

Preliminary and Incomplete

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

Adriana Kugler
Universitat Pompeu Fabra, CEPR and IZA

Juan F. Jimeno
Universidad de Alcalá, FEDEA, CEPR and IZA

Virginia Hernanz
Universidad de Alcalá

July, 2002

* We are grateful to Josh Angrist, Walter Garcia-Fontes, José Garcia Montalvo, David Green, Christian Haefke, Dan Hamermesh, Steve Machin, and to seminar participants at the 2002 SOLE meeting, the 2002 ESPE meeting, and Universitat Pompeu Fabra for comments. We would also like to thank Mario Izquierdo and Climent Quintana i Domeque for able research assistance. Adriana Kugler thanks the Spanish Ministry of Science and Technology through Grant No. SEC-2000-1034 for financial support. *Address for correspondence:* Adriana Kugler, Department of Economics, Universitat Pompeu Fabra, Ramón Trias Fargas, 25-27, 08005 Barcelona, Spain. E-mail: adriana.kugler@econ.upf.es.

Abstract

Temporary employment contracts allowing unrestricted dismissals were introduced in Spain in 1984 and quickly came to account for most new jobs. As a result, temporary employment increased from around 10% in the mid-eighties to more than 30% in the early nineties. 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 use individual data from the Spanish Labor Force Survey to estimate the effects of reduced payroll taxes and dismissal costs on the distribution of employment and worker flows. We exploit the fact that recent reforms apply only to certain demographic groups to set up a natural experiment research design that can be used to study the effects of contract regulations. Our results show that the reduction of payroll taxes and dismissal costs increased the employment of young men on permanent contracts. Results for older men show smaller effects that are not always significant. The results suggest a reasonably elastic response of permanent employment to non-wage labor costs, especially for young workers. We also find positive effects on the transitions from unemployment and temporary employment into permanent employment for young and older workers, although the effects are marginally significant for older workers. On the other hand, transitions from permanent employment to non-employment increased only for older men, suggesting that the reform had little effect on dismissals.

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 (1997) 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), Dolado, Garcia-Serrano and Jimeno (2002), and Bertola and Ichino (1995).

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 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 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.

We use data from the Spanish labor force survey from the second quarter of 1987 to the fourth quarter of 2000 to estimate the impact of the 1997 reform on employment and worker flows. 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 men. Results for older men show smaller effects that are not always significant. The estimates also show increased quarterly transition probabilities from non-employment and temporary employment to permanent employment for all groups, although the effects for older men are marginally significant. 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. Our results are robust to the inclusion of quarter effects, sector-and province-specific time trends, and to controls for age-specific cyclical effects. An

implication of these findings is that costly permanent contracts and high payroll taxes inhibit 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 explains the natural experiment research design used to evaluate the impact of the 1997 reform. Section IV describes the data and presents estimates of the effects of the reform on employment levels, accessions, conversions, and separations. 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 restricting dismissals. 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 “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 out of the salary per year of seniority, with a maximum of 12 months. In the case of unfair dismissals, firms had to pay 45 days per year of seniority out of the salary, with a maximum

of 42 months. Severance payments for “economic reasons” were the same as for fair dismissals under “personal reasons.” In practice, these rules 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 was terminated before its term. In particular, temporary workers were entitled to 12 days per year of seniority based on the salary and could not be appealed in labor courts. Moreover, employers could use temporary contracts to avoid severance payments altogether when separation occurred at the agreed date.

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.

² See Güell-Rotllan and Petrongolo (2000).

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, 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

³ In the case of workers over 45 years of age, temporary contracts could be continued to be used for all types of jobs and not only for seasonal 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.

from 45 to 33 days out of the salary per year of seniority and the maximum was reduced from 42 to 24 months out of the salary. 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 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.

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 Figure 1, which plots the total number of newly signed permanent contracts and conversions of temporary into permanent contracts for men. The figure shows that the number of newly signed permanent contracts increased sharply for young and older workers after the second quarter of 1997, but remained roughly constant for the long-term unemployed and disabled workers. On the other

⁴ Payroll taxes are generally high in all Continental Europe (with Denmark being an exception) and have often been pointed to as an explanation for high unemployment in Europe. Laroque and Salanie (2002) and Kramarz and Philippon (1999) study the consequences of high payroll taxes in France.

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

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 men. The figure also shows a marked rise in the number of conversions of temporary into permanent contracts after the second quarter of 1997.

III. Identification Strategy

Our goal in this paper is to identify the impact of the reduced dismissal costs and payroll taxes 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. 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 Figure 1 above, the greatest impact of the reform appears to have been on the two affected age groups.

The identification strategy is illustrated in Figure 2, which plots permanent employment probabilities for men by age group relative to the base period, first quarter of 1997, for the same years as Figure 1 (i.e., 1995-2000).⁶ The figure shows 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, Figure 3 plots the permanent employment probabilities 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, this figure

⁶ These give the probabilities of being employed with permanent contracts relative to non-employment.

shows the increase in permanent employment probabilities for the young after the second quarter of 1997, but it also shows higher permanent employment probabilities for the young during the expansion of the late 1980's. The figure highlights the importance of proper control for cyclical effects, especially because the young appear to benefit disproportionately during expansions. On the other hand, the figure shows 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 use a triple differences estimator which compares the employment of treated and control individuals during the reform period with the employment of treated and control individuals during an earlier expansionary period. This triple differences estimator uses the period without reform to check for the possibility that expansions have differential effects on younger and older workers.⁷ In addition, the triple differences strategy is implemented in samples limited to narrower age groups, concentrated around the affected age groups. For example, the sample for the young is restricted to the 25-35 age group. Since the 25-30 age group and the 30-35 age group are likely to face similar age-specific cyclical effects, restricting the sample in this way is an important robustness check.

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

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

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

⁷ 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.

t, including quarter dummies and, in some specifications, province- and sector-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. The province- and sector-specific trends control for factors affecting employment differentially in different provinces and sectors over time, including EU active labor market programs introduced in some Spanish regions and skilled-biased technical change.⁸ 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 dummy, E_t , which equals 1 in 1987-90 and 1996-2000 and zero otherwise. That is, the estimating equation is modified to be

$$\Pr[e_{it}=1 | X_{it}, d_i] = \Lambda[\alpha_t + \beta' d_i + \gamma' X_{it} + \delta_E'(d_i \times E_t) + \delta_R'(d_i \times E_t \times R_t)]. \quad (2)$$

Here, the impact of the reform is captured by the third-order term, δ_R , which measures the reform impact relative to the pre-treatment expansion. The age-specific cyclical effect is captured by the expansion interaction, 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 (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

⁸ We include interactions of province and sector dummies with a time trend because both active labor market programs and technical change increased during the 1990's. However, in contrast to the sharp timing of the 1997 reform which was introduced after the second quarter of 1997, the timing of EU active labor market programs and especially skilled-biased technical change cannot be identified precisely.

age-specific cyclical effects by allowing differential transition probabilities for treated groups during the expansions of the late 1980's and 1990's.

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 2000.⁹ 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. 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. We also restrict the sample to men between 21 and 59 years of age to focus on workers with strong labor market attachment.

The LFS has a rotating panel structure that follows individuals for a maximum of sixth 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 for similar data sets in other countries.¹⁰ To identify transitions, we match individual records from one quarter to the next

⁹ 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 and 3 they do not appear to have affected estimates of individual employment probabilities for those in this age group.

¹⁰ Acemoglu and Angrist (2001) report an attrition rate of around 29% in the CPS.

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 for men by age group for the periods before and after the reform. The table shows lower permanent employment probabilities for middle-aged and older workers, but higher permanent employment probabilities for younger workers after the reform became effective. Simple comparisons of means show lower transitions during the post-reform period, although this probably reflects the fact that the pre-reform period includes the strong expansion of the late 1980's. As shown in the regressions below, controlling for year effects and other covariates shows a different picture. Men are also older, more educated, less likely to be married and to be the head of household, and have shorter tenures during the reform period.

B. Employment Effects

Table 3 reports logit marginal effects estimated using equations (1) and (2). 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 household and marital status dummies, four schooling groups, tenure, seven occupation groups, 10 sector groups, 15 province main effects, year effects, 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. The marginal effects of these interactions capture the change in permanent

employment probabilities of younger and older relative to middle-aged men during the reform years. The results show a large and statistically significant increase in permanent employment probabilities for young and older relative to middle-aged workers after the 1997 reform became effective. For example, Column (1) shows that the probability of permanent employment increased by 0.0393 for younger men and by 0.0157 for older men relative to middle-aged men during the reform years. Columns (2) and (3) report the results for specifications which control for sector- and province-specific trends. The results decrease somewhat but remain large and significant. These results indicate that the probability of permanent employment increased by around 0.025 for young men and by 0.012 for older men relative to middle-aged men during the reform years.

Column (4) 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 that while expansions do seem to disproportionately benefit younger men, they do not benefit older men relative to middle-aged men. Moreover, the results show that, after controlling for the beneficial effects of expansions on younger workers, the probability of permanent employment increased by close to 0.018 for younger and by close to 0.011 for older workers. These results suggest a percent increase in permanent employment probabilities of 3.01% for the young and 1.26% for the old. Nonetheless, since the expansion of the late 1980's was stronger in terms of GDP growth, our control for age-specific cyclical effects probably provides a lower bound of the effect of the reform. The next two columns limit the sample to narrower age groups to further control for age-specific cyclical effects. Column (5) uses the 25-30 age group as the treated and the 30-35 age group as the control for young workers, while Column (6) uses the 45-55 age group as the treated and the 40-45 age group as the

control for older workers. The results in Column (5) are very similar to the results above. Permanent employment probabilities of young workers increase by 0.017 (i.e., 2.8%) relative to middle-aged workers during the reform years. On the other hand, the effect of the reform on older workers becomes insignificant when using the triple differences specification on the restricted sample of older workers.

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 transitioned 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.¹¹ Table 4 shows increases in transitions from non-employment to permanent employment for young and older relative to middle-aged men after the 1997 reform became effective. For example, Column (1) shows an increase in the relative transition probabilities from non-employment to permanent employment of 0.0048 or 30.5% for younger men and of 0.0036 or 10.62% for older men during the reform years. Controlling for sector- and province-specific trends in Columns (2) and (3) does not change the results for the group of young workers and slightly decreases the magnitude of the effect for the older group.

The rest of the columns in Table 4 report results which control for age-specific cyclical effects. Column (4) shows no differential effect on the probability of transitioning from non-employment to permanent employment for young men during the expansion of the late 1980's, but a negative effect for older men. The transition from non-employment to permanent employment falls slightly to 0.004 or 25.5% for younger workers, but increases to

0.0059 or 17.4% for older workers. Results on the restricted sample of younger men in Column (5) now show no significant effect of the reform on transitions from non-employment to permanent employment. On the contrary, results on the restricted sample of older men in Column (6) now show a large and significant effect of 0.0068 or 20% of the reform on the transitions from non-employment to permanent employment.

Table 5 reports logit marginal effects from models for transitions from temporary to permanent employment.¹² The results 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 0.016 or 19.7%, while the results which control for age-specific cyclical effects indicate an increase of between 0.0103 and 0.0145 (or between 12.4% and 17.5%). On the contrary, the results show no significant change in the transitions from temporary to permanent employment for older men.

Table 6 reports logit marginal effects from models for transitions from permanent employment to non-employment.¹³ There is no change in the transition from permanent employment to non-employment for young workers during the reform years, which explains why increased flows from non-employment and temporary to permanent employment suggested by Tables 4 and 5 translate into a net positive effect on permanent employment. On the contrary, there is a rise in the transition from permanent employment to non-employment for older relative to middle-aged men during the reform years of about 15.4%, with and without controlling for age-specific cyclical effects. The results are slightly smaller when we

¹¹ The controls are as in the permanent employment probability specifications.

¹² The controls here include head of household and marital status dummies, four education groups, two age groups, and province, quarter and year dummies.

restrict the treated group of older workers to those under 55 to avoid capturing the impact of early retirement programs. The results using the restricted sample imply an increase of 14.8% in the transitions from permanent employment to non-employment for older relative to middle-aged men during the reform years. In the case of older men, the increased flows from non-employment to permanent employment shown in Table 4 and the increased flows from permanent employment to non-employment shown in this table appear to cancel out, explaining the weak net effect on permanent employment.

D. Economic Interpretation of Magnitudes

Estimates of the net effect on young and older men in Table 3 can be compared to the effect of the 1997 reform on the costs of employing young and older workers to estimate elasticities of employment with respect to non-wage labor costs. The 1997 reform reduced dismissal costs from 45 to 33 days per year worked or, equivalently, a reduction of 26.7% for both young and older workers. In contrast, the reform reduced the uniform payroll tax rate of 28.3% of the salary by different amounts for different age groups. The payroll tax reduction for young workers was of 40% for contracts signed in 1997 and 1998 during the first two years of the contract, and of 35% and 25% for contracts signed after 1999 during the first and second years of the contract, respectively. The payroll tax reduction for older workers was of 60% for contracts signed in 1997 and 1998 during the first two years of the contract and of 50% thereafter. After 1999, the payroll tax reduction for older workers was of 45% and 40% for 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.

¹³ The controls are the same as in Table 5.

Expected quarterly dismissal costs for young workers are equal to the probability of dismissal times the estimated dismissal costs. While we do not have the probability of a dismissal, Table 2 reports separation probabilities by age (i.e., 2.75% for young men and 1.76% for older men). Dismissal costs can be estimated based on the following formula:

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

where we multiply by 4 to obtain a quarterly dismissal cost. Mean salaries from the Survey of Salary Structure for 1995 indicate a yearly salary of 10,680 Euros and 20,892 Euros for young and older men, respectively. From the LFS we get mean tenures for young and older men of 2.16 and 17.2 years in 1995. Combining these numbers, we get quarterly expected dismissal costs of 222.6 Euros and 3,467.7 Euros for young and older workers, respectively.¹⁴

Payroll tax costs are easier to obtain. The payroll tax rate is 28.3%, implying a quarterly payroll tax cost of 755.6 Euros and 1,478.1 Euros for young and older men, respectively. Consequently, dismissal costs account for 8.5% and 32.1% of labor costs for young and older men, respectively. Payroll taxes account for 20.2% and 15% of labor costs for young and 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 reductions of 40% and 60% for young and older workers, the percent reductions in total labor costs implied by the reform were of 10.34% and of 17.5%, respectively. Using the smaller payroll tax reductions of 30% and 50% for young and older workers applied during the second year of the contract, the percent reductions in total labor costs implied by the reform were of 8.32% and 16.05%,

¹⁴ This means that we do not have to consider the change in the maximum payment of dismissal costs from 42 to 24 months, since it is never binding.

respectively. Of the total labor cost reduction induced by the reform, 78% and 51% can be attributed to the larger payroll tax reduction and 72% and 47% to the smaller payroll tax reduction for young and older men, respectively. This means that while payroll tax reductions were larger in absolute terms for older workers, the payroll tax reductions were relatively more important for younger workers.

The results in Table 3 that do not control for age-specific effects imply an increase in permanent employment probabilities of 0.0393 or 6.4% for young men and of 0.0157 or 1.9% for older men during the reform period. These results imply elasticities of between -0.62 and -0.77 for young men using payroll tax reductions of 40% and 30%, respectively. The equivalent elasticities for older men are much lower (i.e., between -0.11 and -0.12 using payroll tax reductions of 60% and 50%).

Results in Table 3 that control for age-specific cyclical effects indicate smaller effects. The results suggest the reform increased permanent employment probabilities by 0.0184 or 3.01% for young men and by 0.0106 or 1.26% for older men. These results imply elasticities of between -0.29 and -0.36 for young men using payroll tax reductions of 40% and 30%, respectively, and of -0.07 and -0.08 for older workers using payroll tax reductions of 60% and 50%, respectively. The results suggest a fairly elastic employment response of young workers to changes in non-wage labor costs, but an inelastic response of older workers.¹⁵

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 reform of

¹⁵ Katz (1998) and Nickell and Bell (1996) discuss the possibility that high labor costs increase unemployment rates for disadvantaged workers, including youth.

1997 to set up a research design based on the fact that the reform applied differently to different age groups. Estimates using the Spanish Labor Force Survey suggest that the reform increased permanent employment probabilities for young relative to middle-aged men. The results for young men are insensitive to the way we correct for macro shocks, i.e., whether we correct for common macro shocks for all age groups, for sector- and province-specific trends, or for age-specific cyclical effects. The results also show increases in the relative transitions from non-employment to permanent employment for older men and from temporary to permanent employment for young men during the reform period. On the other hand, relative transitions from permanent employment to non-employment decline for older men but not for young men.

Our results suggest that the reduction in dismissal costs and payroll taxes increased both hiring and dismissals for older men, but had a positive effect on the hiring margin of young men with little effect on dismissals. This explains why the reform seems to have had a positive net effect on permanent employment for young men but not for older men.

The estimated elasticities suggest a fairly elastic response of permanent employment to non-wage labor costs, especially for younger workers for whom the payroll tax reduction was relatively more important. Further institutional reform along the lines of the 1997 legislation seems at least as likely to increase employment levels as reforms promoting the use of temporary contracts. On balance, the results reported here support the view, widely discussed though not previously substantiated, that the high non-wage labor costs and lack of flexibility associated with permanent contracts have reduced employment levels in Spain.

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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	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	None	None
Young unemployed workers (under 30 years of age)	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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)	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 45 days' wages per year of seniority with a maximum of 42 months' wages	<u>Fair dismissals</u> : 20 days' wages per year of seniority with a maximum of 12 months' wages <u>Unfair dismissals</u> : 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

Variable	Age 21-29		Age 30-44		Age 45-59	
	Pre-Reform	Post-Reform	Pre-Reform	Post-Reform	Pre-Reform	Post-Reform
Permanent Employment Probability	0.6114	0.6304	0.8365	0.8358	0.8389	0.7869
Non-employment to Permanent Employment Transition Probability	0.0157	0.0071	0.0431	0.0272	0.0339	0.0157
Temporary to Permanent Employment Transition Probability	0.0828	0.0579	0.1027	0.0513	0.0979	0.042
Permanent Employment to Non-employment Transition Probability	0.0275	0.0191	0.0134	0.0084	0.0176	0.0126
Age	25.31 (2.13)	25.38 (2.49)	36.23 (4.33)	37.20 (4.25)	50.76 (4.26)	51.51 (4.18)
Tenure (in months)	37.17 (40.04)	32.68 (34.6)	117.52 (87.24)	113.97 (87.44)	208.93 (130.29)	207.49 (134.39)
% Head of Household	28.15	16.35	80.57	74.18	93.6	90.7
% Married	31.1	16.6	82.68	77.16	91.73	90.2
% No Education	1.82	1.03	4.41	2.06	14.91	9.17
% Primary Education	37.32	17.94	47.55	26.49	57.59	46.88
% Secondary Education	36.79	45.35	24.1	36.76	11.3	20.11
% Technical Education	17.64	24.22	13.25	16.85	7.45	8.2
% University Education	6.44	11.44	10.68	17.83	8.75	15.63
N	128,500	40,139	310,439	96,286	246,798	87,630

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

Table 3: Permanent Employment Probabilities

Regressors	Full Sample				Restricted Age Groups	
	(1)	(2)	(3)	(4)	(5)	(6)
Age < 30	-0.1402* (0.0143)	-0.0537* (0.0028)	-0.0542* (0.0028)	-0.0679* (0.039)	0.0518* (0.0102)	-
Age >= 45	-0.109* (0.0234)	-0.2321* (0.0012)	-0.2324* (0.0012)	-0.2377* (0.0019)	-	-0.0813 (0.0061)
Age < 30 x Reform	0.0393* (0.0073)	0.0255* (0.0058)	0.0251* (0.0058)	0.0184* (0.0057)	0.0171** (0.0091)	-
Age >= 45 x Reform	0.0157* (0.0063)	0.0117** (0.0058)	0.0119** (0.0058)	0.0106 ⁺ (0.0058)	-	0.0125 (0.0083)
Age < 30 x Expansion	-	-	-	0.0222* (0.0065)	0.0004 (0.0091)	-
Age >= 45 x Expansion	-	-	-	0.0085 (0.0068)	-	-0.0078 (0.0083)
Sector Trends	NO	YES	NO	NO	NO	NO
Province Trends	NO	NO	YES	YES	YES	YES
Log-likelihood	-325,407	-291,012	-290,819	-290,803	-71,964.3	-76,054.2
N	722,166	715,520	715,520	715,520	193,215	268,171

Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by individual. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, education, tenure, and occupation, sector, and province dummies. The first four 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 (5) is restricted to the 25-35 age group and the sample in Column (6) is restricted to the 40-55 age group. * Significant at 1% level, ** Significant at 5% level, ⁺Significant at 10% level.

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

Regressors	Full Sample				Restricted Age Groups	
	(1)	(2)	(3)	(4)	(5)	(6)
Age < 30	-0.0006 (0.0011)	-0.0004 (0.0011)	-0.0004 (0.0011)	-0.0007 (0.0016)	0.0 (0.0021)	-
Age >= 45	-0.0145* (0.0005)	-0.014* (0.0006)	-0.0139* (0.0006)	-0.0089* (0.0011)	-	-0.0022 (0.0023)
Age < 30 x Reform	0.0048* (0.0022)	0.0048* (0.0022)	0.0048* (0.0022)	0.004** (0.0022)	0.0025 (0.0023)	-
Age >= 45 x Reform	0.0036+ (0.0022)	0.0033+ (0.0022)	0.0032+ (0.0022)	0.0059* (0.0026)	-	0.0068* (0.0033)
Age < 30 x Expansion	-	-	-	0.0006 (0.002)	-0.0006 (0.0023)	-
Age >= 45 x Expansion	-	-	-	-0.0079* (0.0014)	-	-0.0099* (0.0019)
Sector Trends	NO	YES	NO	NO	NO	NO
Province Trends	NO	NO	YES	YES	YES	YES
Log-likelihood	-13,618.6	-13,401.4	-13,390.4	-13,380.3	-6,802.5	-5,331.3
N	142,852	140,332	140,332	140,332	66,898	63,869

Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by individual. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, education, tenure, and occupation, sector, and province dummies. The first four 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 (5) is restricted to the 25-35 age group and the sample in Column (6) is restricted to the 40-55 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		Restricted Age Groups	
	(1)	(2)	(3)	(4)
Age < 30	-0.0111* (0.0012)	-0.0189* (0.0018)	-0.0142* (0.0023)	-
Age >= 45	-0.0013 (0.0015)	0.0025 (0.0031)	-	0.0013 (0.0043)
Age < 30 x Reform	0.0163* (0.0032)	0.0145* (0.0032)	0.0103* (0.0032)	-
Age >= 45 x Reform	-0.001 (0.0038)	0.0007 (0.0039)	-	0.0078 (0.0055)
Age < 30 x Expansion	-	0.0099* (0.003)	0.0314 (0.0033)	-
Age >= 45 x Expansion	-	-0.0048 (0.0031)	-	0.0013 (0.0048)
Log-likelihood	-69,942.3	-69,928.3	-48,796.5	-16,821.6
N	262,058	262,058	182,416	62,449

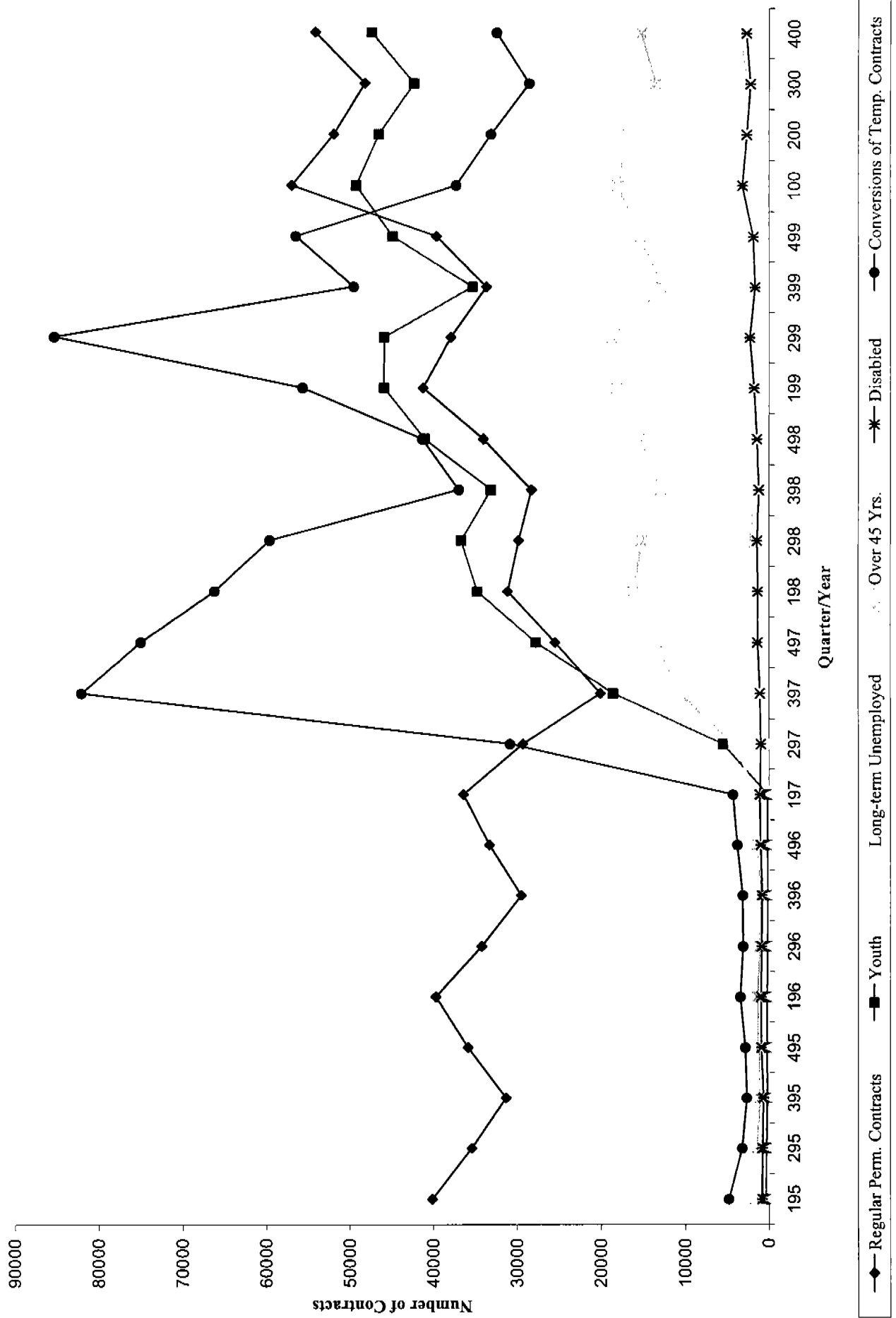
Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by individual. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, and education. The first four columns use the entire 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 (3) is restricted to the 25-35 age group and the sample in Column (4) is restricted to the 40-55 age group. * Significant at 1% level.

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

Regressors	Full Sample		Restricted Age Groups	
	(1)	(2)	(3)	(4)
Age < 30	0.0142* (0.0036)	0.0145* (0.0021)	0.0112* (0.0022)	-
Age >= 45	0.0106* (0.0005)	0.0106* (0.0014)	-	0.013* (0.0023)
Age < 30 x Reform	-0.0004** (0.0002)	-0.0003 (0.0013)	-0.0006 (0.0016)	-
Age >= 45 x Reform	0.0027* (0.0001)	0.0027* (0.0011)	-	0.0026** (0.0013)
Age < 30 x Expansion	-	-0.0004 (0.0011)	-0.0006 (0.0014)	-
Age >= 45 x Expansion	-	0.0 (0.0009)	-	0.0 (0.0014)
Log-likelihood	-66,476.6	-66,476.5	-23,473.3	-39,130.2
N	716,588	716,588	240,537	409,929

Note: The table reports logit marginal effects. The robust standard errors reported in parenthesis allow for clustering by individual. The logit controls for age and year main effects, quarter effects, head of household and marital status dummies, and education. The first four 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 (3) is restricted to the 25-35 age group and the sample in Column (4) is restricted to the 40-55 age group. * Significant at 1% level, **Significant at 5% level.

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



**Figure 2: Permanent Employment Probabilities for Men by Age Group
Normalized to First Quarter of 1997**

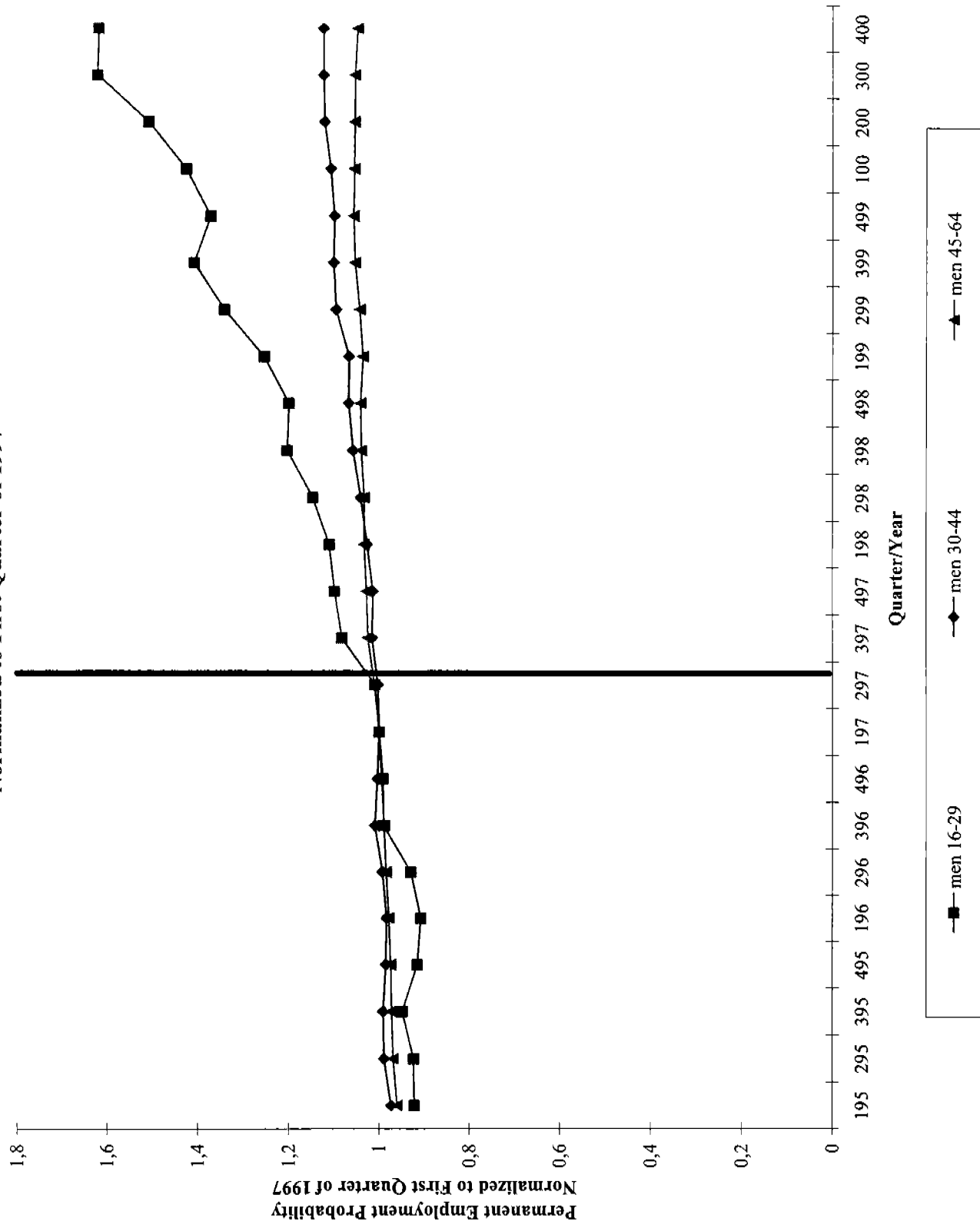


Figure 3: Permanent Employment Probabilities for Men by Age Group Normalized by First Quarter of 1997

