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

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Abstract

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 levels 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 and older men on permanent contracts. The results suggest a moderately elastic response of permanent employment to non-wage labor costs for young men and a less elastic response for older men. Consistent with both dismissal cost and payroll tax effects, we also find large positive effects on the transitions from unemployment and temporary employment into permanent employment and moderate positive effects on the transitions from permanent employment to non-employment for young and older men.

Keywords: Temporary Employment, Dismissal Costs, Payroll Taxes, European Unemployment.

JEL Codes: J23, J32, J38, J63, J65.

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 institutions has been difficult to establish is that institutional changes in Europe have been so widespread that it is difficult 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 have undesirable consequences for output, employment, and segmentation of the labor market.[1]

In this paper we assess the impact of a recent reform in the Spanish labor market. A study of the recent Spanish experience is especially compelling because, in

[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.

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 unexpected sharp reductions in payroll taxes and dismissal costs for permanent contracts. A reduction in dismissal costs for permanent contracts should have ambiguous effects on employment because hiring and dismissals both increase, but a reduction in payroll taxes should unambiguously increase employment as long as the fall is not shifted entirely to higher wages. Consequently, by examining the impact of the reforms on hirings and separations, we can infer whether the effects came from the reduction in dismissal costs and/or the reduction in payroll taxes. Moreover, by estimating the impact of the reforms on employment levels we can provide an estimate of the elasticity of permanent employment with respect to total non-wage labor costs.

In this paper we examine the impact of the 1997 reforms on employment levels 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 Spanish reforms increased permanent employment probabilities for young and older men. The results for men are robust to controls for common macro shocks for all age groups, for age-specific trends, and for age-specific cyclical effects. The results suggest a moderately elastic response of permanent employment to non-wage labor costs for young men and a less elastic response for older 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 and from permanent employment to non-employment for both young and older men. In addition, we find some evidence showing that reduced non-wage labor costs for contract conversions increased transitions from temporary to permanent employment for men of all age groups. On the other hand, like Blundell et al. (2004), we find little effect of subsidies and reduced dismissal costs on women.

Previous studies looking at the impact of employment regulations in Europe have often relied on macro data (e.g., Nickell and Layard (1999), and Lazear (1991)).[2] However, the sensitivity of macro studies to the inclusion of time-varying covariates and country effects highlights the importance of finding within country variation to study the effects of regulations. Recent studies attempt to exploit the differential impact of labor market regulations for different groups of workers within a country. These include Blundell et. al. (2004) and Hunt (1995), who examine the impact of job assistance and wage subsidies in the U.K. and unemployment insurance in Germany by exploiting age-related eligibility. Acemoglu and Angrist (2001) and Oyer and Schaeffer (2000) exploit differential dismissal costs for different groups of workers by disability status and race in the U.S. Like earlier studies, our study uses individual panel data and exploits group-related eligibility rules to study the impact of regulations on employment and worker flows. Blundell et al. (2004), which is probably the closest to our study in

[2] While most studies examine the effect of regulations on employment, using macrodata, more recent studies look at the impact of regulations on worker effort and workplace accidents using microdata (see, e.g., Ichino and Riphahn (2005) and Guadalupe (2003)).

its use of age-eligibility criteria to evaluate subsidies, find that the New Deal for young people in the U.K. raised transitions to employment by about 5 percentage points, while we find that the Spanish reforms raised transitions from non-employment to permanent employment by 4 percentage points. In other respects our study differs. First, we control for group-specific cyclical effects since the Spanish reform was introduced during an expansion and different age-groups appear to be affected differently by the business cycle. Second, we examine the impact of the reforms on all transitions and not only on transitions from non-employment to employment or from employment to non-employment. Moreover, to the best of our knowledge, our study is the first European study to examine the impact of dismissal costs on employment and worker flows using clean treatment and control groups.

The paper is organized as follows. Section II describes the institutional framework and the Spanish labor market reforms. Section III explains the natural experiment research design used to evaluate the impact of the 1997 reforms. Section IV describes the data and presents estimates of the effects of the reforms on employment levels, accessions and separations, and contract conversions. We conclude in Section V.

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 administrative approval.

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, up to a maximum of 42 months. Severance payments for "economic reasons" were the same as for fair dismissals under "personal reasons." In practice, this turned out to be 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, 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. In particular, temporary workers were entitled to severance pay of 12 days per year of seniority, which could not be appealed in labor courts. However, temporary contracts could only be used up to maximum of three consecutive years.

As a result of the 1984 reform, the proportion of employees on temporary contracts increased from 10% during the 1980s to over 30% in the early 1990s. 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%.[3] 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.[4] In practice, employers found ways to get around this restriction and 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, approval for collective dismissals under "economic reasons" continued to be granted mainly when there was an agreement between employers and workers which, as mentioned above, was often achieved by establishing severance payments above the legally required amount. Consistent with the small real impact that the 1994 reform seems to have generated, below we find no evidence of an increase in new permanent contracts after 1994.

The perceived ineffectiveness of the 1994 reform led to a new reform in 1997, which was initially extended in 1999 and eventually adopted indefinitely in 2001. This reform was introduced unexpectedly after the employer confederation and the two major trade unions reached an agreement in terms of reductions of dismissal costs for permanent contracts, which had not happened in the last 20 years. As with the 1994 reform, the goal of the 1997, 1999 and 2001 reforms was to increase the use of permanent contracts. However, rather than trying to achieve this goal by limiting the use of temporary contracts through further possibly ineffective regulation, the new

[3] See Bover and Gómez (2004).

reform increased the incentives for firms to hire workers in certain population groups using permanent contracts. In particular, the 1997 reform reduced unfair dismissal costs by about 25% and payroll taxes between 40% and 90% for newly signed 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. In addition, the reform reduced unfair dismissal costs by about 45% and payroll taxes by 50% for conversions of temporary into permanent contracts for all age groups.

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 new permanent contracts.[5] Table 1 shows payroll taxes falling by 40% for workers under 30 years of age and for the long-term unemployed, by 60% for workers over 45 years of age and women under-represented in their occupations, and by 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.[6]

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[4] In the case of workers over 45 years of age, temporary contracts could continue 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.

[5] 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.

[6] In 1999 the reform was initially extended with the same reductions in dismissal costs, but with smaller payroll tax reductions. 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.

The research value of the 1997 and subsequent reforms is partly due to the fact that the new regulations affected different groups of workers differently.[7] In particular, the reforms introduced after 1997 changed payroll taxes and dismissal costs over time for newly signed permanent contracts 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 worker flows.[8] Although payroll taxes and dismissal costs fell simultaneously for the targeted groups, because the reduction in dismissal costs should affect both hiring and dismissals while payroll tax subsidies should only affect hiring, we can infer whether it was the change in dismissal costs or payroll taxes that affected the labor market by examining both accessions and separations.[9]

The reforms introduced after 1997 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.[10] The figures show that the number of newly signed permanent contracts increased sharply for young men and women 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

[7] For short, we refer to the 1997 and subsequent reforms as the 1997 reforms.
[8] However, for our analysis of transitions from temporary to permanent employment, we can only exploit the temporal variation in the legislation since reduced dismissal costs and payroll taxes applied to conversions of all age groups.
[9] See the Appendix for a model showing the differential effects of dismissal costs and payroll taxes on hiring and dismissals.
[10] The data for Figures 1 and 2 come from the official registry of contracts reported to the Spanish Labor Department.

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 temporary into permanent contracts after the second quarter of 1997 for both men and women. The sharp rise in the figures right after the second quarter of 1997 suggests the reforms affected conversions of temporary into permanent hires as well as new permanent hires for young and older workers.

III. 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 reforms. 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, our data do not report disability status. Moreover, as shown in Figures 1 and 2 above, the greatest impact of the reforms 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 entire period for which we have data (i.e., 1987 to 2000). This includes an economic expansion over the reform period as well as another expansion in the late 1980s and a recession in the early 1990s. The figures show that permanent employment probabilities started to increase right after the implementation of the reform (i.e., second quarter of 1997) and that the increase was greatest for younger workers. At the same time, the figures also show high permanent employment probabilities for the young during the expansion of the late 1980s.[11] Thus, the figures highlight the importance of proper control for cyclical effects, especially because the young appear to benefit disproportionately during expansions. On the other hand, the figures show similarly high permanent employment probabilities during the two expansions, even though the expansion of the late 1980s was stronger than the expansion of the late 1990s 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.

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

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

where $e_{it}=1$ if the person is employed with a permanent contract and 0 if the person is non-employed (i.e., either unemployed or out of the labor force); α_t is a year effect, d_i is an age-group effect, and X_{it} includes covariates affecting individual i at time t, including quarter dummies. The age-group effects capture differential permanent employment rates of the treated groups before and after the reforms, while the quarter and year effects capture the impact of seasonal and macro shocks affecting workers in both treated and control groups.[12] In some specifications, we also include age-specific trends, by replacing d_i with $d_{0i} + d_{1i}t$, to control for factors affecting employment

[11] Similarly, Bover, Arellano and Bentolila (2002) find that favorable business conditions in Spain increase the hazard of leaving unemployment.
[12] In addition, including quarter and year effects helps to control for cohort effects.

differentially in different age-groups over time.[13] 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 GDP growth as well as age-specific trends. That is, the estimating equation is modified to be

$$\Pr[\, e_{it}=1 \mid X_{it}, di \,] = \Lambda[\, \alpha_t + d_{0i} + d_{1i}t + \gamma' X_{it} + \delta_G'(d_i \times G_t) + \delta_R'(d_i \times R_t) \,], \tag{2}$$

where G_t denotes GDP growth. Here, as before, the impact of the reforms is captured by the reform/treatment group interaction term, 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 GDP growth/treatment group interactions, δ_G. We estimate equations (1) and (2) in samples limited to narrow groups, concentrated around the affected age groups. In particular, we concentrate on workers between 20 and 39 years of age for the group of young workers and on workers between 35 and 54 years of age for the group of older workers.[14]

Transition probabilities from non-employment to permanent employment and from permanent employment to non-employment, were estimated by fitting equations (1) and (2) conditional on the relevant labor market state. That is, all parameters are free to 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

[13] The inclusion of age-specific trends helps to control for the serial correlation problem in differences-in-differences inference pointed out by Bertrand, Duflo, and Mullainathan (2003) if serial correlation is persistently positive or negative, as is the case with employment. We also tried specifications including province-specific and sector-specific trends to control for factors potentially affecting employment differentially in different provinces and sectors over time. Results with province and sector trends are very similar to those with age-specific trends for young men, while results on accessions become significant for older men and insignificant for young women with age-specific trends instead of province or sector trends. We report results with age-specific trends because, given that treatment groups are defined by age, including age-specific trends is more relevant and more restrictive in this context.

by allowing differential transition probabilities for treated groups during the expansions of the late 1980s and 1990s. By contrast, since reduced non-wage labor costs for conversions applied across the board for all age groups, models of transitions from temporary to permanent employment do not control for year and age-group effects but instead include a time trend and a post-reform dummy.[15]

Like the estimates in Blundell et al. (2004), our estimates capture the total effect of dismissal costs and subsidies on employment levels and worker flows. In particular, our estimates capture the direct effect of the reforms as well as potentially general equilibrium effects. There are two potential general equilibrium effects. First, after the reform, employers may have the incentive to substitute the cheaper under-30 and over-45 year olds for those not covered. Substitution in this direction would imply that the closer in age the control group, the larger the estimated effect would be as it would capture both an increase in employment for the treated group as well as a decrease (due to substitution) for the control group. We check for the possibility of substitution by looking at broader age groups (16-34 and 30-59 year olds) to see if the effects are indeed consistently smaller for these groups.[16] Second, lower dismissal costs after the reforms could have decreased reservation wages and increased flows into employment and decreased flows out of employment. The general equilibrium effect on wages implies effects on worker flows going in opposite directions, so that this would imply that we would overestimate the effects on hiring but underestimate the effects on dismissals. Unfortunately, we do not have wage information in our data, but the

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[14] By restricting the samples to 10 years above and 10 years below the cutoff age for treatment status, we can avoid confounding the effects of the labor market reforms with effects due to early retirement (at age 60) on the group of older workers.

[15] Since there is no control group for the analysis of conversions, in this case, it is not possible to control for other macro shocks coinciding with the reforms. For this reason, the results on conversions have to be interpreted much more cautiously.

presence of downward wage rigidities suggests this is not as much of a concern in the Spanish context since most adjustments probably take place through quantities rather than through prices.[17]

IV. 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.[18] The LFS covers basic individual and family characteristics, 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.[19] We exclude individuals in the military, workers employed in agriculture, as well as employers, co-op members, family workers and the self-employed from our sample. Our original sample includes men and women between 16 and 65 years of age, but we then restrict our sample to 20-39 and 35-54 year olds 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) report an attrition rate of about 20% in the rotating panel, which is close to that found

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[16] Blundell et al. (2004) also check for substitution effects by looking at the program impact in pathfinder areas and non-Pathfinder areas not initially exposed to the program. We check for substitution instead by looking at broader age-groups, since the 1997 labor market reforms applied to all Spanish provinces.

[17] See, e.g., Nickell and Layard (1999) and Kramarz (2001) for evidence of wage rigidities in Europe.

[18] 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 3 and 4, they do not appear to have affected estimates of individual employment probabilities for those in particular age groups.

for similar data sets in other countries.[20] 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.

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 1980s. 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 the head of the household during the reform period.

B. Employment and Worker Flow Effects

Table 3 reports logit marginal effects estimated using equations (1) and (2). The dependent variable is a dummy that indicates employment on a permanent contract. The controls are head of household and marital status dummies, four schooling groups, tenure, seven occupation groups, 10 sector groups, year effects, quarter effects, 15 province groups, and under-30 and over-45 age groups, depending on whether we are considering the sample of young or older workers. The effects of interest are captured

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[19] The Spanish LFS does not have earnings information, so we cannot study the effect of payroll taxes and dismissal costs on wages. However, as pointed out above, the presence of downward wage rigidities in the Spanish context probably implies that most adjustments take place through quantities.

[20] 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 generates important selection biases in estimating quarterly transition probabilities.

by the interactions of the under-30 and over-45 age groups with the reform dummy.[21] 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.[22]

Results in Table 3 show significant positive effects of the reforms on permanent employment of young and older men after controlling for age-specific trends. The reported standard errors allow for clustering by year-age group to control for common random effects within the 28 cells (14 years and 2 age groups for young and middle-aged and older and middle-aged, respectively).[23] Panel A reports results of young workers and Panel B for older workers. Results controlling for age-specific time trends and cyclical effects in Column (3) show an increase in permanent employment probabilities of 0.015 (i.e., 2.6%) for young men and of 0.17 (i.e., 2.1%) for older men after the reforms.[24] To check for substitution of younger and older men for middle-aged workers, we also look at samples of 16-34 and 30-59 year olds. While the estimate for young men in the broader sample is smaller than the one in the narrower sample, they are not statistically different from each other. Moreover, contrary to substitution effects, the estimate for older men in the broader sample is actually larger than in the

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[21] Since it is the age at which you entered the contract that determines the dismissal costs and payroll taxes, the age group and post-reform dummies are defined by the age and quarter at the time of hiring. Thus, for 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 employment probabilities and 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).

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

[23] 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 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.

[24] Controlling for age-specific time trends and cyclical effects seems important, as young workers are more likely to be employed during booms and older workers faced a downward trend during this time-period, so that failing to control for these would overestimate the effect on young workers and under-estimate the effect on older workers.

narrower sample. Positive effects on employment could be due to the effects of payroll tax subsidies alone or a net positive effect of dismissal costs. We turn next to the effects on worker flows to check whether the effects are consistent with payroll taxes or dismissal costs driving these employment changes.

Results in Table 4 show increased transitions from non-employment to permanent employment for young and older men relative to middle-aged workers after the 1997 reforms. The table reports logit marginal effects from models for transitions from non-employment to permanent employment. The dependent variable indicates transitions from non-employment to permanent employment from one period to the next in the sample of non-employed.[25] As before, Panel A reports the results for young workers and Panel B for older workers. Results controlling for age-specific trends and cyclical effects in Column (3) show an increase in the relative transition probabilities from non-employment to permanent employment of 0.04 or about 40% for younger men and of 0.05 or about 20% for older men relative to middle-aged men during the reform years.[26] Here we also examine the magnitude of the effects on broader samples to check for substitution of treated for control individuals. While the estimate for young men is smaller in the broader sample, the estimate is not significantly different from the one in the narrow sample. Similarly, the estimate on the broader sample of older men is only slightly smaller and, again, not statistically different from the one in the narrow sample. Positive effects on accessions could be due to reduced payroll taxes or dismissal costs. On the other hand, payroll tax subsidies should have no effect on separations while reduced dismissals costs should increase separations, so we turn next to the effects on transitions from employment to non-employment.

[25] The controls for transition probabilities are as in the permanent employment probability specifications.
[26] Like for employment probabilities, we find increased hiring of young workers during expansions and a negative trend in hiring for older workers.

Results in Table 5 show moderate positive effects on the transitions from permanent employment to non-employment for young and older men after the reforms. The table reports logit marginal effects from models for transitions from permanent employment to non-employment. The results in Panels A and B show a rise in the transitions from permanent employment to non-employment for young and older men relative to middle-aged men during reform years of about 6%, whether or not age-specific trends and cyclical effects are included.[27] To check for substitution of treated men for middle-aged men, we re-estimate our results on the broader samples of young and older men. Contrary to what would be expected if dismissals of younger and older workers were replacing dismissals of similar middle-aged workers, the results on the broader samples show bigger effects. Effects on both separations as well as accessions suggest the reduction in dismissal costs is driving the changes in the labor market after the reforms. At the same time, we find much larger effects on hiring suggesting that payroll tax subsidies are probably also contributing to the observed changes in worker flows after the reform.

The results for men in Tables 3-5 show that the reforms increased hiring as well as dismissals of younger and older men, with a net positive effect on employment. By contrast, the results for women show little effect on hiring, dismissals and employment. Although, there seem to be positive effects on employment and accessions of young women after the reform, the effects become insignificant when age-specific trends are included. Given a systematic trend in the labor market behavior of younger compared to middle-aged women, it is clear that the problem here is the inappropriateness of the

[27] The results also show that older men are more likely to be dismissed during recessions than middle-aged men, once again highlighting the importance of controlling for age-specific cyclical effects.

comparison group. Coincidentally, Blundell et al. (2004) encounter the same problem in their study of the New Deal on women.

While the results for men in Tables 3-5 suggest the reforms increased hiring, dismissals, and net employment, they also suggest the presence of pre-existing age-specific trends. To check for the possibility that the results are being driven by pre-existing changes, we try a falsification test similar to the one used by Angrist and Krueger (1999), who used the "Failed Mariel Boatlift" to look for spurious estimates of the relation between immigration and employment. We use the "Failed 1994 Reform" as a falsification test for our research design to look at the impact of regulations on the Spanish labor market. Column (4) of Tables 3-5 report results of equation (2), but replacing the reform/age-group interaction term with an interaction of the post-1994 period with age-groups, and estimated using the sample in the pre-reform period only. In contrast to results using 1997 as the (actual) reform year, results for the "Failed 1994 reform" show a negative effect on permanent employment of young men and no effect on permanent employment of older men; no effects on accessions for young and older men; and a marginally significant positive effect on separations of older men but no effect on separations of younger men. The very different effects on employment and worker flows for the pre-reform period provide additional evidence that the effects on young and older men after the second quarter of 1997 are likely to be driven by the labor market reforms.

C. Effects on Temporary to Permanent Conversions

Results in Table 6 show some evidence of increased conversions of temporary into permanent contracts for men after the 1997 reforms. The table reports logit marginal effects from models for transitions from temporary to permanent employment. However, because lower dismissal costs and payroll taxes applied across-the-board to all age groups, these regressions do not have a control group, so we control for time trends and look at the post-reform term as the effect of interest. Results with trends in Columns (2) and (6) show increased conversions of temporary to permanent employment of about 30% for men and of about 15% for women after the 1997 reforms. Results from the falsification tests in Columns (4) and (8) show a smaller and marginally significant increase in conversions for men, but a similar increase in conversions for women after 1994 using the pre-reform sample. Given the absence of a control group in this specification and the fact that we find similar increases in conversions for women during the pre-reform period, suggests that the effect on women is being driven by pre-existing trends. By contrast, we find a significantly greater increase in conversions for men after 1997 compared to the pre-reform period of 0.027 or about 30%.

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 reforms. We concentrate on young and older men because the results for women show insignificant effects.

The 1997 reform reduced 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. Payroll taxes for older workers fell by 60% for the first two years and by 50% thereafter for contracts signed in 1997 and 1998, and by 45% the first year and by 40% the second year for contracts signed after 1999. 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 and 2.4% for older men). The probability of ruling a dismissal unfair in Spain is 0.72.[28] Costs for unfair dismissals can be estimated based on the following formula:

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

Mean salaries from the Survey of Salary Structure for 1995 indicate a yearly salary of 10,680 Euros for young men and 20,892 Euros for older men. From the LFS we get mean tenure of 2.16 and 17.2 years for young and older men in 1995. Combining these numbers, we get quarterly expected dismissal cost of 17 and 191 Euros for young and older men, respectively.[29]

Payroll tax costs are easier to obtain. The payroll tax rate is 28.3%, implying an average quarterly payroll tax cost of 756 and 1,478 Euros for young and older men, respectively. Consequently, dismissal costs and payroll taxes account for about 1% and 21% of total labor costs for young men and for 3% and 22% of total labor costs for older 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 reduction of 40% for young workers, the percent reduction in total labor costs implied by the reform for young men was of 8.9%. Using the smaller payroll tax reduction of 35% for young workers applied

[28] 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.

during the second year of the contract, the percent reduction in total labor costs implied by the reform for young men was of 7.8%. Using the large tax reduction of 60% for older men, the percent reduction in total labor costs implied by the reform was of 13.6%, while using the smaller payroll tax reduction of 45%, the percent reduction in total labor costs was of 10.4%.

Results in Table 3 that control for age-specific trends and cyclical effects suggest the reform increased permanent employment probabilities by 0.0151 or 2.6% for young men and by 0.0168 or 2.1% for older men. These results imply elasticities for young men of between –0.29 and –0.33 using the payroll tax reduction of 40% and 35%, respectively. By contrast, the results imply elasticities for older men of –0.15 and –0.19 using the payroll tax reductions of 60% and 45%, respectively. The results suggest a moderately elastic employment response of young men to changes in non-wage labor costs, but a less elastic response of older workers. The elasticities are well in the range of labor demand elasticities 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 than for older and skilled workers.

V. 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 reforms after 1997, which reduced payroll taxes and dismissal costs, to set up a research design based on the fact that the reforms applied differently to different age groups. In principle, the reduction in dismissal costs should increase hiring and dismissals, with an ambiguous effect on employment. On the other hand, the reduction in payroll taxes

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[29] This means we do no have to consider the change in the maximum payment of dismissal costs from 42

should increase hiring and, thus, permanent employment. Estimates using the Spanish Labor Force Survey suggest the reforms increased permanent employment probabilities for young and older men relative to middle-aged men. The results are robust to controls for common macro shocks for all age groups, and for age-specific trends and cyclical effects. The results also show increases in the relative transitions from non-employment to permanent employment and from permanent employment to non-employment for young and older men. Moreover, results for accessions and separations based on broader age-groups suggest that the reform is not simply generating substitution of middle-aged for younger and older men. In addition, we find some evidence showing increased conversions of temporary into permanent jobs for men after the 1997 reforms. Falsification tests examining the effects of the "Failed 1994 reform" during the pre-reform period, suggest the results are not simply driven by pre-existing changes in the labor market. By contrast, weak effects on women seem to reflect the lack of a good control group as well as pre-existing trends for women.

Our results suggest that the reduction in dismissal costs and payroll taxes had a positive effect on both hiring (both new hiring as well as conversions) and dismissals of young and older men, but a bigger effect on hiring. This explains why the reform seems to have had a positive net effect on permanent employment for young and older men. Given that both hiring and dismissals were affected, the results suggest that reduced dismissal costs were at least partly behind the changes in labor market dynamics after 1997. At the same time, bigger effects on hiring also point to the likely role played by lower payroll taxes after the 1997 reforms. Finally, evidence on both increased conversions as well as new hires out of unemployment for men, suggests the reforms

to 24 months, since it is never binding.

did not simply encouraged substitution of temporary for permanent jobs but also encouraged the creation of new permanent jobs.

The estimated elasticities suggest a moderately elastic response of permanent employment to non-wage labor costs for younger men and a less elastic response for older men. 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.K., 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 reduce employment levels in Spain, especially for young and older men. On the other hand, given the moderate response of employment to payroll taxes and dismissal costs, non-wage labor costs alone are unlikely to explain the bulk of 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 workers in Europe.

Appendix: **Theoretical Analysis of the Effects of the Reform**

We present a simple dynamic model incorporating payroll taxes and dismissal costs for permanent contracts in a dual labor market. The model extends Blanchard and Landier (2002) by introducing payroll taxes and by allowing for endogenous destruction of jobs under permanent contracts. The main point is to distinguish the differential effects of a reduction in payroll taxes and a reduction in dismissal costs.

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.[30] 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:

$$rJ_T(\varepsilon_o) = \varepsilon_o - (1+s_T)w_T(\varepsilon_o) + \lambda \max\{E(J_P(\varepsilon')-J_T(\varepsilon_o) \mid \varepsilon' \geq \underline{\varepsilon}),0\},$$
$$rJ_P(\varepsilon) = \varepsilon - (1+s_P)w_P(\varepsilon) + \lambda E(J_P(\varepsilon')-J_P(\varepsilon) \mid \varepsilon' \geq \bar{\varepsilon}) + \lambda(J_T(\varepsilon_o)-J_P(\varepsilon)-F)G(\bar{\varepsilon})$$

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 φ=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

[30] By imposing that temporary jobs need to be either converted into permanent or terminated after the first productivity shock we are capturing some features of the regulation of fixed-term contracts that limit them from being extended forever.

unemployed are $W_T(\varepsilon_o)$, $W_P(\varepsilon)$, and U, and are given by the following Bellman equations:

$$rW_T(\varepsilon_o) = (\ 1+bs_T\ )w_T(\varepsilon_o) + \lambda\max\{E(\ W_P(\varepsilon')-W_T(\varepsilon_o) \mid \varepsilon' \geq \underline{\varepsilon}\ ),0\} + \\ + \lambda(\ U-W_T(\varepsilon_o)\ )\ G(\ \underline{\varepsilon}\ )$$

$$rW_P(\varepsilon) = (\ 1+bs_P\ )w_P(\varepsilon) + \lambda\max\{E(\ W_P(\varepsilon')-W_P(\varepsilon) \mid \varepsilon' \geq \bar{\varepsilon}\ ),0\} + \\ + \lambda(\ U-W_P(\varepsilon)\ )G(\ \bar{\varepsilon}\ )$$

$$rU = \varphi\max\{\ W_T(\varepsilon_o)-U\ ,0\}$$

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:

$$J_P(\ \underline{\varepsilon}\ ) = J_T(\ \varepsilon_o\ ) = K \tag{3}$$

$$J_P(\ \bar{\varepsilon}\ ) = J_T(\ \varepsilon_o\ ) - F \tag{4}$$

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:

$$J_T(\ \varepsilon_o\ ) - K = W_T(\ \varepsilon_o\ ) - U \tag{5}$$

$$J_P(\ \varepsilon\ ) - J_T(\ \varepsilon_o\ ) + F = W_P(\ \varepsilon\ ) - U \tag{6}$$

Substituting the free-entry condition into equation (5) 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 and over $\bar{\varepsilon}$ and ε_m, yields $E(J_P(\varepsilon')-W_P(\varepsilon') \mid \varepsilon' \geq \underline{\varepsilon}\ ) = (K-F)(1-G(\ \underline{\varepsilon}\ ))$ and $E(J_P(\varepsilon')-W_P(\varepsilon') \mid \varepsilon' \geq \bar{\varepsilon}) = (K-F)(1-G(\ \bar{\varepsilon}\ ))$. Using these expressions together with the fact that the value of being unemployed is zero and with the Bellman equations for temporary and permanent jobs yields the following wages,

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

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,

$$(r+\lambda)K = \{[(1+bs_P)\,\underline{\varepsilon} + r(K-F)]/[2 + (1+b)s_P]\} + \lambda E(\,J_P(\varepsilon') \mid \varepsilon' \geq \bar{\varepsilon}\,) + \lambda(K-F)G(\,\bar{\varepsilon}\,) \tag{7}$$

$$(r+\lambda)(K-F) = \{[(1+bs_P)\,\bar{\varepsilon} + r(K-F)]/[2+(1+b)s_P]\} + \lambda E(\,J_P(\varepsilon') \mid \varepsilon' \geq \bar{\varepsilon}\,) + \lambda(K-F)G(\,\bar{\varepsilon}\,) \tag{8}$$

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. In contrast Blanchard and Landier (2002) who find that reducing temporary dismissal costs reduces permanent conversions and increases wage differentials, here a reduction in permanent dismissal costs increases permanent conversions and reduces wage differentials. Thus, unlike previous reforms affecting temporary contracts only, the 1997 reform should reduce labor market segmentation.

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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 (and not already in one of the other groups)	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 hired 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

	Men				Women			
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Young				
Age < 30 × Reform	0.017**	0.0186**	0.0151**	-	0.0175**	-0.0099	-0.0107	-
	(0.008)	(0.0091)	(0.008)		(0.0091)	(0.0106)	(0.0106)	
Age < 30 × Post-1994	-	-	-	-0.0179*	-	-	-	0.017
				(0.0042)				(0.012)
Age < 30 × Trend	-	-0.0007	-0.0012	0.0005	-	0.0114*	0.0117*	0.0003
		(0.0009)	(0.0008)	(0.0007)		(0.0023)	(0.0024)	(0.0019)
Age < 30 × GDP Growth	-	-	0.0031^{+}	0.0014**	-	-	0.0913	0.1244
			(0.0013)	(0.0011)			(0.1371)	(0.1282)
N	327,082	327,082	327,082	259,706	276,202	276,202	276,202	162,788
				B. Older				
Age > 45 × Reform	0.004	0.0165*	0.0168**	-	-0.0071	0.0001	0.0011	-
	(0.0063)	(0.0069)	(0.0072)		(0.0058)	(0.006)	(0.0062)	
Age > 45 × Post-1994	-	-	-	0.0075	-	-	-	0.0034
				(0.0048)				(0.0121)
Age > 45 × Trend	-	-0.0489*	-0.0049*	-0.0054*	-	-0.0032*	-0.0003*	-0.0058
		(0.0009)	(0.0009)	(0.0011)		(0.0015)	(0.0016)	(0.0047)
Age > 45 × GDP Growth	-	-	-0.0002	0.0006	-	-	-0.1407	-0.0175
			(0.0008)	(0.0009)			(0.1846)	(0.2344)
N	367,074	367,074	367,064	285,922	209,388	209,388	209,388	108,946

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 and sector effects. The first column does not control for age-specific trends or cyclical effects. The second column controls for age-specific trends, and the third column controls for both age-specific trends and cyclical effects by interacting age groups with GDP growth. Column (4) estimates models with age-specific trends and cyclical effects replacing the reform interaction with a post-1994 interaction, where these models are estimated on the sub-sample for the pre-reform period. The sample in Panel A is restricted to the 20-39 age group, while the sample in Panel B 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

	Men				Women			
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					A. Young			
Age < 30 × Reform	0.0346**	0.0419*	0.04**	-	0.0085**	0.00027	0.0028	-
	(0.0156)	(0.017)	(0.0171)		(0.0037)	(0.0048)	(0.0056)	
Age < 30 × Post-1994	-	-	-	-0.0029	-	-	-	-0.014^{+}
				(0.0034)				(0.0082)
Age < 30 × Trend	-	-0.0026	-0.0018	0.0983	-	0.0027*	0.0027**	0.0022
		(0.0018)	(0.0019)	(0.161)		(0.0011)	(0.0012)	(0.0036)
Age < 30 × GDP Growth	-	-	0.356^{+}	0.0127	-	-	-0.0036	-0.1849
			(0.2119)	(0.0128)			(0.213)	(0.2687)
N	70,484	70,484	70,484	47,132	102,844	102,844	102,844	59,618
					B. Older			
Age > 45 × Reform	0.0319	0.0543**	0.0549**	-	-0.0016	0.0097	0.0059	-
	(0.0267)	(0.0238)	(0.0228)		(0.0073)	(0.008)	(0.0103)	
Age > 45 × Post-1994	-	-	-	0.0073	-	-	-	-0.008
				(0.0074)				(0.017)
Age > 45 × Trend	-	-0.0082^{+}	-0.0084*	-0.2934	-	-0.0065*	-0.006*	-0.0054
		(0.0048)	(0.0044)	(0.3287)		(0.0019)	(0.002)	(0.0074)
Age > 45 × GDP Growth	-	-	-0.0914	0.0004	-	-	0.655**	0.3241
			(0.4489)	(0.0162)			(0.29)	(0.5671)
N	44,509	44,509	44,509	27,610	52,431	52,431	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 and sector effects. The first column does not control for age-specific trends or cyclical effects. The second column controls for age-specific trends, and the third column controls for both age-specific trends and cyclical effects by interacting age groups with GDP growth. Column (4) estimates models with age-specific trends and cyclical effects replacing the reform interaction with a post-1994 interaction, where these models are estimated on the sub-sample for the pre-reform period. The sample in Panel A is restricted to the 20-39 age group, while the sample in Panel B 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 Permanent Employment to Non-employment

	Men				Women			
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Young				
Age < 30 × Reform	0.0018*	0.0019^{+}	0.002^{+}	-	0.0003	0.0031	0.003	-
	(0.0007)	(0.0012)	(0.0012)		(0.0024)	(0.0029)	(0.003)	
Age < 30 × Post-1994	-	-	-	-0.002	-	-	-	-0.0046^{+}
				(0.0172)				(0.0029)
Age < 30 × Trend	-	0.0	0.0	0.0003	-	-0.0007*	-0.0007*	0.0005
		(0.0002)	(0.0002)	(0.0004)		(0.0002)	(0.0002)	(0.0006)
Age < 30 × GDP Growth	-	-	-0.0098	0.0171	-	-	0.0161	0.0239
			(0.0124)	(0.0153)			(0.0279)	(0.0268)
N	197,430	197,430	327,082	123,508	178,580	178,580	178,580	119,806
				B. Older				
Age > 45 × Reform	0.0014**	0.0014**	0.0015**	-	-0.0001	-0.001	-0.0011	-
	(0.0007)	(0.0007)	(0.0007)		(0.0014)	(0.0014)	(0.0014)	
Age > 45 × Post-1994	-	-	-	0.0014^{+}	-	-	-	0.0001
				(0.0048)				(0.0026)
Age > 45 × Trend	-	0.0	0.0	0.0	-	0.0	0.0	-0.0004
		(0.0)	(0.0)	(0.0002)		(0.0)	(0.0)	(0.0006)
Age > 45 × GDP Growth	-	-	-0.0222^{+}	-0.0147	-	-	0.0057	-0.0058
			(0.012)	(0.0144)			(0.023)	(0.0233)
N	279,643	279,643	279,643	172,130	168,968	168,968	168,968	99,644

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 and sector effects. The first column does not control for age-specific trends or cyclical effects. The second column controls for age-specific trends, and the third column controls for both age-specific trends and cyclical effects by interacting age groups with GDP growth. Column (4) estimates models with age-specific trends and cyclical effects replacing the reform interaction with a post-1994 interaction, where these models are estimated on the sub-sample for the pre-reform period. The sample in Panel A is restricted to the 20-39 age group, while the sample in Panel B 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 Temporary to Permanent Employment

	Men				Women			
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Reform	-0.0233* (0.0056)	0.0271* (0.0087)	-	-	-0.047* (0.0068)	0.0159** (0.0089)	-	-
Post-1994	-	-	-0.0337* (0.0069)	0.0175+ (0.0103)	-	-	-0.0482* (0.0072)	0.0204** (0.01)
Trend	-	-0.0113* (0.0012)	-	0.0163* (0.0021)	-	-0.0108* (0.0011)	-	-0.0172* (0.0017)
N	176,337	176,337	105,404	105,404	153,471	153,471	99,991	99.991

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 quarter effects, head of household and marital status dummies, education, tenure, and occupation, province and sector effects. Odd numbered columns do not control for a time-trend, while even numbered columns control for time-trends. Columns (1)-(2) and (5)-(6) show the results including a post-reform dummy. Columns (3)-(4) and (7)-(8) show results of regressions with a post-1994 dummy, estimated on the sub-sample before the reform. *Significant at 1% level, **Significant at 5% level, +Significant at 10% level.

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

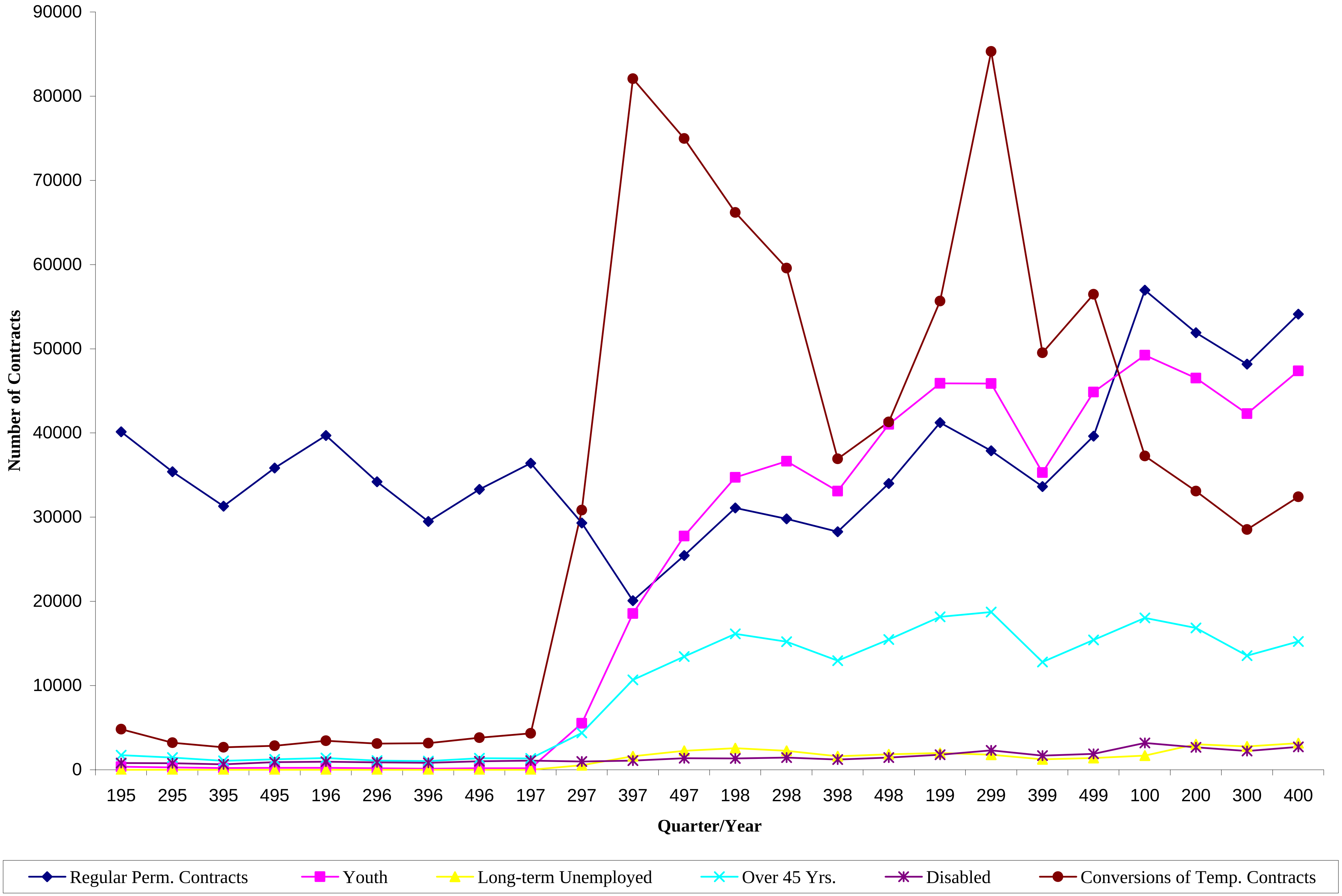

Figure 2: Number of New Permanent Contracts for Women in Population Groups affected by the 1997 Reform

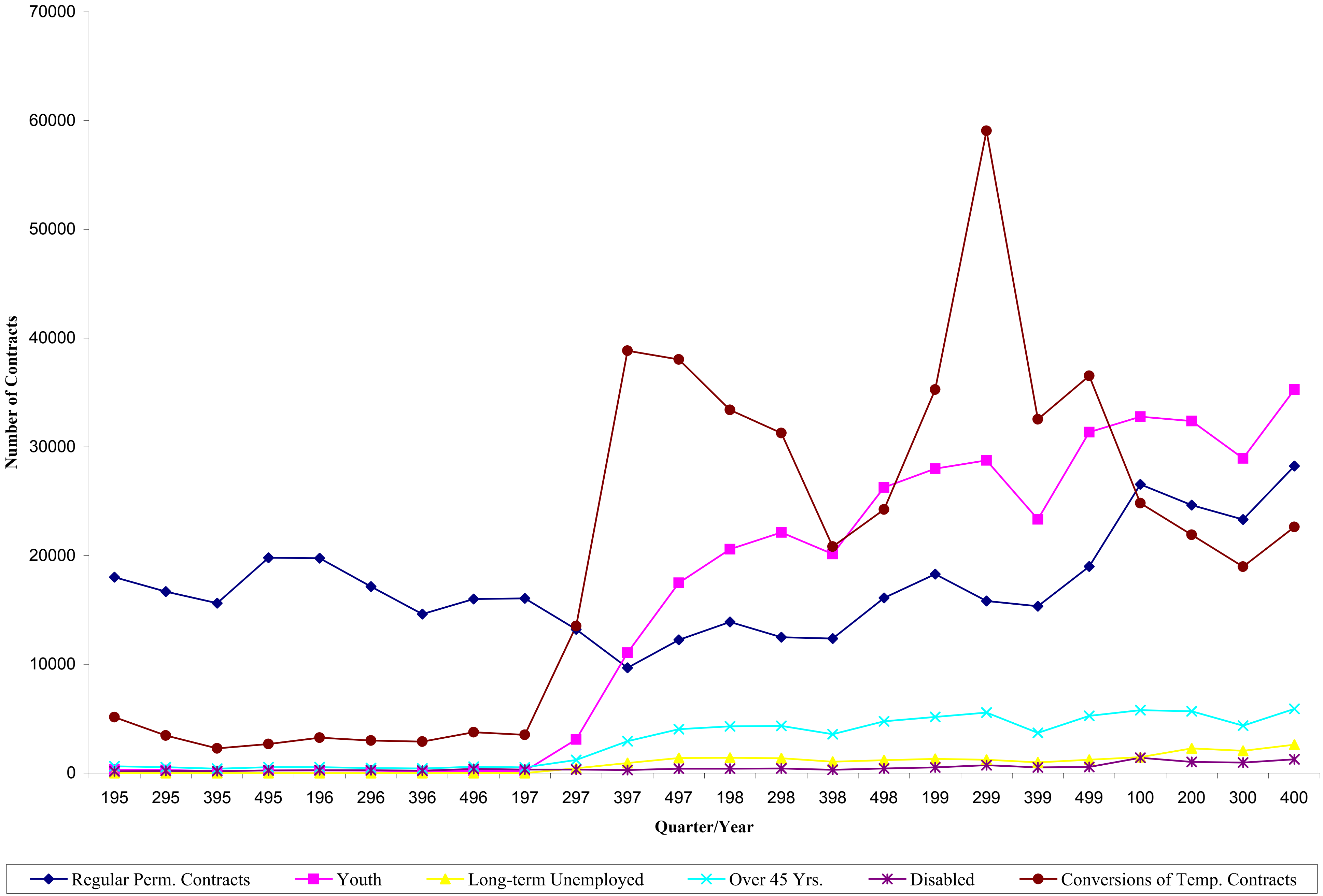

Figure 3: Permanent Employment Probabilities for Men by Age Group (Relative to First Quarter 1997)

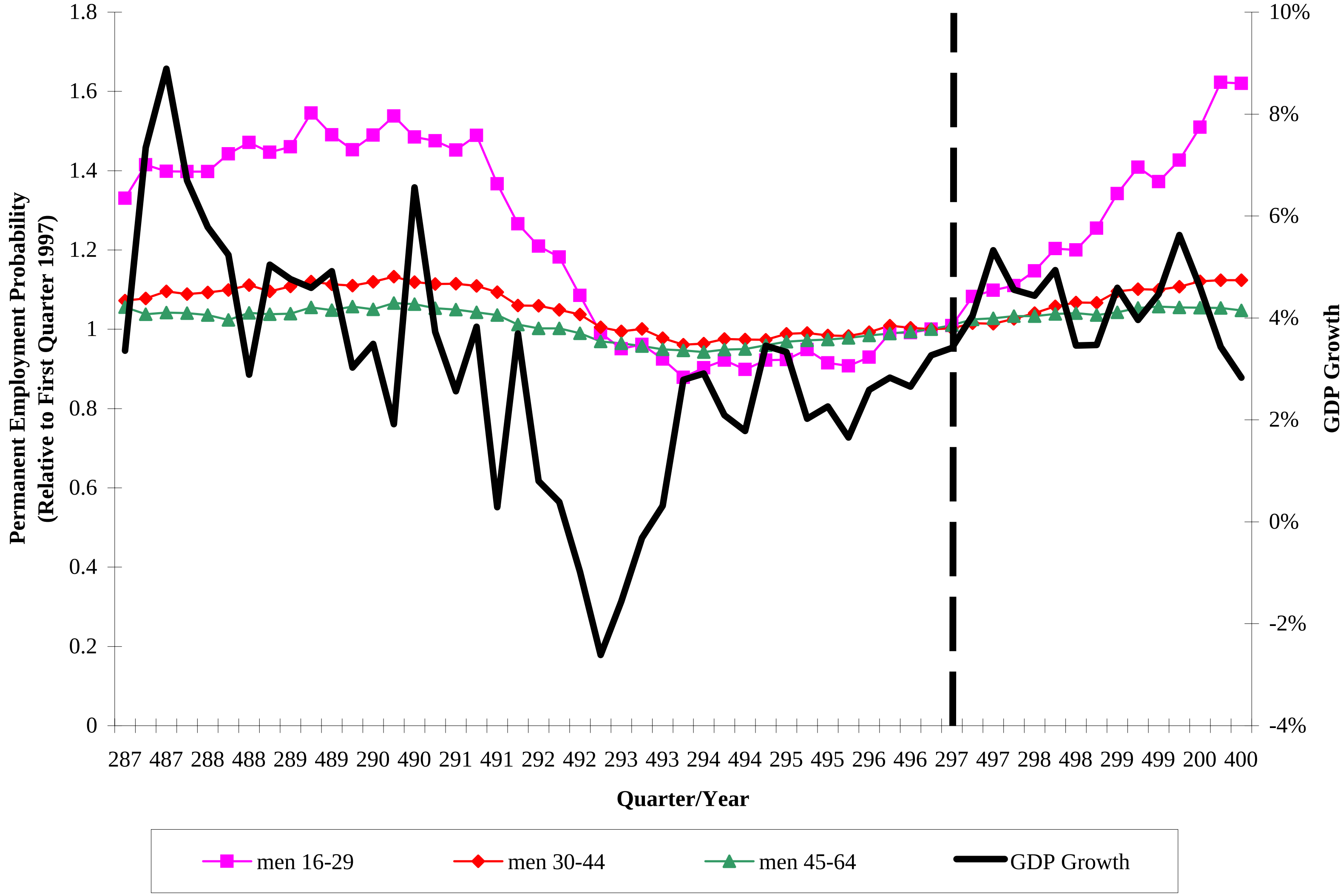

Figure 4: Permanent Employment Probabilities for Women by Age Group (Relative to First Quarter 1997)