(0.0015)	(0.0008)	
Age >= 45	0.0124*	0.0129*	0.0125*	-	0.0067*
	(0.0011)	(0.0012)	(0.0011)		(0.0006)
Age < 30 x Reform	0.0002	0.0008	0.0002	0.0018*	-
	(0.0009)	(0.001)	(0.0009)	(0.0008)	
Age >= 45 x Reform	0.0018**	0.0021^{+}	0.0019*	-	0.0021*
	(0.0009)	(0.0012)	(0.0009)		(0.0001)
Age < 30 x Expansion Dummy	-	0.0004	-	-	-
		(0.0011)			
Age >= 45 x Expansion Dummy	-	-0.0007	-	-	-
		(0.0008)			
Age < 30 x GDP Growth	-	-	0.0	-0.0081	-
			(0.0149)	(0.0145)	
Age >= 45 x GDP Growth	-	-	-0.0241	-	-0.0278
			(0.0188)		(0.0243)
N	475,228	475,228	475,228	217,171	293,221
			B. WOMEN		
Age < 30	0.0276**	0.0285*	0.0274*	0.0238*	-
	(0.0012)	(0.0019)	(0.0012)	(0.0011)	
Age >= 45	0.0026^{+}	0.0017	0.0028*	-	-0.0018
	(0.0012)	(0.0015)	(0.0012)		(0.001)
Age < 30 x Reform	0.0029	0.0034	0.0027	0.0009	-
	(0.0027)	(0.0027)	(0.0027)	(0.0026)	
Age >= 45 x Reform	-0.002	-0.0025	-0.0019	-	-0.0004
	(0.0017)	(0.002)	(0.0018)		(0.0019)
Age < 30 x Expansion Dummy	-	-0.0013	-	-	-
		(0.0021)			
Age >= 45 x Expansion Dummy	-	0.0014	-	-	-
		(0.002)			
Age < 30 x GDP Growth	-	-	0.0165	0.0104	-
			(0.0246)	(0.0288)	
Age >= 45 x GDP Growth	-	-	-0.0242	-	0.0003
			(0.0225)		(0.0342)
N	331,559	331,559	331,559	195,085	177,092

Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by year/age group. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, education, tenure, and occupation, and province-specific trends. The last row indicates whether and how age-specific cyclical effects are controlled for. The first column does not control for age-specific cyclical effects. The second column controls for age-specific cyclical effects by interacting age groups with an expansion dummy which takes the value of 1 for the years 1987-90 and 1996-2000. Columns (3)-(5) control for age-specific cyclical effects by interacting age groups with GDP growth. The first three columns use the entire sample, while the last two columns restrict the sample to age groups which allow for more comparable treatment and control groups. The sample in Column (4) is restricted to the 20-39 age group and the sample in Column (5) is restricted to the 35-54 age group. * Significant at 1% level, **Significant at 5% level, $^{+}$Significant at 10% level.