Labor Demand Elasticities Over the Life Cycle: Evidence from Spain's Payroll Tax Reforms

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Abstract

This paper estimates the employment and wage effects of payroll tax credits at different moments of the life cycle. In 1997, Spain reduced payroll taxes for new hires younger than 30 and older than 45. Time variation and age discontinuities allow me to perform both a difference-in-difference analysis and a regression discontinuity design. Using administrative data, I find that employment at age 30 increased by 2.42%. Moreover, I show that the gains do not come at the expense of non-subsidized workers, indicating that the policy led to net job creation. Wages of new hires are not affected by the reform. In contrast, the tax cut at 45 had no effect on employment or wages. For prime-age workers, the lower payroll taxes can be interpreted as a transfer from taxpayers to firms. Combining the above estimates and standard tax incidence formulas, I obtain a lower bound labor demand elasticity of -0.63 at age 30 and zero for workers who are 45 years old. An analysis of wage densities and other observable characteristics supports the conjecture that the elasticity decreases with age because the quality of available workers decreases with age. I consider several alternative explanations for the results, but none of them are consistent with the evidence. A cost-benefit analysis shows that payroll tax receipts would increase if the tax rate for workers under 30 was reduced. The results at age 45 suggest low efficiency costs of payroll taxes for prime-age workers. Finally, I discuss implications for payroll tax reforms, welfare-to-work schemes, and job-search assistance.

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1 Introduction

Labor demand and labor supply elasticities are a central parameter for the design of tax systems and welfare programs. In recent decades, major policy developments focused on encouraging labor supply by making earnings subsidies conditional on work. Accordingly, much attention has been devoted to measuring supply responses for men, women, and young and older workers (Blundell and MaCurdy, 1999; Moffit, 2002). However, the employment and wage effects of these policies also depend on labor demand. For instance, the more inelastic demand is, the less welfare-to-work programs will increase employment and earnings. Despite that, less attention has been devoted to estimating labor demand elasticities at different points of workers careers.[1]

In this paper, I exploit payroll tax cuts in Spain that affected workers younger than 30 and older than 45 to estimate labor demand elasticities at different ages. The Spanish context is interesting for evaluation of active labor market policies given its high and persistent level of unemployment (Figure 1).[2] Despite the relevance of understanding the effects of labor policies on employment in dysfunctional labor markets, there is little evidence from such settings. Most evaluations have focused on countries without sustained levels of high unemployment. (Card et al., 2010).[3]

The central empirical fact established in this paper is that labor demand elasticities vary over the life cycle. Reduced form estimates show an increase of employment of 2.42% at the age of 30, and a zero effect at 45. For both groups, wages are not affected by the reform. Combining the employment and wage estimates with standard tax incidence formulas, I can recover a lower bound for the structural labor demand elasticity at each age. I measure a labor demand elasticity of -0.63 for 30 year old workers, and a perfectly inelastic labor demand, or zero, for workers around the age of 45.[4] I consider several explanations for the different elasticities. The evidence is consistent with the quality of available workers decreasing with age, such that at some point between 30 and 45 marginal workers might start facing a perfectly inelastic labor demand.

The Spanish context offers two main advantages for the study of demand elasticities over the life cycle. First, it is difficult to find a quasi-experiment that happens during the same macroeconomic

[1] For evidence for young workers see Katz (1998) and Egebark and Kaunitz (2014). Huttunen et al. (2013) provide evidence for low-wage workers older than 54. Conclusions regarding the value of labor demand elasticities over the life cycle are complicated given the different contexts studied in each paper. For a review of the earlier literature, see Hamermesh (1993).

[2] In the mid-nineties, unemployment reached 25%. During the Great Recession, unemployment has been even higher. The lowest level of unemployment during the last three decades was around 10%, a number that would be considered high in most OECD countries.

[3] For instance, Card et al. (2010) review the effects of active labor market policies in 26 countries. Only 3 of them (Dominican Republic, Slovakia, and Poland) featured levels of unemployment similar to those in Spain for some years between 1990 and 2014. The majority of the countries studied had unemployment rates between 5 and 10% between 1990 and 2014.

[4] In the paper, I call workers aged 25-30 young workers, and those with ages around 45 prime-age workers.

context, within the same set of institutions, and with the same policy *but* at different ages. Second, I use a rich administrative dataset that contains employment and wage records of over one million individuals throughout their labor lives. Access to wage data is crucial to estimate the reduction in labor costs generated by the policy. Moreover, the quality of the data allows me to analyze potential substitution effects in "non-treated" groups (Davidson and Woodbury, 1993; Crepon et al., 2013). This is important if one wants to understand whether the demand responses reflect net job creation or not. In addition, I can measure the extent to which lower payroll taxes are subsidizing employment that would have existed regardless of the policy change (Katz, 1998; Becker, 2011), a key estimate for a cost-efficiency analysis.[5]

In May 1997, the Spanish government enacted a reform that allowed firms to claim payroll tax credits only when hiring workers as *new permanent employees*.[6] The program featured age discontinuities that specifically targeted workers younger than 30 years old and older than 45 years old. A tax credit for the long-term unemployed, regardless of their age, was also approved. In March 2001, the employment credit for young workers was removed, providing an additional natural experiment. I estimate the impacts of payroll tax credits on employment, job transitions, and wages using two empirical strategies: first, a difference-in-difference (DD) for each policy change; second, a regression discontinuity design (RDD) based on the age thresholds.

I begin by showing how firms respond to employment credits for workers that are older than 45. The age distribution of workers under permanent contracts has a hole or missing mass between the ages of 44-45. These "missing" permanent workers are hoarded in temporary contracts: the age distribution of short-term employees has an excessive mass at the same ages. Once workers reach their 45th birthday, firms convert them to permanent employees. The RDD estimates show that the decrease in temporary workers offsets the increase in permanent ones: firms are just arbitraging between subsidized and non-subsidized contracts, without further effects on prime-age employment. In other words, all subsidized contracts after 45 would have existed absent the policy. Moreover, there is no evidence of workers capturing the tax credit in higher wages; instead, the tax credit acts as a transfer from taxpayers to firms, a finding that is consistent with employers holding all bargaining power. Accordingly, the tax cut is very costly for the government (4.4 billion euros of lost revenue). The lack of an employment effect suggests low efficiency costs of payroll taxes for prime-age workers.[7]

[5]The literature calls this effect "windfalls" (Katz, 1998). I will use this terminology throughout the paper. Windfalls can also be understood as the effects of the policy for inframarginal workers.

[6]The tax credits were not available for new temporary hires, the other main type of contract in Spain. The main difference between permanent and temporary contracts is that the former does not have an agreed expiration date and are subject to severance payments in case of dismissal. One of the objectives of the government was to reduce the fraction of temporary workers since they are generally regarded as bad jobs. See section 2.2 for more details on the characteristics of permanent and temporary workers.

[7]Saez et al. (2012) reach a similar conclusion for a payroll tax reform that targeted high-wage, prime-age workers

I then turn to the effects on younger workers. Immediately after the 1997 policy change, both permanent and temporary employment increase. Temporary contracts are not subsidized, but they are also positively affected by the reform because firms are using them to screen workers and make permanent those who perform better. Accordingly, transitions from temporary to permanent jobs within the same firm double. Overall, employment of young workers increases by 0.84% relative to its pre-1997 level in the DD estimate. Importantly for the cost-efficiency of the policy, half of the employment effect comes through a reduction in unemployment insurance (UI) recipients.[8] Wages are not affected by the reform. The RDD results confirm all the DD findings, but the estimates measure a 2.42% increase in employment. This number is significantly higher than the DD result. Since the RDD is not based on the effects immediately after the policy change, it can be interpreted as a long-run treatment effect. It suggests that the short-run results underestimate the effects of the policy in the long-run due to adjustment costs (Chetty et al., 2011; Chetty, 2012; Kleven and Waseem, 2013). I estimate that 7.6 out of 10 subsidized jobs created would have existed in any case in 1997. This goes down to 4.6 out of 10 in 2001 due to new limitations in the use of employment credits.[9] Despite these windfalls, the program for young workers is very cost-efficient because it decreases the number of UI recipients. I estimate an increase in net revenue of 1.4 billion euros. The marginal efficiency cost of funds (MECF) is -0.52, indicating that the employer's payroll tax rate for workers under 30 is on the declining part of the Laffer curve.

The key threat to interpreting the effect on young workers as net job creation is that the estimates might be confounding positive and negative employment effects across the threshold. I show evidence that the estimates indeed represent net job creation. First, between 1997-2001 the age distribution of hires features a jump at 30 years, and the removal of the threshold in March 2001 allows me to see how the jump disappears. If substitution had been ocurring, we expect to observe that hiring above 30 jumps up. However, both visual inspection and regression results show that the jump disappears *only* because hiring below 30 converges to the level of hiring above 30, and that the latter stays constant. Evidence from separations does not show any significant change across the threshold. Second, this result might be specific to the change in 2001 and not reflect substitution for the whole period when the policy was in place. I construct a counterfactual based on data far away from the discontinuity (Saez, 2010; Kopczuk and Munroe, Forthcoming) and compare it to the actual hiring distribution between 1997-2001. The assumption is that workers close to the thresholds should suffer more from displacement because they are closer substitutes. While this strategy detects a significant missing mass before 45, it does not for workers after 30.[10]

around the age of 38.

[8] Results exploiting the 2001 removal of the credit for young employees are analogous.

[9] Specifically, the new regulations restricted firms' ability to fire subsidized workers and then use such contractual arrangements again with new workers.

[10] I perform three additional strategies to show that substitution is not a concern. All of them provide evidence inconsistent with substitution. See section 3.3 for further details.

The results are consistent with adverse selection of marginal workers increasing with age. The most important increases in the employment rate happen before the age of 30. Firms might infer that workers who have not joined the labor force by then might be of lower ability. That signal will strengthen with age, as these individuals do not obtain basic working skills. Subsidized prime-age workers should therefore come from a pool of much lower ability workers, while this negative selection should be weaker for young workers. Analysis of the wage densities confirms that. The wage density of young subsidized workers is only slightly shifted to the left compared to their non-subsidized age peers. For prime-age workers, the wage density of subsidized workers is much more concentrated on the lower end than that of non-subsidized workers. In addition, adverse selection should also be detectable in other observable characteristics. Regression results show that both younger and prime-age subsidized workers are less likely to have finished college and have accumulated less work experience during the last 12 months. More importantly, this negative gap between subsidized and non-subsidized workers is significantly larger in magnitude for prime-age workers relative to young workers.

I show that several alternative explanations for the results are inconsistent with the evidence. First, I confirm that the null employment effect for prime-age workers is not caused by inelastic labor supply. Second, I consider whether the different elasticities could be explained by young and prime-age workers being in different firms. Third, I show that different levels of bargaining power or hiring costs at each age cannot explain the results. Fourth, pass-through of payroll taxes on wages is not consistent with the findings.

The fundamental characteristics of the labor market that point to a decreasing labor demand elasticity with age, and to adverse selection as its main cause, are not unique to Spain. Blundell et al. (2013) show similar employment rates with respect to age for the US, UK and France. In addition, Topel and Ward (1992) show that the early years in the labor market are very important, since that is when most wage increases and job changes happen. Moreover, the finding that labor demand elasticities are higher for younger workers than for prime-age workers is consistent with recent evidence that estimated demand elasticities for different age groups separately and in different contexts. Katz (1998) estimates an elasticity of -0.5 for disadvantaged youth in the US. Egebark and Kaunitz (2014) estimate an elasticity of -0.31 for young workers in Sweden during the Great Recession.[11] Huttunen et al. (2013) measure an elasticity between -0.067 and -0.13 for low-wage workers older than 54 in Finland.[12] The advantage of this paper is that the same policy applied at different ages, during the same macroeconomic context and within the same set of institutions.

[11] A potential explanation for the higher elasticity estimated in this paper relative to Egebark and Kaunitz (2014) is the high level of unemployment for workers younger than 30 in Spain.

[12] A crucial difference between the policy studied in Huttunen et al. (2013) and the one in this paper is that the tax credit applies to current employees and new hires, not only to new hires. They show that the policy reduces exits to non-employment, but does not affect entry from unemployment. It is thus consistent with the results in this paper: the elasticity for prime-age marginal workers is 0. I discuss further the implications of this in section 5.

Thus, we can be sure that the different elasticities are caused by changes during the life cycle and not by other contextual factors. In light of previous evidence, the policy implications might apply to other countries.

I discuss the two main implications for policy. First, the estimates of labor demand elasticities of young and prime-age workers suggest that the optimal profile of payroll taxes should be age-dependent. It would start at a lower level for young workers and rise with age, reaching a plateau somewhere between the ages of 30 and 45 as increasing adverse selection makes marginal workers unemployable. While a lower payroll tax would not encourage hiring of workers close to retirement, results in Huttunen et al. (2013) suggest it would help some to stay employed. Thus, payroll taxes should start decreasing as workers approach the age of retirement. Theoretical work on the optimal age-profile of payroll taxes is a promising avenue for future research. Related work exists for income taxes (Weinzierl, 2011) and employment protection (Cheron et al., 2011).

Second, labor demand elasticities are also important for the design of work-encouraging transfer schemes such as the Earned Income Tax Credit (EITC). Saez (2002) shows that the EITC resembles an optimal transfer program when labor supply responses happen along the extensive margin. However, his analysis rests on perfectly elastic demand. Rothstein (2010) simulates the impact of the EITC and shows that with a finite labor demand elasticity a substantial part of EITC payments is captured by firms through lower wages. Moreover, workers who are ineligible also experience wage declines. Thus, the estimates in this paper draw into question the ability of the EITC as a redistributive tool.

The paper is organized as follows. Section 2 describes the administrative dataset I use, the institutional details of payroll tax legislation in Spain, and considers the theoretical predictions of the reforms for the young and prime-age labor market. Section 3 presents the empirical strategy, the results, and a cost benefit analysis. Section 4 discusses the age-specific labor demand elasticities estimated in light of the previous literature, and provides evidence in favor of adverse selection as the main driver of the decline of the elasticity with age. I also show evidence ruling out other potential stories. Finally, section 5 discusses the policy implications.

2 Data and Institutional Context

2.1 Data

I use data from the Continuous Sample of Work Lifes (Muestra Continua de Vidas Laborales, MCVL). It is a joint administrative dataset from three different sources: the social security administration, the census, and the tax administration in Spain. It has detailed information on the start and end of each employment and unemployment spell, monthly wages (bottom- and top-coded), the reason why the job relationship ended, the type of contract (very importantly, whether the

contract benefited from a tax credit or not), the size of the firm, the sector, whether the job was part-time and the number of hours, the location of the job, etc. The data also contains information about the individual: sex, education, date of birth, province of birth, citizenship, as well as the date of birth and sex of the members of their household.

The sample was constructed in the following way: in 2004, over 1 million of workers, or 4% of all individuals who had some relationship with social security, were selected.[13] Sampling was random, without any kind of stratification. The data contains the labor history of each individual since he started working, including periods when the worker was collecting UI or after he retired and started receiving pension benefits. The same individuals selected in 2004 were followed for each edition of the dataset between 2005 and 2012. Thus, I can reconstruct the working life of the individuals since they started working up to 2012. In case a worker selected in 2004 leaves the sample in any of the future years, because he stops having a relationship with social security (i.e. he is out of employment and does not collect UI; he dies), he is replaced by another randomly selected worker that had some relationship that year with social security. Similarly, the whole labor life of that new worker is included in the dataset. Finally, if any of the workers is not sampled during one of the editions of the dataset because he did not have any relationship with social security for a year or more, but he becomes employed again, he will reappear in the dataset the year in which he had restarted his relationship with the social security system.

The retrospective nature of the dataset raises concerns about its representativeness for the years before 2004. This might be a problem specially for the results exploiting the policy change in 1997. However, as shown in Bonhomme and Hospido (2012) it is only an issue when going back to the late 1980s. There are four main reasons for that. First, mortality rates are low throughout the period. Second, attrition due to exit from the labor force because of retirement is not a problem since the dataset includes pensioners and their previous labor histories. Third, from the mid nineties and until the Great Recession, emigration out of Spain was very low. In fact, Spain became a host country for immigrants. Fourth, early career interruptions are a concern for women. However, as I show in section 4, the results are the same across genders. Thus, problems of attrition due to the retrospective design of the sample are not a concern.

Table 1 reports summary statistics for the year 1997, when the tax credit policy was enacted. I classify the workers in 5 year age groups and report the descriptive statistics for the main groups of the empirical analysis: 25-30, 30-35, 40-45, and 45-50. Workers are more likely to be men for all age groups. Most of them have achieved at most secondary education. The fraction that at most completed primary education is increasing in age. Most of them are Spanish citizens, but the importance of the immigrant labor force is bigger for the younger cohorts (almost 20%) than for older cohorts (around 5%). Their real daily wage is of 37 euros for young workers, rising to

[13] Individuals who had some relationship with social security were either formally employed, receiving some kind of unemployment insurance, or were perceiving a contributory pension.

47.26 for prime-age workers. The fraction of workers in what is considered good jobs (permanent and public workers) is increasing in age, while the fraction of employees in short-term contracts is decreasing. Incidence of part-time work is higher for younger workers. The mean size of the firm is around 8-10 workers and most people work in the services sector.

2.2 Payroll Tax Legislation in Spain

Payroll tax legislation sets different payroll tax rates depending on the regime to which the worker is affiliated. The main group, called *"Régimen General"*, includes most private and public employees (13,419,951 workers or 77% of total).[14] The following groups are self-employed workers (2,951,021 workers or 17% of total) and farmers (685,960 or 4% of total). There are other small schemes for coal workers, sea workers, and housekeepers. The employment credits that are the focus of this paper apply to all *new permanent jobs*, except for the sector of self-employed individuals. I will thus focus on workers affected by the policy, but will also discuss the employment effect for self-employed workers, since it is a common practise by firms to declare some work as carried out by self-employed individuals to avoid paying payroll taxes and severance payments.

Payroll taxes in Spain are paid both by the employer and the employee. They are a function of the wage of the employee and two tax rates: one that applies to employers and one that applies to employees. There is a maximum and a minimum base for the wage depending on the occupational category of the worker. The first two columns in table 2 show, for employees in *"Régimen General"*, the minimum and maximum basis for each category of worker for 1997. The last three columns show the tax rates for both employers (23.6%) and employees (4.7%), as well as the combined tax rate (28.3%).[15] The tax revenue collected is used to pay unemployment, workers' accident, and health insurance; and retirement, widow and orphan pensions. The money is also allocated to pay for training courses and to protect the workers in case of firm's default.

Spain's unemployment is very high and volatile as can be seen in figure 1a. Even in the peak of the 2000's housing boom, unemployment was around 10%. It is also higher for younger cohorts and women (figure 1b). In order to stimulate hiring and increase employment, the Spanish government has implemented policies to reduce labor costs.[16] Permanent and temporary contracts are the two main types of work arrangements in Spain; the programs that reduce employers' payroll taxes apply only for workers hired as *new permanent workers*. It would have been a controversial political decision to stimulate temporary employment. In fact, one of the objectives of the reform was to reduce the fraction of temporary jobs. Temporary contracts are generally considered bad jobs in

[14] Data is for 2010. Source is Ministerio de Empleo y Seguridad Social (MESS), 2012.

[15] The payroll tax rate of the main group of workers has been very stable. Last reform took effect in 1995 and decreased firms' payroll tax rate by 3.3% and employees' payroll tax rate by 4.1%.

[16] See online appendix for a full list of laws regarding payroll tax policy.

Spain, though it is hard to causally prove that they harm workers.[17] Jimeno and Toharia (1993) show that temporary contracts have a negative wage differential of about 10%. However, Davia and Hernanz (2004) show that this wage differential is caused by different worker characteristics. Arranz and Garcia-Serrano (2007) show that job stability has declined in Spain since the introduction of temporary contracts. Regarding the effects on training of short-term contracts, Albert and Hernanz (2005) find that workers holding temporary contracts are less likely to be employed in firms providing training. More importantly, temporary workers employed in firms providing training are less likely to be chosen to participate in training programs.

There are three main programs that reduce payroll tax rates: first, for workers hired before their 30th birthday; second, for employees hired after their 45th birthday; third, for the long-term unemployed (LTU) regardless of their age. Figure 2 displays the evolution of these programs over time. The upper figure depicts the case of the credit for young workers and the LTU. Both tax cuts were introduced in May 1997. At that time, the credits represented a 40% decrease of the tax rate for the first two years of the contract. There were some changes in the generosity of the tax credit, but they always applied for the first two years of job relationship. The main difference between them is that the credit for young employees did not require the worker to have been at least 1 year unemployed. Thus, they applied to a broader population. The credit for the young was discontinued in March 2001, while the LTU credit was kept in place. After that, firms hiring young workers with a credit could only do so if the individual fell in the category of LTU.

The lower figure depicts the case of the prime-age credit after 45 and that for the LTU. The credit for workers older than 45 was first enacted in 1982. Until 1997, it also required that the worker had been LTU (red dashed line). After that, it applied to all workers older than 45 (red solid line). During the period of study, the prime-age credit after 45 suffered only small changes in its generosity. The presence of a credit for the LTU is important because it will minimize substitution effects across the age thresholds.

The dose of the treatment is sizable. Using the basis and tax rates for 1997 and assuming an employee with wage equal to 2,000 euros, the monthly tax credit after 1997's reform, for hiring a young worker, is $2000 \times .236 \times .4 = 188.8$. Thus, the subsidy represents a 7.6% saving in labor costs during the first two years of the job relationship.[18] In contrast, the monthly tax credit for workers older than 45 years old when hired as permanent workers would have been $2000 \times .236 \times .6 = 283.2$ euros each month during 2 years, and $2000 \times .236 \times .5 = 236$ euros for the rest of the contract duration. The savings represent 11.5% and 9.5% of the labor costs for the first two years and the rest of the contract, respectively. Thus, the employment credits represent an important reduction

[17] Autor and Houseman (2010) do provide evidence that temporary job positions harm workers in a US context.

[18] Note that this is the percentage saving for each new subsidized young hire during the first two years of job relationship. Since currently employed workers are not subsidized, the average reduction in labor costs per worker will be much smaller. See section 4 for more details.

in total labor costs.

Though the tax credits are not limited to firms that expand its workforce, its administrative design makes them similar to a marginal employment subsidy (Johnson and Layard, 1986). There are several limitations that limit the scope of the employment credit so that it targets only individuals with low job stability (temporary workers and unemployed).[19] Most importantly, they can only be received for workers who have not been working in a permanent contract during the last 3 months. Since on average temporary and unemployed workers have lower skills, this implies that the program targets low-skilled individuals (Albert and Hernanz, 2005; Arranz and Garcia-Serrano, 2007; Davia and Hernanz, 2004; Jimeno and Toharia, 1993).

Two administrative details of the employment credits are important to limit the possibility of strategic behaviors by firms, like excessive churning. The first one was introduced in 1999. Firms who wrongfully dismissed workers with a tax cut are ineligible to hire again with a tax credit.[20] The second limitation is that an employment credit contract cannot be signed with workers who hold a permanent contract with the same firm group during the previous 24 months.[21]

Wage-setting in Spain is quite centralized. All collective bargaining agreements negotiated at a level superior to the firm (i.e. national and provincial agreements; sectoral agreements) apply to all firms that belong to the corresponding geographical or sectoral area, even if they did not participate in the negotiation.[22] In general, lower level agreements cannot modify agreements reached at a superior level. Consequently, around 90% of workers in the private sector have their wages fixed by collective bargaining (Izquierdo et al., 2003; OECD, 2012). The negotiated wage is occupation specific (i.e. manager, administrative, etc.), applies to all ages, and increases with tenure within the firm.[23]

Claiming a tax credit was an easy task. Figure 3 shows the back-page of a labor contract. The employer has to fill in one of the options available in the sixth clause of the contract. Option a) specifies the tax credit that was available between January 2000 and March 2001 for workers under 30. Option c) specifies the tax credit when the employer hires a worker over 45 years old. Finally,

[19] Guell and Petrongolo (2007) estimate that 86% of new entries in Spain are under short-term contracts, and that only 5.7% of them are converted into permanent jobs.

[20] The limitation applies either for a year since the dismissal happened or for as many workers as wrongful dismissals happened.

[21] Other limitations are: the tax-credited contracts cannot be used to hire relatives of the owner or of the management chief; there are people who cannot benefit from the contract too: firms managers, home service, people in jail, professional sportsmen, artists, and dockers working for public societies; the employers need to be current with tax payments and must not have been excluded from the program because of any infraction they could have committed. Finally, the tax-credited contract, combined with other programs, cannot suppose a tax credit of more than 60% of the annual wage.

[22] Agreements affecting most workers (50%) are negotiated at a sectoral and provincial level. 25% of workers' conditions are negotiated at the national level. 8% are negotiated at the state level (Izquierdo et al., 2003).

[23] Other issues negotiated are overtime hours, conversion of temporary contracts into permanent, limitations to temporary and part-time hiring, retirement, and services to workers as provision of lunch and transport.

options b), d) and e) describe the tax cuts available if the firm hires employees in other situations not only related to their age: workers registered as unemployed for at least a year, women hired in sectors in which they are underrepresented, and unemployed people perceiving unemployment assistance.

Finally, the age-targeted employment credits were accompanied by lower severance payments. However, Elias (2014) explores the effects of lower severance payments for young workers during the period 2001-2006, when no employment credits were available for that group. There are no effects on hiring, employment or wages of reduced dismissal costs. Elias (2014) argues that the main reason why this policy was not effective is that only firms that did not dismiss a worker in the last 6 months could hire another worker with lower severance payments. The rationale for such restriction was to limit excessive churning. Firms with the most turnover are likely to be the most affected by high employment protection, but the limitation will not allow them to benefit from lower severance payments. Such limitation was in place between 1997-2001 only to claim lower severance payments, not the tax credit. Therefore, the main effect of the policy changes in 1997 must have been related to the employment credits. The details of severance payments regulation are explained in the online appendix.

2.3 Theoretical Predictions and Heterogenous Responses

The standard tax incidence model, or competitive labor market, predicts that a decrease in payroll taxes will shift demand outwards. My identification strategy thus relies on this exogenous change in demand. The new employment and wage equilibrium will depend on the elasticities of labor demand and supply. The more elastic is supply, the greater will be the effect on employment, and the smaller the effect on wages. On the other hand, the more elastic is demand, the greater the effect on both employment and wages. Figures 4a, 4b, and 4c represent the extreme cases with perfect elastic supply, perfectly inelastic supply, and perfectly inelastic demand, respectively.[24] [25]

But as explained in section 2.2, the labor market in Spain is not a spot market. Around 90% of workers in the private sector have their wages determined by collective bargaining. Figure 4d represents the equilibrium in a right-to-manage model, in which unions and employers bargain over wages (Nickell and Andrews, 1983; Johnson and Layard, 1986; Boeri and van Ours, 2008).

[24] Note that supply will not shift out. In the standard tax incidence model, shifts in supply depend on changes in the reservation wage. Its main determinant is non-wage income. Since the reform does not alter that variable, supply does not shift out. In the case of a search model, an increase in the arrival rate of job offers increases the reservation wage. That lowers the probability of accepting an offer and is akin to a decrease in labor supply. In that case we expect to find increases in wages.

[25] Models that depart from the competitive labor market framework by introducing search-and-matching frictions also predict that an employment credit will shift demand out (Pissarides, 1998; Mortensen and Pissarides, 2001), and that the new employment and wage equilibrium will depend on the point where the demand and wage (supply) functions cross. I discuss further the implications of search-and-matching models in section 4.

Then, employers take wages as given and choose employment levels that maximize the profits of the firm. The outcome depends on the bargaining power of unions ($0 \leq \beta \leq 1$; 0 is the competitive case, and 1 the case in which the union sets wages unilaterally). The solid black line represents the competitive equilibrium case.[26] The stronger the bargaining power of unions, the higher the equilibrium wage. The dashed black line depicts the situation when all bargaining power is on the union-side. The exact location of the equilibrium depends on the bargaining power of unions. The shift outward in demand will increase employment, but wages will remain the same as long as supply constraints do not become binding. Given the level of union coverage in Spain, and the high level of unemployment, such a representation seems realistic. Note that the predictions are the same in a competitive market with perfectly elastic supply, and thus the standard incidence formulas can still be applied.

A shift in demand corresponds to firms who are at the margin of hiring. The tax credit makes some new matches productive and employment increases. However, there will also be responses by firms that would have hired in any case, but will do now with a tax credit. This can happen through two channels: first, firms can substitute workers above 30 (below 45) for workers below 30 (above 45) (Davidson and Woodbury, 1993). Second, firms can claim a tax credit for a worker under 30 or above 45 that would have been hired in any case, and receive the tax credit as a transfer. Such behavioral responses will give rise to inefficiencies in the implementation of employment credits and I will explore them too.

But transaction costs will limit the extent to which substitution and windfalls are happening. First, as described in section 2.2, there are several limitations in the policy that will minimize such behavior. Second, substitution across age groups depends on the extent to which workers under 30 and above 45 are good substitutes in production for existing workers, and on the extent that it is easy to churn employees. Given the high level of severance payments for permanent workers, dismissing permanent workers to replace them with subsidized ones seems a rather costly alternative.[27] Third, there is also an employment credit for the long-term unemployed that can be used regardless of the age of the worker. Thus, the hiring of the most disadvantaged workers between 30 and 45 was also incentivized.

However, can we expect the responses to be identical in the young and prime-age labor market? In other words, are labor demand and supply elasticities the same over the life cycle? Closer inspection of each labor market suggests that this will not be the case. Each labor market has very distinct features. Figure 5 contains several pictures that summarize the main differences.

[26] Labor supply is flat as a consequence of assuming that all workers are identical and have the same reservation wage. While this might not be the case, it eases the graphical representation.

[27] For dismissals considered wrongful by the courts, severance payments amount to 45 days of salary for each year of tenure in the firm, up to 42 months of salary. 3/4 of all layoffs that go to court are considered wrongful by judges (Bentolila, 1996). For more details on severance payments see the online appendix.

Figure 5a shows that the employment rate of workers younger than 30 years old is much lower than for prime-age workers. It also shows that the most important increases in the employment rate happen before the age of 30. Between the age of 20 and 30, the employment rate increases by 35.86 percentage points (pp). After 30, the evolution is much slower, increasing by an extra 12.95 pp at the age of 45. The low employment rate of young workers is certainly due to these individuals being in other activities, such as education, but it is also caused by a much higher incidence of unemployment for young workers. As can be seen in figure 5b, the unemployment rate is around 37% for workers aged 20-25, and drops to 20% for those aged 30-35. Unemployment of prime-age workers is much lower, being around 12.5% at ages 45-50.

Since the policy subsidizes only new permanent hires, it is important to further distinguish between permanent workers and those in other types of contracts. Figures 5c and 5d display the ratio of permanent and temporary workers with respect to all individuals who are working at each age, respectively. Before the age of 30, there is an increase in the ratio of permanent workers, and a decrease in the ratio of temporary ones. Therefore, the young labor market is characterized by transitions to more stable jobs. Note that the ratio of permanent workers barely changes after the age of 30. It remains constant around 50% until the ages of 55-60, when workers start retiring. In contrast, the ratio of temporary workers after 30 is still decreasing, though at a much slower rate. These are workers that are becoming self-employed or are finding public sector jobs, as can be seen in figure 9 in the online appendix. Thus, there does not seem to be much room at the age of 45 to increase permanent employment.

Figure 5e shows the starting wage of permanent workers at the age at which they are hired. It is before the age of 30 when most wage increases happen. After 30, the wage of new permanent hires stabilizes. Consequently, the young labor market is also characterized by transitions to better jobs, while such dynamism halts after 30.

Finally, figure 5f shows the mean length of permanent contracts over age at hired. Job tenure of new hires is increasing until 30, is stable until the age of 50, and then starts dropping as workers approximate the age of retirement. Lower job tenure for young workers is important since it might deter some firms from hiring them. If an employer has to invest in worker skills, he wants to maximize the expected return from a job relationship. To the extent that younger workers stay shorter in firms, that can deter hiring in the young labor market.

It is important to note that these characteristics are not unique to the Spanish labor market. Blundell et al. (2013) plot the employment rate for the USA, UK and France in 1977 and 2007 and find similar patterns. Topel and Ward (1992) also show, for the US, that it is during the early years in the labor market that most wage increases and job changes happen. Finally, Murphy and Welch (1990) show, also for the US, that the age-earnings profile is an increasing concave function, with most wage increases happening during the early years of a worker's career. To the extent that the characteristics of the Spanish young and prime-age labor market are shared across countries and

over time, the findings of this paper will have a wider applicability for labor policy design.

3 Empirical Strategy and Results

The policy changes in 1997 and 2001, as well as the age discontinuities at 30 and 45 years old, provide the opportunity to explore the effects of employment tax credits through two different empirical strategies. First, I implement a RDD exploiting the policy age cutoffs. The estimation window is 12 months on each side of the threshold. The specification for the discontinuity at 30 is:

$$y_{it} = \eta \mathbb{1}[\widetilde{age}_{it} < 30] + \beta \widetilde{age}_{it} + \lambda \widetilde{age}_{it} * \mathbb{1}[\widetilde{age}_t < 30] + \epsilon_{it} \tag{1}$$

y_{it} indicates whether individual i is employed or transitions in and out of a job (permanent, temporary, self-employed, public), $\widetilde{age}_{it}$ is the month distance with respect to their 30th birthday, and $\mathbb{1}[\widetilde{age}_{it} < 30]$ is a dummy indicating that the worker did not cross his 30th birthday yet. Thus, the coefficient of interest is η.[28]

Identification in a RDD relies on no manipulation of the running variable, that is, the age at which the hire occurs. It is plausible that firms game the regulation by substituting workers older than 30 (younger than 45) for workers younger than 30 (older than 45). But substitution might not only happen across ages, but also across contract types (permanent, short-term, self-employed, and public). Given the diversity of manipulation strategies, the RDD will first help us identify the relevant adjustment channels.

The second strategy exploits the policy changes through a difference-in-difference. I select a window of time of a year and a half before and after the reforms, and construct a quarterly balanced panel of workers aged 25-30, 30-35, 40-45 and 45-50. Individuals aged 30-35 and 40-45 are the control group. The specification is:

$$Y_{it} = \alpha + \beta_1 Treatment_i + \beta_2 Post_t + \beta_3 Treatment_i * Post_t + \gamma X_{it} + \epsilon_{it} \tag{2}$$

Subscript i denotes the individual, and q the quarter. I will use as outcome variables, Y_{iq}, dummies indicating whether an individual was employed, and also whether the worker was hired, laid-off, or quitted his job. The latter two variables are important since they will show whether there is excessive churning from employers willing to game the regulation by separating from their workers, and later rehiring employees with a tax credit. X_{iq} is a vector of control variables: sex, education, disability, immigrant, dummies for industry sector, part-time job, firm's workforce size, province fixed effects.

Identification in a DD analysis relies on parallel trends for both treatment and control groups. If transitions and employment for each group were following different trends, a DD estimate might just capture these differential patterns. Thus, I complement the static evidence by running a

[28] Similarly, for the discontinuity at 45, the specification is: $y_{it} = \eta \mathbb{1}[\widetilde{age}_{it} > 45] + \beta \widetilde{age}_{it} + \lambda \widetilde{age}_{it} * \mathbb{1}[\widetilde{age}_t > 45] + \epsilon_{it}$

specification including interactions between the treatment group and 10 quarter time interactions.[29] I will plot the coefficients and standard errors for each period and confirm that there are no differential pre-treatment trends across groups. Still, as in the RDD, the parallel trends assumption might not hold after the policy change. Substitution from the control to the treatment group will bias upwards any employment estimate.

Assessing the importance of the substitution effects is of central importance. The RDD and DD estimates will provide first evidence of which are the various channels that firms are using to adjust to the policy. These strategies will show us that in the prime-age labor market, the policy only induces substitution and has no effect on employment. For the case of young workers, there will be an increase in the employment of workers younger than 30, relative to those older than 30. I will show that the relative increase in young employment is indeed net job creation, and that substitution was not a concern. To do that, I follow three main strategies: first, I use 2001's reform to observe how the age-distribution of hires and separations converges. Second, I construct a counterfactual of how hiring would have been in non-treated areas next to the thresholds. If substitution is more intense next to the discontinuities, such strategy will detect a missing mass (Kopczuk and Munroe, Forthcoming). Third, I repeat the DD results but using several control groups: 30-31, 31-32, 32-33, 33-34, and 34-35. I detail each strategy and provide some additional tests in section 3.3. All the strategies fail to provide evidence consistent with substitution effects for workers older than 30.

Finally, the difference-in-difference and RDD estimates can be interpreted as reflecting the short- and long-run responses to the policy, respectively. The DD analysis focuses on the effects six quarters following the policy reform in 1997. Short-run responses might not be very informative of how employment credits affect behavior in the long-run or in the new steady-state if agents face optimization frictions or adjustment costs in the short-run (Chetty, 2012; Kleven and Waseem, 2013). For instance, (Card et al., 2009) finds that the distribution of long-run outcomes of active labor market policies is more positive in the long-run than in the short-run. Nevertheless, it might also be the case that in the long-run, firms and workers start using the policy discontinuities in a strategic way, undoing any benefitial effects of the policy. The RDD design can shed light on long-run responses since it does not focus on the effects immediately after a policy change. However, a caveat of this interpretation is that the sample of workers that are subject to the policy changes over time.

[29] The specification is:

$$Y_{iq} = \alpha + \delta_q + \phi_a + \beta_q Treatment_i + \gamma X_{iq} + \epsilon_{iq} \tag{3}$$

Y_{it} indicates whether individual i is hired, laid-off, quits or is employed in period t. ϕ_a are age specific dummies. β_q are the quarter by quarter DD estimates. X_{it} is a vector of control variables as in equation 2.

3.1 Effects on Transitions, Employment and Wages of Prime-Age Workers

The evidence on transitions and employment of prime-age workers is the same both using the DD and the RDD strategy. I thus discuss only the RDD here, and I relegate the DD findings to the online appendix. Figure 6 displays the hiring flows around the 45th birthday. Figures on the left are for the period when the policy is in place (1997-2006). Figures on the right are for a period when the policy had an extra requirement: only workers older than 45 that had been unemployed for a year could be hired with a tax credit. As can be seen, the policy generates a big jump in permanent hiring at 45 between 1997-2006. Visual inspection suggests that firms are gaming the regulation. After the 43rd birthday, the distribution features a faster decline in hiring. This suggests that firms delay some hires until the worker's 45th birthday. For the period 1992-1997, the distribution of permanent hires does not display a similar distortion. There is though a jump at 45, which is consistent with the one year of unemployment that was necessary then to claim the tax credit over 45. Thus, some workers might have waited to be hired until they fulfilled both requirements. As can be seen from the lower figures, the policy does not affect the flows into temporary jobs. This is consistent with the policy only subsidizing permanent jobs.

Figure 7 displays the stocks of workers within each type of job around the 45 year threshold for the period 1997-2006. In contrast to what the flow figures suggest, the policy is affecting both temporary and permanent workers. The stock of permanent workers decreases around the age of 44, whereas the stock of short-term workers increases around the same age. At 45, there is a jump upwards in the stock of permanent workers, and a jump downwards in the stock of temporary employees. The slope after 45 for permanent jobs is steeper than before the threshold. However, as can be seen in figure 7c, this does not translate in a reduction in non-employed workers. The figures suggests that some of these extra permanent workers after 45 would have been either temporary or would have worked in the public sector. Overall, the figures suggest a null effect on employment of prime-age workers.

If employers were delaying the entry into permanent contracts of temporary workers, there should be an increase in the length of temporary contracts before 45. Consistent with that, figure 7f shows that between the ages of 41 and 44.5, temporary contracts were unusually long.

Table 3 translates the above discussion into estimates. Panel A and B analyze the effects on hires. Panel A restricts the sample to 1 year immediately after the policy change (short-run). Panel B is for the sample between 12 months after the policy change until 2006 (long-run). Both in the short- and long-run, the policy only affects flows into permanent employment at the thresholds, but not entries into short-term, self-employment, public jobs or UI. Long-run estimates of transitions are slightly larger than short-run ones, but the estimates are not significantly different. This finding suggests that for new hires adjustment to the policy was fast and that optimization frictions were not very important.

Panel C reports the results for employment effects at the 45 years old discontinuity. There is a positive effect on the number of people employed with permanent contracts. However, the size of the effect is identical to the drop of people employed with short-term contracts after 45, as column 3 indicates. There is no effect on employment of the program. Thus, the program for workers older than 45 years just retimes conversions from short-term to permanent that would have had happened otherwise. Note that the slope after 45 for permanent contracts is positive and significant. The slope after 45 for short-term workers is negative and significant, and so is the slope for non-employed workers. However, that might be caused by changes in the slope before 45 because firms and workers are delaying permanent hiring until the worker crosses the 45th birthday, as discussed above. To test whether the change in slopes is caused by reallocation in the proximity of the threshold, panel D reports the results of a donut RDD. I omit 1 year on each side of the threshold. Thus, I test whether the progression of employment is the same between 12-24 months before the discontinuity with respect to 12-24 months after the threshold. The lower panel of table 3 reports the results of the slopes: permanent employment is still increasing faster after the threshold, but once one accounts for the decrease in temporary employment, the slope after 45 is no longer significant (column 3). There is a small but significant negative effect on the slope after 45 for public workers. Overall, non-employment is not decreasing faster after the discontinuity (column 6), confirming the graphical evidence.

The results for prime-age workers show that the payroll tax credit at 45 was not successful in stimulating prime-age employment. Firms gamed the policy by arbitraging between subsidized contracts (permanent) and non-subsidized ones (temporary). This result is surprising in light of a 9.5% decrease in labor costs for the whole duration of the contract. One potential reason why the policy might fail in increasing employment is that workers were capturing the rent in terms of higher wages. However, in section 4 I will show that that was not the case and that firms are actually the ones receiving the transfer.

I now turn to discuss the impacts on wages. The results are based on a DD analysis exploiting the change in 1997 and are shown in columns 1-3 in table 4. The dependent variable is the log real daily wage and I select the sample of new permanent hires. The retiming of entry into permanent jobs around 45 will cause compositional effects on the treatment and control groups. Better workers should be able to avoid a delay when signing their permanent contract. Thus, the pool of permanent hires after 45 should become relatively worse after 1997. And the pool before 45 relatively better. Column 1 reports the results without control variables. Wages of new hires after 45 are on average 2.3% lower. However, when I include individual controls the coefficient drops to -1.8% and is only significant at the 10% level.[30] When I include occupation, cohorts, calendar quarter, and province fixed effects the coefficient is no longer significant and drops to -0.3%. Thus, once one accounts for compositional changes, there does not seem to be any effect on wages of prime-age workers.

[30]Controls are: education, experience, sex, citizenship, workers' disability, firm size, firm sector, and part-time job.

3.2 Effects on Transitions, Employment and Wages of Young Workers

Difference-in-Difference Evidence. I start by exploring the effects of the employment credit through a DD. Since permanent and temporary contracts are the most prevalent ones, I will focus most of the discussion on them. The results for transitions in and out of these work arrangements are in the upper panel of table 5. Permanent hires increase by 0.35 (pp). The number of permanent lay-offs decreases by 0.046 pp. This could be caused by firms dismissing workers over 30 in order to replace them with younger subsidized workers. In contrast, the number of quits increases by 0.082 pp. The latter effect is consistent with some young workers breaking their job matches knowing that, thanks to the employment credit, they are more likely to receive a job offer and improve their job situation. However, the estimates of separations are an order of magnitude smaller than those of hires. This indicates that excessive churning is not a concern.

There is also an increase in temporary hires of 0.37 pp. This is an indirect effect of the policy, since temporary contracts were not subsidized. However, both lay-offs and quits of temporary workers also increase by .32 pp and .15 pp. Firms could be using temporary contracts to screen young workers. For those who perform better, they will break the temporary contract and hire them as permanent. Consistent with that story, transitions from temporary to permanent contracts within the same firm also increase by 0.18 pp (column 7, panel A).

The analysis of flows suggests that the stock of young workers should increase. The lower panel of table 5 reports the estimates for employment. The stock of young permanent and temporary workers increases by 0.22 pp and 0.19 pp, respectively. Note that there is no evidence of crowding out of other work arrangements like self-employment or public jobs. Thus, overall employment increases by 0.34 pp. Very importantly too, the stock of workers receiving UI decreases by 0.17 pp. Part of that effect can happen because temporary workers are more likely to suffer non-employment spells because of the short-term nature of their job. Another part can come from workers unemployed for a long time. The decrease in UI implies that subsidizing employment might be a cost-efficient way to increase employment, since it will trigger savings from social insurance schemes.

Figure 8 displays the dynamic effect of the policy. The identification assumption of parallel trends across treatment and control groups, before the policy change, appears to hold. The estimates oscillate around zero and are not significant before the policy change. Once the policy is enacted, the estimates jump upward and become significant.

If I compare the estimates to the mean level of transitions and employment during the year previous to the reform in 1997, they represent an increase in permanent transitions of 57% (relative benchmark mean is 0.61%). The increase in short-term transitions is of 15.6% (benchmark is 2.38%). Permanent employment increases by 1.06% (benchmark is 20.85% and short-term employment by 0.97% (benchmark is 19.68%). Overall private sector employment of workers aged 25-30 increases by 0.84% (benchmark is 40.53%) and the number of UI recipients in this age group decreases by

3.82% (benchmark is 4.42%).

I now turn to the effects on wages of employment credits. I repeat the difference-in-difference analysis as in equation 2. The dependent variable is the log daily real wage of new permanent hires. On one hand, the increase in employment of young workers might push wages up if there are supply constraints. However, such effect is unlikely given that the wage of 90% of private sector workers are decided by collective bargaining (see section 2.2). On the other hand, the increase in employment under 30 might be happening through workers of lower ability or in less productive positions (and hence, lower collectively-bargained wages). If such compositional effects are happening, we expect to find a negative effect on the average wage. Columns 4-6 in table 4 show the results. Column 4 does not include controls, column 5 includes individual characteristics, and column 6 includes several fixed effects.[31] None of the coefficients are statistically significant. Note also that the coefficient in the most stringent specification is very small (0.09%). In line with the institutional details of collective bargaining, there is no evidence of supply constraints. There is no evidence either that the increase in employment of young workers is happening mainly through an expansion of jobs for low-wage positions.

The employment credit for young employees was discontinued in 2001. This provides and opportunity to check the robustness of the 1997 DD. Since the tax cut had been in place for almost 4 years, it also allows to test for how persistent the effects are. The findings are in the online appendix. All the results are analogous to those obtained with the change in 1997.

Regression Discontinuity Evidence. Figure 9 displays the hiring flows around the 30th birthday. Figures on the left are for the period when the policy is in place (1997-2001). Figures on the right are for a period when the credit for young workers did not exist (2001-2005). As can be seen, the policy generates a jump in permanent hiring at 30 between 1997-2001. For the period 2001-2005, the distribution of permanent hires does not display such jump. The lower-left figure shows the hiring distribution of temporary contracts when the credit was available. In contrast to the DD evidence, there is not evidence of the subsidy affecting temporary hires around the threshold. This is consistent with employers needing time to screen workers before hiring them as permanent with the tax credit.

Table 6 reports the estimates of the jump at 30. Panel A shows the results for the first 12 months after the policy is enacted (short-run), and panel B from the 12th month and onward (long-run). Like the results for prime-age workers, the tax credit does not affect transitions into temporary or public jobs, self-employment, and UI. The long-run estimate is larger (.14 pp) than the short-run one (.11 pp), but not significantly different. Like for prime-age workers, the response in hiring was fast.

In terms of the stocks of workers, the payroll tax credit for individuals younger than 30 generates

[31] Individual controls are education, experience, sex, citizenship, workers' disability, firm size, firm sector, part-time job. Fixed effects include occupation, province, cohort, and calendar quarter.

kinks at the discontinuity. Figures 10 and 11 display the evolution of workers in permanent or short-term contracts, receiving UI, or the fraction of all those who work. I restrict the treatment sample to cohorts that were at most 29 in May 1997 (born between 05/1968 and 03/1971), so that they could benefit from the policy for at least a year. I use as a placebo sample cohorts that could not benefit from the employment credits during the months before becoming 30 because the policy had been removed (born between 03/1973 and 03/1976). Cohorts crossing their 30th birthday when the policy was in place experienced slower increases in their probability of being permanent workers after the threshold. The fraction of them being short-term workers was decreasing before 30, and decreased at a slower pace after 30. This indicates that in the long-run, part of the effect is shifting between subsidized and not subsidized contracts. There also seems to be a smaller slope after 30 for the evolution of all individuals who are working, indicating that the policy is also having employment effects. As a confirmation of that, note that the evolution of UI recipients was decreasing before 30, and stabilizes or slightly increases after 30.[32] Note that for placebo cohorts there are not such changes in slopes centered at 30.

To translate the above discussion into estimates, I perform a RDD as in equation 1, but now the coefficient of interest is λ, or the differential employment slope after 30 relative to the slope before 30. Table 6 reports the results. Panel C shows it for treated cohorts and panel D for the placebo cohorts. The coefficients confirm the visual analysis. The slope after 30 for permanent workers is significantly less positive, whereas that of short-term workers is significantly less negative. The slope for UI is also significantly less negative after 30. Most importantly, the slope after 30 for overall employment is significantly less positive. The coefficient indicates that the increases in overall employment are 0.065 pp smaller every month after 30. To compare that estimate with the difference-in-difference one, 6 quarters after overall employment would have increased by 1.17 pp more if the policy had been in place until the age of 31.5. The estimate in the long-run is significantly larger than the short-run one, that indicated an increase of .34 pp 18 months after the policy had been enacted. Thus, while the effects on transitions are quite immediate, the increases in employment take more time to build up. Finally, note that none of the estimates for the slope after 30 for the placebo cohorts are significant (panel D).

3.3 Substitution Effects

Both the DD and the RDD evidence confirm that the payroll tax credit increased employment under 30 relative to that over 30. However, part or all the increase could be offset by a negative effect on workers older than 30. I turn now to explore this possibility. I rely on several methods.

Convergence of the hiring distribution after 2001. First, I consider the potential effects that the credit for workers younger than 30 could have on hiring on each side of the threshold.

[32] For self-employed and public workers, see figure 8 in the online appendix .

Figure 12 illustrates each case. The first graph considers the situation absent any discontinuity. In that case, the number of entries into permanent contracts would have been smooth across the 30 year threshold. The second graph represents a situation in which both job destruction and creation are taking place around the threshold. Finally, the third graph shows the case when there is no job destruction next to the threshold and the policy only stimulated hiring below it. Exploiting the 2001 reform, I can look at how the age-distribution of permanent hires adapts once the credit is removed. The idea is to identify a pattern similar to the ones just described. The 2001 change is more adequate for that purpose because the only change across the 30th threshold was the removal of the credit for workers under 30. Inference based on the 1997 change is more complicated because a credit for long-term unemployed workers was also introduced.[33]

Figure 13a shows the raw hiring data before and after each policy change. As is visually evident, the removal of the tax credit for workers younger than 30 led to a convergence of the hiring distribution only from below 30, as illustrated in the hypothetical figure 12c. Hiring above 30 does not show any jump upwards as expected with displacement effects. In order to translate the discussion into numbers, I estimate difference-in-difference coefficients for each age bin. The specification is:

$$y_{it} = \alpha + \delta Treatment_t + \sum_{a=20}^{40} \beta_a Age_{it} + \sum_{a=20}^{40} \delta_a Age_{it} * Treatment_t + \epsilon_{it} \tag{4}$$

The omitted group are workers 20 years old or less. The coefficient δ_a is a difference-in-difference estimate of the effects of the policy for each age bin relative to the workers in the omitted group. The age bins are 3 months wide. As can be seen in figure 13b, the policy was creating jobs below 30, but was not destroying jobs above 30, relative to workers below 20 years old. Also, the effects of the policy are slightly stronger for workers between 25 and 30 years old than for younger workers.

Though the evidence so far suggests that there were no displacement effects, and that all the adjustment happened through an expansion of hiring below 30, it could still be the case that workers above 30 were more likely to separate from their employers. Figures 13c and 13d show that this was not the case. Estimates for lay-offs and quits, with respect to the age when the separation occurs, are not significantly different for the treatment (25-30) and the control group (30-35).

Hiring counterfactuals. A caveat with the strategy above is that it might be specific to the period around 2001, and not reflect substitution happening during the whole period (1997-2001) for which the policy was in place. For that reason, I construct counterfactuals of how the hiring distribution would have looked like if the threshold had been moved to the left of 30 (i.e. at 29 years) or to the right of 45 (i.e. 46). Under the assumption that workers close to each threshold are more substitutable than workers far away from the discontinuity, this method should detect a missing mass of hires after 30 and before 45.

[33] Evidence on displacement for 1997 is in the online appendix. See figure 7. The results are consistent with those for 2001.

The following is a description of the details of the estimation. I only explain the case for young workers, but the case for the prime-age workers is symmetric. c_j is the log number of individuals in bin j in my main specification. I group individuals into age bins indexed by j. Each bin is 3 months wide. To construct the counterfactual I run the following specifications:

$$c_j = \sum_{i=0}^{p} \beta_i (a_j)^i + \sum_{i \geq a_L}^{a_U} \gamma_i \mathbb{1}[a_j = i] + v_j \tag{5}$$

a_j is the age at bin j, p is the order of the polynomial, which is 1 for the preferred specification.[34] a_L and a_U are the lower and upper bounds of the area that is not used to construct the counterfactual.

The counterfactual distribution is estimated as the predicted values from 5 omitting the contribution of the dummies in the excluded range:

$$\hat{c}_j = \sum_{i=0}^{p} \hat{\beta}_i (a_j)^i \tag{6}$$

Missing mass is estimated as the difference between the observed and counterfactual bin counts between the threshold (a^*) and the upper bound of the omitted area (a_U). I choose as lower and upper bound for young workers 20 and 31.5, respectively.[35] The equation for missing mass is:

$$\hat{M} = \sum_{j=a^*}^{a_U} (\hat{c}_j - c_j) \tag{7}$$

The measure of the hole is:

$$\hat{h} = \frac{\hat{M}}{\sum_{a^*}^{a_U} \hat{c}_j} \tag{8}$$

Standard errors are obtained using a bootstrap procedure. I sample residuals from equation 5 with replacement to generate many age-hire distributions. The standard errors are the standard deviation of the distribution of estimates obtained from each sample.

Results are in figure 14. As can be seen, the counterfactual between 30 and 31.5 matches very well the actual distribution of hiring. The estimate of the missing mass has the opposite expected sign and is not significantly different than 0. In contrast, the counterfactual between 43.5-45 is different from the actual hiring distribution. It detects a significant missing mass of hires of 6.77%. This evidence complements the results based on the age distribution of hires before and after the policy change in 2001. It suggests that substitution of workers older than 30 years for younger workers is not happenning, not only around 2001, but during the whole period when the policy was in place.

DD changing the control group. The counterfactual strategy fails to detect a missing mass of hires just after 30, but does so before 45. If substitution is proportionally higher next to the

[34] The rationale behind the election of a first-order polynomial is that the hiring distribution of workers between 30-45 years is highly linear.

[35] The lower bound for prime-age workers is 43.5, and the upper bound is 55.

discontinuity, but dies away smoothly as we move to older cohorts, this strategy will fail to detect substitution. A way to detect if that is the case is to repeat the DD estimation, but using several control groups: 30-31, 31-32, 32-33, 33-34, and 34-35. If substitution was happenning, we would expect to see that the estimated effects decrease as we choose as a control group older workers. This is the same strategy as the one used in Blundell et al. (2004).

Results for both policy changes are in table 7. The estimates for new hires using different control groups are very stable. Moreover, they are not significantly different from each other and are not decreasing as we move away from 30. The coefficients for lay-offs and quits show a similar picture.

Additional evidence on displacement. The evidence points to workers older than 30 not being affected by substitution. I perform some additional robustness checks. The specifications and results are detailed in section 5 in the online appendix. First, individuals born in March 1971 crossed their 30th birthday when the policy was removed in March 2001. Thus, those who were still unemployed at the age of 30 cannot suffer from displacement since the policy is no longer in place. Therefore, if displacement was an issue, the changes in slope that we have detected for cohorts crossing their 30th birthday between May 1997 and March 2001 should be smaller for the March 1971 cohort relative to similar cohorts born in the previous year. However, the findings in tables 5-7 in the online appendix rule out that possibility.

The second strategy looks at cohorts that had already crossed the 30th birthday in March 2001. In that case, after the removal of the policy they should benefit positively since they will not suffer from displacement any more. Therefore, we should detect a positive change in the evolution of their employment slope after March 2001. Results are in tables 8 and 9 in the online appendix and I do not find evidence of a positive rebound.

Finally, I plot the flows in and out of permanent contracts around each policy change. If workers in the control group are substituted for workers in the treatment group, we should observe that their monthly number of hires shifts down after May 1997 (up after March 2001). The graphs are displayed in figure 6 in the online appendix and do not show evidence consistent with substitution.

Overall, the evidence rules out the presence of substitution effects. Thus, the employment estimates reflect net job creation, and do not confound positive and negative employment effects on the treated and non-treated, respectively. It is important to keep in mind that there were also non-age related employment credits targeting other disadvantaged groups such as the long-term unemployed. Such schemes are probably the reason why the policy is successful at stimulating employment below 30, while not creating significant negative employment effects above 30.

3.4 Windfalls

The policy reduced the employer's payroll tax for new permanent hires. However, some hires would have happened in any case and employers receive a windfall of money for them. In this section, I measure to what extent the tax cut for new hires generates windfalls. This is a key estimate to perform a cost-benefit analysis (see section 3.5).

The data distinguishes between subsidized permanent contracts and non-subsidized ones. Then, I can measure the extent to which there is crowding-out of non-subsidized permanent contracts after the introduction of the subsidized contract. The ratio between crowded-out jobs and new subsidized jobs will tell us the fraction of new subsidized employment that would have happened in any case. Note that since for young workers there is an increase in employment, this ratio will be below 1.

I perform a difference-in-difference analysis for each type of contract exploiting the 1997 and 2001 reforms. The upper panel of table 8 reports the windfall effects for employment variables. In 1997, the crowd-out of permanent not subsidized employment of young workers accounts for 76% of the increase in subsidized permanent employment. However, in 2001 the crowd-out had decreased to 46%. The drop in the number of windfalls is consistent with regulatory changes in the administration of the employment credits. After May 1999, firms that wrongfully dismissed a worker hired with a tax credit cannot rehire somebody with a tax credit for a year or for as many subsidized workers who were wrongfully dismissed. Therefore, the administrative change might make employers more careful when they dismiss a permanent worker to hire a subsidized one, because in case the latter match does not turn out to be productive, they might not be able to benefit again from employment credits. Consistent with no prime-age employment creation, the windfall above 45 accounts for all of the increase in subsidized permanent employment.[36] Finally, figure 15 confirms that the estimates are not due to pre-treatment trends.

3.5 Cost Benefit Analysis

Although the results in the paper suggest that the policy was successful in increasing employment of young workers, it might have come at large costs for the government. In this section, I use the estimates to perform a cost benefit analysis of the policy. Payroll tax credits generate revenues

[36]The next three panels in table 8 shows the results for transitions. There is no reduction in new hires without a tax cut after the policy change in 1997 for young workers. However, when the tax cut for young workers is removed in 2001, new permanent regular contracts increase and account for 35% of the decline in subsidized contracts. The presence of windfalls for older workers is immediate after the reform in 1997. Separations of non-subsidized permanent contracts increase after the policy reform both for young and old workers. Note that the increase happens both for lay-offs and quits. The former is consistent with firms prefering to fire workers that fall into the ages of treatment to replace them with subsidized workers. The latter suggests that workers that can be hired with age-based employment credits might be aware that they have higher probability of finding a new job, and thus quit their current one. However, note that they cannot find a subsidized job in the same firm, since tax credits cannot be claimed for former permanent workers during 2 years. Note also that the removal of the policy in 2001 causes symmetric effects.

for the government by inducing behavior that would not have happened absent the policy. There are two types of behavioral responses: jobs that would not have been created otherwise (B_1) and decreases in UI recipients (B_2). However, the policy also incurs in mechanical costs (M): jobs that would have existed in any case and that now receive a tax credit. Once one knows the effect on the government budget of both behavioral and mechanical responses, one can estimate the Marginal Efficiency Cost of Funds (MECF), or the ratio of the cost to taxpayers of the government increasing taxes relative to the value of the additional revenue received by the government (Slemrod and Yitzhaki, 2001).

Table 9 shows the main variables that are needed to estimate whether the policy was cost-efficient or not. "Sample" is the mean number of individuals within each group of workers in the dataset. "Population" is the translation of "Sample" into the population following the construction of the MCVL dataset (elevated by 25). "Mean Wage" and "Mean Length" are the average wage and duration for each type of contract and for the relevant age group. Revenue is the estimated money accrued or lost by the government according to the following equations:

$$B_1 = Revenue_s = N_s \text{ x } W_s \text{ x } \tau_f(1-t) \text{ x } 730 + N_s \text{ x } W_s \text{ x } \tau_f \text{ x } (L_s - 730) + \\ + N_s \text{ x } W_s \text{ x } \tau_e \text{ x } L_s \tag{9}$$

$$B_2 = Savings_{UI} = N_{UI} \text{ x } W_{UI} \text{ x } (1 - \tau_e \text{ x } .65) \text{ x } L_{UI} \tag{10}$$

$$M = RevenueLoss_w = N_s \text{ x } W_s \text{ x } \tau_f(1-t) \text{ x } 730 + N_s \text{ x } W_s \text{ x } \tau_e \text{ x } L_s \tag{11}$$

$$\text{Net Revenue} = B_1 + B_2 - M \tag{12}$$

where the subscript i refers to permanent subsidized jobs (s), permanent windfall jobs (w), and unemployment insurance recipients (UI). N_i is the number of i jobs or UI claims, W_i is the mean wage in situation i, τ_f is the firm's payroll tax rate, t is the tax rate discount, L_i is the mean length of spell i, and τ_e is the employee's payroll tax rate. 730 is the number of days that the tax discount applies for young hires. Finally, the taxes paid under UI are discounted by .65 because 35% of the payments are covered by social security.

For workers under 30, the net number of jobs created was around 400,000.[37] However, around 600,000 other jobs that would have existed in any case were now receiving a tax credit. The decrease in UI receipts was around 200000. Multiplying these numbers by the mean job wage and mean job length, and by the payroll tax rate, the increase in tax collection because of new contracts was of

[37] All the magnitudes used in the cost benefit analysis are significant at the 1% level.

1220 million euros. Revenue also increased due to savings in UI by 887 million euros. Revenue lost because of mechanical responses was of 724 million euros. Overall, for the group of workers under 30, the policy is very cost efficient and increased net government revenue by 1383 million euros.

Since for workers older than 45, the policy did not increase employment or reduced the number of UI recipients, all we need to know are mechanical responses. Around 900,000 jobs that would have existed in any case were now subsidized. The policy for prime-age workers is very cost-inefficient and implied losses in revenue of 4360 million euros.

The MECF is the ratio of mechanical costs relative to the difference between behavioral responses and mechanical costs (Slemrod and Yitzhaki, 2001):

$$MECF = \frac{M}{B_1 + B_2 + M} \tag{13}$$

The MECF measures the ratio of the cost to taxpayers of funds raised to the value of the funds received by the government. The difference in value between the numerator and the denominator is caused by leakages in tax collection caused by firms that are maximizing profits in the presence of taxation. The MECF for young workers is -0.52 (.15). That implies that the payroll tax rate for workers under 30 is on the declining portion of the Laffer curve. For the group of prime-age workers, the MECF is 1. Thus, the efficiency costs of the current level of payroll taxes for prime-age workers are very low.[38]

4 Age-Specific Labor Demand Elasticities and Explanations

4.1 Age-Specific Labor Demand Elasticities

The estimates on employment and wages can be used to recover labor demand elasticities for each age group. In a partial equilibrium model, the impact on employment and wages depends on the wage elasticities of labor demand and supply for the targeted group:[39]

$$\frac{d\log w}{dt} = \frac{-\epsilon_D}{\epsilon_S + \epsilon_D} \tag{14}$$

$$\frac{d\log W}{dt} = \frac{\epsilon_S}{\epsilon_S + \epsilon_D} \tag{15}$$

$$-\frac{d\log L}{dt} = \frac{\epsilon_S \epsilon_D}{\epsilon_S + \epsilon_D} \tag{16}$$

where w is the net wage, W is the gross wage or $w(1+t)$, t is the tax rate; and ϵ_D and ϵ_S are the elasticities of labor demand and supply, respectively.

[38] Saez et al. (2012) reach a similar conclusion for a payroll tax reform that targeted high-wage, prime-age workers around the age of 38.

[39] Derivations are in appendix C. See also Salanie (2003).

Using equations 14 and 16, the null employment and wage effect in the prime-age labor market can only be rationalized as a consequence of marginal workers in that market facing a perfectly inelastic labor demand.[40] If demand had been elastic, either employment or wages would have increased, or both. However, it does not inform us about the elasticity of labor supply because the results are consistent with both elastic and inelastic supply.[41]

In the market for young workers, the increase in employment and the zero effect on wages can only be rationalized by a somewhat elastic labor demand and a very elastic labor supply ($\epsilon_D << \epsilon_S$). That at the aggregate level labor supply is very elastic is consistent with predictions of a right-to-manage union model. Recall that around 90% of private workers in Spain are covered by collective bargaining. Thus, bargained wages are above the market clearing level, there is involuntary unemployment, and an expansion in demand will happen along a very flat supply function. The perfectly elastic supply is also consistent with the large level of unemployment for workers under 30 in Spain. Thus, there should not be supply constraints for young workers. Then, we can rewrite equation 16 as:

$$-\frac{d\log L}{dt} = \frac{\epsilon_D}{1+\frac{\epsilon_D}{\epsilon_S}} \simeq \epsilon_D \tag{17}$$

as long as $\frac{\epsilon_D}{\epsilon_S} \simeq 0$. Then, the increase in employment is set by labor demand. Note also that we can combine equations 15 and 16 to get the formula for the labor demand elasticity:

$$-\frac{d\log L}{dt} = \frac{\epsilon_S}{\epsilon_S+\epsilon_D}\epsilon_D = \frac{d\log W}{dt}\epsilon_D \Rightarrow \epsilon_D = -\frac{\frac{d\log L}{dt}}{\frac{d\log W}{dt}} \tag{18}$$

where the numerator is the percentage increase in employment, and the denominator the percentage decrease in labor costs. Recall that the policy increased employment of workers aged 30 by 1.17 pp. Private sector employment at those ages is 48.39%. Thus, employment increased by 2.42%.

What is left now is to compute the reduction in labor costs caused by the tax cuts, and the overall labor costs at 30. To estimate the decrease in labor costs, I calculate the amount of taxes saved by firms during the first 2 years of job relationship in a subsidized contract.[42] I measure savings of 3 million euros for workers aged 30. However, such reduction in labor costs does not meet the definition of an aggregate labor demand elasticity: the change in employment due to a change in labor costs for all workers, both incumbents and new entrants. If the tax cut had applied

[40] The exact estimated labor demand elasticity is -0.00905, with a 95% confidence interval between [-0.032,0.014]. The employment estimate is based on column 3, panel C in table 3. Average private sector employment at 45 is 49.5%. Overall labor costs at 45 are around 2 billion euros and savings in labor costs are around 194 million euros. For more details about estimation of labor costs see the discussion of the labor demand elasticity for young workers in that same section.

[41] To make sure that supply is not playing any role in the 0 employment effect for prime-age workers, I repeat the results for men and women. Women are known to have more elastic supply. Thus, if demand had been elastic we would detect a larger employment effect. I find same results across gender, consistent with demand being inelastic. For more details see section 4.2.

[42] Recall that the tax cut for young workers applies only for the first two years of the contract.

to all workers, the reduction in labor costs would have been around 38 million euros at age 30. Overall labor costs of workers aged 30 between 1997 and 2001 were around 1 billion euros. Thus, the reduction in labor costs would have been of 3.8%. Assuming that employment would have increased by the same amount, 2.42%, with a hypothetical reduction of 3.8%, I measure a lower bound for the labor demand elasticity of -0.63, with a 95% confidence interval between [-0.8,-0.47].

There are two reason why this estimate has to be interpreted as a lower bound: first, the critical assumption is that a tax cut both for incumbents and new entrants would not have increased employment by more than 1.17 pp. Such a policy would have subsidized incumbent workers, which has no direct effects on employment. However, the lower payroll tax bill could have increased new hires by more, and this could have caused an increase in employment higher than 1.17 pp. Second, the wage data is top-coded according to the maximum base for payroll taxes. Thus, I am not including the wage cost to firms of workers who earn over the maximum base. Then, the percentage reduction in labor costs caused by the tax cut would have been smaller than 3.8%.

This is not the first paper to exploit employment tax credits to estimate labor demand elasticities.[43] In fact, the finding that labor demand elasticities are higher for younger workers than for prime-age workers is consistent with recent evidence that estimated demand elasticities for different age groups separately and in different contexts. Huttunen et al. (2013) study a subsidy for low-wage workers older than 54 in Finland. They find very small employment effects and report demand elasticities between -0.067 and -0.13. Their estimate is close to the one in this paper, but different from 0. The Finnish scheme applied to all workers older than 54, not only to new hires. The authors show that the impact is driven by decreases in the exit to non-employment, and not from entry from unemployment. Thus, their results are also consistent with old unemployed workers facing inelastic demand. Katz (1998) evaluates the Targeted Jobs Tax Credit in the US for young disadvantaged workers. He reports a demand elasticity for young workers of -0.5 under the assumption of infinitely elastic supply.[44] In a more recent paper, Egebark and Kaunitz (2014) evaluate a firm-side payroll tax cut implemented in Sweden for workers younger than 25 just before the onset of the Great Recession. Their findings point to a labor demand elasticity of -0.31.[45]

However, we should be cautious before concluding from the previous literature that labor demand elasticities decrease with age. The evidence at each age is not based on the same policy changes. Moreover, the different estimates in the literature could be specific to the characteristics of the targeted group and the context on which each program was implemented. First, Katz (1998) sample is composed of disadvantaged youth, who might face a different labor demand than the group of young workers as a whole. Second, it is a well-known fact that hours of work of young

[43] See Hamermesh (1993) for review of earlier studies on labor demand.

[44] He does not have wage data and thus cannot infer the slope of the supply function.

[45] A potential explanation for the higher elasticity estimated in this paper relative to Egebark and Kaunitz (2014) is the high level of unemployment for workers younger than 30 in Spain.

workers are more procyclical than that of prime-age men (Clark and Summers, 1981; Gomme et al., 2004). Though this could partly be due to changes in labor supply over the cycle, it could also be due to labor demand for young workers being more elastic during downturns. In fact, Jaimovich et al. (2013) show in a simulation that age-specific labor supply is not enough to account for the differential cyclicality across age groups, and that a model including age-specific labor demand does a better job. Therefore, the estimates in Egebark and Kaunitz (2014) might reflect just recessionary periods.[46] Third, the estimates might be conditional on each country's labor market institutions. Finland and Sweden do not have minimum wage laws. Instead, minimum wages are decided through collective bargaining.[47] Then, payroll tax shifting on wages might be more likely to happen. In such situation, employment credits do not decrease labor costs. That could explain the null effect in Huttunen et al. (2013).

The main advantage of the quasi-experiment in this paper is that the same policy targeted different age groups, during the same macro context, and within the same set of institutions. Then, the potential channels that could explain the heterogeneous results in the literature are shut off and we can be certain that the labor demand elasticity does decrease with age. But why is labor demand elasticity decreasing with age? In the remaining of this section, I consider several channels that could rationalize the findings.

4.2 Explanations

The decreasing demand elasticities with age are consistent with the pool of marginal workers being more adversely selected as they age. In this section, I show evidence consistent with that. I also consider several alternative explanations. I show that the results do not change when I separate by gender. Then, I focus on firm characteristics, on the possibility that payroll taxes are shifted on wages for prime-age workers, on wage capture by prime-age workers, and on hiring costs for each age group. None of these alternative channels is consistent with the results

Adverse Selection. The evidence based on the characteristics of the young and prime-age labor market (figure 5) is consistent with adverse selection increasing with age. The ratio of permanent workers does not increase after 30. Firms could infer that workers who failed to have a permanent contract before 30 are of lower ability. That signal might be strengthened over time (Greenwald, 1986). The more a worker stays out of the permanent workforce, the more likely he is to suffer unemployment spells that depreciate his skills. Then, firms might see prime-age non-permanent workers as adversely selected with certainty.

In contrast, the signal will be less strong for young workers, as firms understand that is difficult for them to land a stable job (Ryan, 2001). Younger workers might be riskier because they have

[46] Katz (1998) and Huttunen et al. (2013) focus on non-recessionary periods.

[47] For more details, see Huttunen et al. (2013) and Egebark and Kaunitz (2014).

less history in the labor market and then greater uncertainty about their productivity. But the average productivity of a risky young worker might be higher than that of an adversely selected prime-age worker.

If adverse selection is increasing with age, it should be detectable in the wages. Suppose a skill distribution for young and prime-age workers. The skill distribution should be mapped into a wage distribution. Thus, if prime-age subsidized workers are more negatively selected than their age peers, they should come from the lower end of the skill distribution. Then, their wages should also be concentrated on the left-end of the distribution. In contrast, young subsidized workers are not as negatively selected as their age peers. Therefore, their skill and wage distribution should only be slightly shifted to the left. As can be seen, subsidized and non-subsidized prime-age workers are very different (figure 16a). Prime-age subsidized workers are coming from a much less productive pool of individuals. In contrast, the average subsidized young worker is not very different than the average non-subsidized young worker (figure 16b). Thus, the analysis of the wage densities is consistent with adverse selection increasing with age.

Adverse selection should also be detectable in other observable characteristics of these workers. Thus, I can still provide an additional test. Note that both young and prime-age workers will be negatively selected with respect to their age-counterparts. But the question here is whether subsidized prime-age workers are relatively more negatively selected than young ones. To test that, I restrict the sample to workers hired as permanent workers and run a regression with a dummy equal to 1 if the worker was hired with a tax credit, and 0 if not, on several predictor and control variables. The specification is as follows:

$$Y_{ipct} = \alpha + \delta_p + \phi_c + \gamma_t + \beta X_{ipct} + \psi 45_{ipct} + \kappa 45_{ipct} X_{ipct} + \epsilon_{ipct} \tag{19}$$

Y_{ipct} is a dummy that indicates whether the individual was hired with a tax cut or not. δ_p, ϕ_c, and γ_t are province, cohort, and year fixed effects, respectively. X_{ipct} is a vector of characteristics of the worker and the job: education, sex, citizenship, disability, experience, wage, industry sector, part-time, and firm's size. 45_{ipct} is a dummy indicating that the worker is 45 years or older.

Table 10 reports the results. Column 1 shows them for workers under 30 years during the period between May 1997 and March 2001 (when the tax credit for young employees was available). Column 2 reports the estimates for prime-age workers 45-50 years old between May 1997 and June 2006. Column 3 reports the results pooling both types of workers and adding interactions for those older than 45. The estimates are consistent with adverse selection worsening with age: both young and prime-age subsidized workers are worse in several observable characteristics (university education, experience during the last 12 months, firm's size, and wage) than their counterparts. Moreover, the interactions reveal that subsidized prime-age workers are much more negatively selected than young employees in three dimensions: they are less likely to have finished a college degree, they have accumulated less experience during the last 12 months, and their wages are lower.

However, they are more likely to work in slighty larger firms.

The above evidence, and the evolution of employment over age (figure 5), are consistent with adverse selection increasing with age in the pool of marginal workers. However, there can be other reasons that could explain the decreasing elasticity between the ages of 30 and 45. I turn now to consider some of the most plausible ones.

Supply of Men and Women. I begin by showing further evidence that the elasticity of labor supply does not play any role in explaining the results for prime-age workers. Women are known to have more elastic labor supply (Eissa and Liebman, 1996; Blundell and MaCurdy, 1999). Figure 1b shows the unemployment rate for men and women aged 45-50. Since unemployment is higher among women, it suggests that female labor supply should be more elastic in Spain too. Thus, employment responses should be higher for women, as long as demand is elastic. I repeat the analysis for men and women separately. The evidence is presented in section 8 in the online appendix. The results across gender are very similar and not significantly different. Therefore, it confirms previous results that the key driver of the null employment effect in the prime-age labor market is an inelastic demand.

Payroll taxes are shifted on wages. If workers value the benefits that are financed through payroll taxation, they will accept a lower wage in compensation for these services. The argument can be traced back to Summers (1989) for mandated benefits and Lazear (1990) for severance payments. In a perfect market, any government-ordered transfer from the firm to the worker can be offset by a voluntary transfer of the same size from the worker to the firm. Then, payroll taxes can be fully shifted onto wages, and employment credits will not affect the level of employment. Gruber (1997) shows that this is the case when there is full valuation of benefits financed by payroll taxes.

However, the presence of minimum wages breaks down the argument. Firms will not be able to fully shift payroll taxes to wages for workers who are in the proximity of the minimum wage. Thus, a potential explanation for the results is that young workers are more likely to be at the minimum wage than prime-age workers. If that is the case, we would expect that younger workers hired with a tax cut are more likely to be at the minimum wage than prime-age workers. Figure 16 shows the wage density for each age group during the period 1997-2001 for permanent and temporary contracts (16c), for all permanent contracts (16d), and for permanent subsidized contracts (16e). The red dashed lines represent the area of the minimum wage. Figures 16c and 16d do show that the density of minimum wage workers is larger for younger workers. If the increases in employment were caused by that factor we would expect that such higher mass also shows up for permanent subsidized contracts. However, figure 16e shows that the wage density for subsidized contracts is very similar across age groups. Thus, differential incidence of minimum wage workers does not seem to explain the results.

Small v. Large Firms. One possibility is that there is no impact on prime-age workers because they are not working in small firms. Small firms are likely to be more liquidity constrained than large firms. Thus, they might not be able to hire workers at the occupation-specific wage given the burden of payroll taxes. Then, the increase in demand might be driven by them. In addition, hiring policies in large firms might be directed by a human resources department. The age requirement of the tax credits might impose an additional transaction cost on the human resources department that deters them from using the employment credits. In fact, Cahuc et al. (2014) study a payroll tax credit for new hires in small French firms during the Great Recession and find increases in employment growth.

I repeat the analysis for small and large firms. The RDD results are in table 11. For young workers, the probability of being hired in a small firm before the threshold is significantly .28 pp higher. In large firms, the estimate is not significant. For prime-age workers, both in small and large firms the estimate is significant. However, note that the increase in hiring is of .37 pp for small firms, whereas it is only of .07 pp for large firms. Then, it is mostly small firms that are delaying the time at which they hire prime-age workers to claim the tax credit. This is not consistent with small firms not hiring prime-age workers and cannot justifiy the inelastic labor demand for older workers.

Another potential interpretation of this result is that informal workers are more likely to be at small firms. However, the effects of the policy happen mostly through workers that were temporary within the same firm before becoming permanent. If firms were trying to save taxes by keeping workers underground, there is no rationale for hiring them as temporary before, since those contracts are not subsidized and have to pay the standard level of payroll taxes. Therefore, this finding signals to another relevant margin through which the policy is acting. The increases in employment happen for young workers in small firms.

Search-and-matching. *(1) Wage capture.* The introduction of rigidities in the labor market highlights some channels that could explain the differential elasticities across groups. Two of the equations determining the equilibrium in such models are the job creation condition (Pissarides, 2000):

$$p - w(1+\tau) - \frac{(r+\lambda)pc}{q(\theta)} = 0 \tag{20}$$

and the wage function:

$$w = (1-\beta)z + \frac{\beta p(1+c\theta)}{1+\tau} \tag{21}$$

where p is the productivity, w is the wage, τ is the firm's payroll tax rate, r is the interest rate, λ is the rate of arrival of an adverse shock that breaks the job match, c is the hiring cost; θ is the ratio between vacancies and unemployed workers, and represents labor market tightness; $q(\theta)$ is the rate at which vacant jobs become filled, β is the worker's bargaining power, z is the income that an unemployed worker receives.

A decrease in the payroll tax rate will increase job creation as long as gross wages, $w(1+\tau)$, also decrease. Gross wages will stay the same if workers are able to capture the rent created by the lower tax rates. That is the case when workers hold all bargaining power, or $\beta = 1$. If prime-age workers have all bargaining power that could explain the zero effect on employment.

Inspection of the wages of subsidized workers provides an opportunity to test this channel. Employers hiring workers with a credit before the age of 45 do so by claiming the LTU credit. The tax cut for those cases is smaller than that after 45 (figure 2b). Moreover, the LTU credit applies only for the first 2 years, while that for workers older than 45 applies for the whole duration of the contract. Thus, if subsidized prime-age workers are capturing the rent, we should see a jump up in their wages after 45. For young workers, the tax cut is not so different as that for the LTU: both apply for 2 years and in 1997 and 1998 the tax credit was exactly the same (figure 2a). Thus, there should not be a jump in wages at 30. Graphical evidence in figure 17 shows a jump in the starting wage for workers hired with a wage subsidy after 45. As expected, there is no jump at 30. However, the jump at 45 could be caused by selection of workers across the discontinuity. I will use a RDD, but adding control variables to correct for selection, in order to test if the jump is causal. The specification is:

$$y_{iptq} = \alpha + \beta_1 age^*_{iptq} + \gamma X_{iptq} + f(a - age^*) + \delta_p + \rho_t + \phi_q + \epsilon_{iptq} \tag{22}$$

where y_{iptq} is the log real daily wage, age^*_{iptq} is a dummy indicating that the employee was hired after his 45th birthday, or before his 30th birthday. $f(a - age^*)$ is a local linear polynomial in age on each side of the threshold. X_{iptq} are control variables such as education, sex, previous wage, experience, disability, part-time job, firm's workforce, a dummy indicating that the worker was short-term within the same firm that hired him as permanent with a tax credit, and the local unemployment rate when hired. In addition, I control for province fixed effects, δ_p; ρ_t, calendar month fixed-effects; and ϕ_q, quarter fixed-effects for the moment when the worker was hired with a tax credit.

Table 12 reports the results for the RDD. The first three columns show it for young workers, and the last three for prime-age workers. I show the results without control variables, adding control variables and fixed-effects, and finally including a dummy indicating that the worker had been a short-term employee within the same firm before becoming permanent. The estimates of specifications not including a dummy indicating that the worker had been a short-term employee within the same firm are positive and significant. However, the coefficient is not significant anymore when I include this control variable. Therefore, there is no evidence that prime-age workers hired with subsidized contracts are capturing the tax cut in terms of higher wages, ruling out the wage bargaining hypothesis. Moreover, for prime-age workers the tax credit acts as a transfer to firms.[48]

[48] Changes in the generosity of the employment credit in May 1999 provide an additional test. I focus only on male

[49]

(2) Hiring costs. Another possibility is that prime-age workers were more costly to hire because they were harder to find, given their high rates of employment. Figure 5c suggests that hiring costs are likely to be lower for young workers as a group. However, note that the ratio of the workforce in permanent contracts at 30 and 45 barely changes. Thus, hiring costs just before the 30th threshold and at 45 should be very similar. Then, we should not observe a positive impact just before 30 either. Recall that the age-incidence estimates of the employment effect show a positive and significant impact just before 30 (figure 13b). And that the effect at 30 is not significantly different than the impact between 20 and 29 years. Then, an explanation based on hiring costs fails to explain the results.

To sum up, the evidence is consistent with adverse selection increasing with age in the pool of marginal workers. Thus, as adverse selection worsens over the life-cycle, the labor demand elasticity of -0.63 for young workers decreases until reaching 0 at some point between the ages of 30 and 45. The fundamental characteristics of the labor market that point to adverse selection as the main explanation of the results are not only characteristic of Spain. Blundell et al. (2013); Topel and Ward (1992); Murphy and Welch (1990) document similar features in other labor markets of developed countries. Then, the implications for labor policy design are likely to hold in other contexts.

5 Discussion

The labor demand elasticity is a key parameter to evaluate and predict the impacts of both demand and supply interventions in the labor market. In this section, I discuss policy implications of the results, interpret earlier findings in light of the estimates in this paper, and suggest avenues for future research.

Demand-Side Policies. *(1) Employment Tax Credits and the Optimal Age-Profile of Payroll Taxes.* Employment tax credits have been considered as a policy to increase employment for a long-time (Kaldor, 1936; Phelps, 1994, 1997). They are used in many countries around the world.

workers because it is for them that the generosity of the employment credit decreased unambiguously: the payroll tax rate cut decreased from 40% to 35% for the first year, and from 40% to 25% in the second year, for male workers under 30. For the case of male workers over 45, it decreased from a 60% tax cut for the first two years, to 45% and 40% for the first and second year, respectively. Moreover, for the remaining of the contract it decreased from 50% to 40%. I perform a DD strategy. Table 10 in the online appendix reports the results. The results are consistent with those of the RDD: none of the estimates are significant when I include all the control variables.

[49]It could still be the case that prime-age workers hired with a tax credit do not perceive higher wages, but firms grant them more non-pecuniary benefits. This is unlikely for two reasons: first, non-pecuniary benefits are also determined through collective-bargaining. Second, higher non-wage benefits for subsidized workers might undermine the morale of non-subsidized workers who had been at the firm for a long time.

Most of them target either young or old workers (or both) (OECD, 2009, 2013).[50] [51] The demand elasticities in this paper show that tax credits for new hires will be effective and cost-efficient for young workers, but not for prime-age workers. That could increase employment by smoothing the school-to-work transition period (Ryan, 2001), and by subsidizing hiring of risky young workers with yet unknown productivity and who have to be trained. Moreover, intervention in the early labor market years can improve employment outcomes as these workers age by reducing the pool of adversely selected workers. More experience during the early labor market years can have positive effects later on.

Yet, the fact that many countries target these policies to the population of older workers is telling about the challenges that economies face in an aging society. The estimates at 45 might not be valid for older workers who are closer to the retirement age. Blundell et al. (2013) show, for the UK and France, that workers over 55 years play a big role in explaining reductions in employment and hours of work during the last 30 years. Huttunen et al. (2013) study a payroll tax cut for all low-wage workers older than 54 in Finland. Their findings show that new entries were not affected, which is in line with the results in this paper for 45 year olds. However, they found a decrease in exits to non-employment, with a small impact on the employment of workers older than 54. The positive effect could be due to this population being less adversely selected since they are currently working. Therefore, the evidence suggests that employment tax credits might also be benefitial if targeted at workers who are much closer to retirement than those who are 45 years old. But this is as long as the tax cuts target the currently employed and not new hires.

Perhaps a simple way to implement a reform according to the results in this paper and in Huttunen et al. (2013), would be to make payroll taxes age-dependent. The optimal payroll tax rate should be lower for young workers, and then increase with age. Since the labor demand elasticity for marginal workers might reach 0 at some point between the ages of 30 and 45, the plateau of the tax rate should be at some point between those two ages. An interesting question for future research is where exactly the plateau should be, but this is beyond the scope of this paper. Following the results in Huttunen et al. (2013), the payroll tax rate should start decreasing again

[50] 12 OECD countries have provisions that target young or old workers. Belgium, France, Japan, Portugal, and the US have specific policies both for young and old workers. Canada, Greece, Italy, South Korea, Turkey and the UK have specific policies for young individuals. Poland has an employment credit for old workers. Neumark and Grijalva (2013) also report that, between 1969 and 2012, US states implemented 149 employment credits.

[51] Another common design is to focus on low-wage workers regardless of their age. Kramarz and Philippon (2001) study payroll tax subsidies for low-wage workers in France and find a small and insignificant impact on entry from non-employment. Crepon and Desplatz (2001) study the same policy and conclude that most employment effects happen through substitution between treated and non-treated workers. Yet another design is to subsidize jobs in small firms. Cahuc et al. (2014) study a payroll tax credit for new hires in small French firms during the Great Recession. They find evidence that employment growth increased and that substitution was not happening. The results in this paper confirm that targeting small firms is another important margin, since the employment effect for younger workers happens only through small firms.

at some point before the age of retirement to prevent some of these workers from losing their jobs.

The development of the optimal age-profile of payroll taxes is an interesting avenue for future research. There are two recent strands of literature that can shed some light to this problem. First, there are studies on the optimal age profile of income taxes (Weinzierl, 2011) and employment protection (Cheron et al., 2011). Second, there is research analyzing the optimal level of payroll taxes and UI over the business cycle (Landais et al., 2014; Jung and Kuester, Forthcoming).[52] While labor demand elasticities can change depending on economic conditions, the findings in this paper show that an important factor for optimal labor policy is age. Future research can provide theoretical fundamentals of how an age-dependent payroll tax should be. In addition, it can look at the optimal policy mix for different age groups.

Supply-Side Labor Market Policies. *(1) Earned Income Tax Credit.* Given the demand elasticities in this paper, EITC would not be an attractive work-encouraging transfer program for neither young nor older workers. The optimal transfer program resembles the EITC when supply responses are concentrated along the extensive margin (Saez, 2002). But most optimal tax analysis and EITC discussions assume that labor demand is perfectly elastic and thus that the incidence of taxes is only borne by workers. Rothstein (2010) simulates the impacts of the EITC with different labor demand elasticities. With infinitely elastic demand, EITC is successful in raising both employment and earnings of low-skill mothers. He estimates that incomes of low-income mothers would rise by $1.39 for every $ spent on the program.

However, assuming more realistic labor demand elasticities of -0.3, Rothstein (2010) finds more modest positive employment effects, and that a substantial portion of low-skill single mother's EITC payments is captured by employers through reduced wages: $1 in EITC spending increases after-tax incomes by $0.73. Importantly for distributional reasons, the $0.73 estimate combines an increase of net-of-tax incomes of women with children of $1.07 for each $ on the program, and a decline of $0.34 in the net-of-tax income of women without children.[53] The estimates of labor demand elasticity in this paper question the ability of EITC schemes as a tool for income redistribution. An alternative is the use of a Negative Income Tax (NIT), but the fear is that it would strongly discourage work. Instead, payroll tax credits are at least successful in raising both employment and earnings of young workers.

(2) Job-Search Assistance. The results in this paper show that employment tax credits can be a more efficient way of raising both employment and earnings than job-search assistance. They can also help us understand why job-search assistance interventions fail in general. Heckman et al.

[52] Landais et al. (2014) look at the optimal level of UI over the business cycle. Jung and Kuester (Forthcoming) study the optimal mix of payroll taxes, UI and employment protection over the business cycle.

[53] There is empirical evidence to support Rothstein (2010) analysis. Leigh (2010) exploits variation in state EITC supplements and finds that a 10% increase in the generosity of EITC is associated with a 5% fall in the wages of high school dropouts. This number implies a labor demand elasticity of -.3.

(1999) and Card et al. (2010) review the literature on active labor market policies and conclude that these interventions (1) have, at best, very small positive effects; (2) have very heterogenous effects depending on age and gender; (3) have no effect when targeted at young workers.[54]

The latter conclusion might seem puzzling in light of the findings in this paper. However, youth unemployment is high across all OECD countries (OECD, 2002, 2013). Acting through the supply-side might then have very small marginal effects (Michaillat, 2012). Moreover, since supply-side policies do not alter firm's hiring incentives, any positive estimates might only reflect partial equilibrium changes. In a research design that can look at general equilibrium effects, Crepon et al. (2013) find zero net employment effects of job-search assistance. In the case of prime-age workers, lack of employment effects of these policies can be caused by them facing an inelastic demand. If job-search assistance or training treatments are not strong enough to undo adverse selection, these policies will fail. Moreover, participation in these programs might stigmatize workers, worsening adverse selection.

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[54] Both surveys review also employment tax credits and find mostly zero effects. They argue that this is because treated individuals are very disadvantaged.

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A Tables

Table 1: Summary Statistics

	Age 25-30			Age 30-35			Age 40-45			Age 45-50		
Variables	**Mean**	**sd**	**N**	**Mean**	**sd**	**N**	**Mean**	**sd**	**N**	**Mean**	**sd**	**N**
% Men	53.49	49.88	151,514	53.93	49.85	137,224	55.52	49.69	94,780	58.72	49.23	77,832
% Citizen	81.01	39.22	151,514	85.2	35.51	137,224	91.14	28.41	94,780	94.34	23.11	77,832
% University Education	21.59	41.14	151,514	22.27	41.6	137,224	24.52	43.02	94,780	25.41	43.54	77,832
% Secondary Education	57.54	49.43	151,514	54.97	49.75	137,224	49.41	50	94,780	45.65	49.81	77,832
% Primary Education	20.87	40.64	151,514	22.76	41.93	137,224	26.06	43.9	94,780	28.94	45.35	77,832
% Perm.	20.29	40.22	151,514	27.8	44.8	137,224	33.31	47.13	94,780	36.1	48.03	77,832
% Temp.	20.61	40.45	151,514	15.57	36.26	137,224	11.06	31.37	94,780	9.996	29.99	77,832
% Self-Emp.	5.25	22.3	151,514	8.667	28.13	137,224	13.95	34.65	94,780	17.12	37.67	77,832
% UI	3.941	19.46	151,514	4.66	21.08	137,224	3.862	19.27	94,780	3.901	19.36	77,832
% Public	1.297	11.31	151,514	3.139	17.44	137,224	5.177	22.16	94,780	4.652	21.06	77,832
% Non-Emp.	52.55	49.93	151,514	44.82	49.73	137,224	36.5	48.14	94,780	32.13	46.7	77,832
% Part-Time	54.4	49.81	151,514	44.6	49.71	137,224	35.95	47.99	94,780	33.31	47.13	77,832
Log Firm Workforce	2.192	3.219	77,860	2.356	3.305	82,108	2.19	3.191	63,839	2.077	3.166	55,850
Daily Wage	37.17	39.84	70,123	42.47	42.71	74,318	46.99	43.06	58,254	47.26	35.63	50,963
% Agriculture	3.08	17.28	151,514	3.79	19.1	137,224	4.467	20.66	94,780	5.454	22.71	77,832
% Industry	8.467	27.84	151,514	9.356	29.12	137,224	12.31	32.85	94,780	14.15	34.85	77,832
% Construction	4.049	19.71	151,514	4.595	20.94	137,224	4.977	21.75	94,780	5.543	22.88	77,832
% Services	32.83	46.96	151,514	38.12	48.57	137,224	40.81	49.15	94,780	40.64	49.12	77,832

Notes: the table shows summary statistics for workers in 1997. I group the workers by 5 year age groups, and report descriptive statistics for the main groups that I will use in the statistical analysis: 25-30, 30-35, 40-45, and 45-50. I include workers' personal characteristics (sex, education, and Spanish citizenship) and job status (whether they work part-time, the size of the firm in which they work, their daily wage, and sector in which they work).

Table 2: Payroll Tax Base and Tax Rates for 1997

Worker Group	Minimum Base (monthly)	Maximum Base (monthly)	Employers Tax Rate	Workers Tax Rate	Total Tax Rate
Engineers and university graduates	697.23	2360.17	23.6	4.7	28.3
Technical engineers	578.23	2360.17	23.6	4.7	28.3
Chief administrative	502.69	2360.17	23.6	4.7	28.3
Non-graduated assistants	467.17	2360.17	23.6	4.7	28.3
Administrative officials	467.17	1936.64	23.6	4.7	28.3
Subordinate employees	467.17	1936.64	23.6	4.7	28.3
Administrative assistant	467.17	1936.64	23.6	4.7	28.3
First- and second-order officials (1)	15.90	64.55	23.6	4.7	28.3
Third-order officials (1)	15.90	64.55	23.6	4.7	28.3
Labourer (1)	15.90	64.55	23.6	4.7	28.3
Employees under 18 years (1)	15.90	64.55	23.6	4.7	28.3

Notes: Source is Ministerio de Empleo y Seguridad Social (MESS), 2012. (1): daily tax base. Worker group refers to the different contribution groups as established by Social Security law.

Table 3: Effects on Transitions and Employment of Crossing the 45th Birthday Threshold

	(1) Perm.	(2) Short-Term	(3) Self-Employed	(4) UI	(5) Public Worker	(6) —
Panel A: Effects on Transitions, Short-Run, RDD						
Treatment	0.191**	0.108	0.0356	-0.0677	0.00345	—
	(0.0723)	(0.0711)	(0.0294)	(0.0499)	(0.0498)	—
Observations	458797	458797	458797	458797	458797	—
Panel B: Effects on Transitions, Long-Run, RDD						
	Perm.	Short-Term	Self-Employed	UI	Public Worker	
Treatment	0.222***	-0.0521	-0.000497	0.00150	0.00812	—
	(0.0192)	(0.0336)	(0.00817)	(0.0215)	(0.00899)	—
Observations	4169416	4169416	4169416	4169416	4169416	—
	Perm.	Short-Term	Perm. or ST	Self-Employed	Public Worker	Non-Employed
Panel C: Effects on Employment, RDD						
Treatment	0.453***	-0.410***	0.0428	-0.00625	-0.000474	-0.0360
	(0.0410)	(0.0549)	(0.0557)	(0.0219)	(0.0111)	(0.0572)
Slope Before 45	0.0386***	0.0372***	0.0758***	0.0289***	0.0153***	-0.120***
	(0.00496)	(0.00632)	(0.00661)	(0.00313)	(0.00156)	(0.00674)
Slope After 45	0.0922***	-0.0625***	0.0297***	-0.00528	-0.00123	-0.0232**
	(0.00791)	(0.0101)	(0.0104)	(0.00468)	(0.00236)	(0.0106)
Observations	3660875	3660875	3660875	3660875	3660875	3660875
Panel D: Effects on Employment, Donut RDD						
Treatment	0.0605	-0.251	-0.191	0.0235	0.0565	0.111
	(0.146)	(0.178)	(0.186)	(0.0844)	(0.0408)	(0.190)
Slope Before 45	0.0832***	0.00546	0.0886***	0.0255***	0.0169***	-0.131***
	(0.00546)	(0.00683)	(0.00713)	(0.00336)	(0.00164)	(0.00725)
Slope After 45	0.0229***	-0.0111	0.0119	-0.00224	-0.00531**	-0.00433
	(0.00779)	(0.00937)	(0.00973)	(0.00458)	(0.00223)	(0.00986)
Observations	3660875	3660875	3660875	3660875	3660875	3660875

Notes: The estimates have been multiplied by 100 so that they can be interpreted as effects in percentage points. Panel A and B report the effects on entries into permanent, short-term, self-employed, or public jobs, as well as into UI, around the 45th birthday thresholds. Panel A focuses on the short-run effects (first 12 months after policy). Panel B focuses on the long-run effects (from the 12th month until policy is removed). Panels C and D display the effects on the probability of being employed in any of these contracts and the overall probability of working. The specification is equation 1. The dependent variables are dummies indicating each type of transition (panels A and B), or the employment status (panels C and D). The estimation window is of 24 months on each side of the threshold. Panel D performs a donut RDD omitting the 12 months before and after the 45th birthday. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Effects on Starting Wages, 1997

	Prime-Age, 1997 Change			Young, 1997 Change		
	(1)	(2)	(3)	(4)	(5)	(6)
	Starting Wage	Starting Wage	Starting Wage	Starting Wage	Starting Wage	Starting Wage
Treatment	-0.00938	0.0112	0.00108	-0.0444***	-0.0517***	-0.0207***
	(0.00998)	(0.00853)	(0.0108)	(0.00604)	(0.00492)	(0.00687)
Post	0.0404***	0.0509***	0.0486***	0.0544***	0.0552***	0.0560***
	(0.00737)	(0.00595)	(0.00612)	(0.00545)	(0.00457)	(0.00477)
Treatment x Post	-0.0227**	-0.0177*	-0.00386	0.00351	-0.00692	0.000900
	(0.0111)	(0.00947)	(0.00916)	(0.00789)	(0.00658)	(0.00590)
Controls	N	Y	Y	N	Y	Y
Prov FE	N	N	Y	N	N	Y
Cohort FE	N	N	Y	N	N	Y
Calendar quarter FE	N	N	Y	N	N	Y
Occupation FE	N	N	Y	N	N	Y
Observations	15666	15666	15666	41564	41564	41564

Notes: The table displays the effects on wages of workers hired as permanent workers of the expansion of payroll tax cuts in 1997. The specification is equation 2. The dependent variable is log daily real wages. Columns 1-3 report the results for prime-age workers, and columns 4-6 for young workers. Columns (1) and (4) do not include control variables. Columns (2) and (4) include as control variables sex, education, experience, firm size, sector, citizenship, part-time job, and workers' disability. Columns (3) and (6) also include calendar quarter, province, cohort and occupation fixed effects. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Effects on Transitions and Employment, 25-30 Vs. 30-35, 1997

Effects on Transitions

	(1) Perm. Hires	(2) Perm. Lay-Offs	(3) Perm. Quits	(4) Temp. Hires	(5) Temp. Lay-Offs	(6) Temp. Quits	(7) Trans. Temp. to Perm.
Treatment	0.307***	0.0721***	0.0417***	1.784***	1.910***	0.320***	0.130***
	(0.0271)	(0.0127)	(0.00676)	(0.103)	(0.0346)	(0.0135)	(0.0109)
Post	0.349***	0.423***	0.187***	0.520***	-0.0541*	0.133***	0.260***
	(0.0216)	(0.0144)	(0.00803)	(0.0335)	(0.0327)	(0.0130)	(0.00821)
Treatment x Post	0.347***	-0.0458**	0.0820***	0.372***	0.315***	0.146***	0.179***
	(0.0300)	(0.0191)	(0.0112)	(0.0556)	(0.0472)	(0.0195)	(0.0141)
Observations	3693279	3693279	3693279	3693279	3693279	3693279	3693279

Effects on Employment

	Perm.	Short-Term	Perm. or ST	Self-Emp.	Public	UI	Employed
Treatment	-2.347***	6.275***	3.928***	-2.296***	-1.379***	0.213***	0.253***
	(0.0548)	(0.0519)	(0.0499)	(0.0345)	(0.0244)	(0.0296)	(0.0363)
Post	1.492***	0.0200	1.512***	-0.348***	-0.225***	-0.610***	0.938***
	(0.0565)	(0.0499)	(0.0510)	(0.0367)	(0.0275)	(0.0290)	(0.0349)
Treatment x Post	0.217***	0.185***	0.402***	-0.0441	-0.0189	-0.169***	0.339***
	(0.0745)	(0.0706)	(0.0669)	(0.0466)	(0.0326)	(0.0381)	(0.0479)
Observations	3693279	3693279	3693279	3693279	3693279	3693279	3693279

Notes: The estimates have been multiplied by 100 so that they can be interpreted as effects in percentage points. The treatment group are workers aged 25-30. The control group are workers aged 30-35. In the upper panel, the dependent variable is a dummy indicating whether a transition in or out of a permanent contract (columns 1-3) or a temporary contract (columns 4-6) happened. In column 7th the dependent variable is a dummy that indicates that the worker was converted to permanent, from a temporary contract, within the same firm. The lower panel reports the results for employment. Each dependent variable is one of the possible employment statuses. The policy change is the expansion of payroll tax cuts in 1997. The specification is equation 2. Control variables included are calendar quarter dummies, sex, education, experience, firm size, sector, citizenship, part-time job, and workers' disability. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Effects on Transitions and Employment of Crossing the 30th Birthday Threshold

	(1)	(2)	(3)	(4)	(5)	(6)
	Perm.	Short-Term	Self-Employed	UI	Public Worker	—
Panel A: Hires, Short-Run, 05/1997-05/1998						
Treatment	0.108*	0.00978	0.0110	-0.00439	0.0202	—
	(0.0618)	(0.0732)	(0.0228)	(0.0697)	(0.0331)	—
Observations	748551	748551	748551	748551	748551	—
Panel B: Hires, Long-Run, 06/1998-03/2001						
Treatment	0.143***	-0.0335	0.00972	-0.0237	0.00414	—
	(0.0415)	(0.0544)	(0.0152)	(0.0344)	(0.0185)	—
Observations	1965552	1965552	1965552	1965552	1965552	—
	Perm.	Short-Term	Self-Emp.	UI	Public	Employed
Panel C: Emp., Treated Cohorts Born 05/1968-03/1971						
Treatment	0.247**	-0.301*	0.00492	0.0201	0.0217	-0.0270
	(0.119)	(0.153)	(0.0395)	(0.0618)	(0.0175)	(0.144)
Slope Before 30	0.297***	-0.0908***	0.0501***	-0.0186***	0.0277***	0.284***
	(0.0123)	(0.0123)	(0.00309)	(0.00519)	(0.00164)	(0.00660)
Slope After 30	-0.0930***	0.0404**	-0.00789	0.0261***	-0.00453**	-0.0650***
	(0.0192)	(0.0158)	(0.00567)	(0.00614)	(0.00208)	(0.00832)
Observations	4240362	4240362	4240362	4240362	4240362	4240362
Panel D: Emp., Placebo Cohorts Born 03/1973-03/1976						
Treatment	0.103	0.221	-0.0181	-0.123	-0.00548	0.300
	(0.116)	(0.254)	(0.0435)	(0.0820)	(0.0212)	(0.269)
Slope Before 30	0.187***	-0.0275*	0.0683***	0.0287***	0.0363***	0.264***
	(0.00824)	(0.0144)	(0.00309)	(0.00500)	(0.00154)	(0.0166)
Slope After 30	0.0139	0.0126	-0.0160***	-0.0276***	-0.0135***	-0.00302
	(0.0148)	(0.0214)	(0.00497)	(0.00664)	(0.00224)	(0.0263)
Observations	4824197	4824197	4824197	4824197	4824197	4824197

Notes: The estimates have been multiplied by 100 so that they can be interpreted as effects in percentage points. Panel A and B report the effects on entries into permanent, short-term, self-employed, or public jobs, as well as into UI, around the 30th birthday thresholds. Panel A focuses on the short-run effects (first 12 months after policy). Panel B focuses on the long-run effects (from the 12th month until policy is removed). Panels C displays the effects on the probability of being employed in any of these contracts, and the overall probability of working, for treated cohorts born between 05/1968-03/1971. Panels D repeats the exercise in panel C but for a placebo cohort: workers born between 03/1973-03/1976. The specification is equation 1. The estimation window is of 12 months on each side of the threshold. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Substitution Test Changing Control Group

	1997 Change			2001 Change		
	(1)	(2)	(3)	(4)	(5)	(6)
	Hires	Lay-Offs	Quits	Hires	Lay-Offs	Quits
Control Group: 30-31						
Treatment	0.212***	0.0171	0.0357***	0.693***	0.0165	0.158***
	(0.0456)	(0.0218)	(0.0112)	(0.0386)	(0.0243)	(0.0220)
Post	0.380***	0.389***	0.232***	-0.184***	0.148***	0.0947***
	(0.0393)	(0.0302)	(0.0173)	(0.0467)	(0.0312)	(0.0282)
Treatment x Post	0.313***	-0.0134	0.0365*	-0.284***	-0.0252	0.0514*
	(0.0440)	(0.0325)	(0.0189)	(0.0516)	(0.0338)	(0.0311)
Observations	2340503	2340503	2340503	2522139	2522139	2522139
Control Group: 31-32						
Treatment	0.307***	0.0687***	0.0278**	0.885***	0.0454*	0.305***
	(0.0422)	(0.0214)	(0.0116)	(0.0377)	(0.0243)	(0.0205)
Post	0.412***	0.452***	0.179***	-0.149***	0.113***	0.158***
	(0.0438)	(0.0302)	(0.0171)	(0.0455)	(0.0310)	(0.0265)
Treatment x Post	0.282***	-0.0756**	0.0906***	-0.318***	0.00734	-0.00835
	(0.0481)	(0.0327)	(0.0188)	(0.0506)	(0.0336)	(0.0296)
Observations	2333315	2333315	2333315	2512780	2512780	2512780
Control Group: 32-33						
Treatment	0.315***	0.0710***	0.0415***	0.889***	0.0595**	0.349***
	(0.0405)	(0.0218)	(0.0116)	(0.0381)	(0.0244)	(0.0202)
Post	0.320***	0.465***	0.171***	-0.211***	0.112***	0.127***
	(0.0562)	(0.0308)	(0.0169)	(0.0457)	(0.0312)	(0.0259)
Treatment x Post	0.373***	-0.0912***	0.0984***	-0.253***	0.0119	0.0217
	(0.0595)	(0.0332)	(0.0186)	(0.0508)	(0.0338)	(0.0290)
Observations	2324668	2324668	2324668	2504805	2504805	2504805
Control Group: 33-34						
Treatment	0.388***	0.0796***	0.0601***	0.974***	0.0410	0.391***
	(0.0334)	(0.0223)	(0.0115)	(0.0382)	(0.0250)	(0.0200)
Post	0.357***	0.371***	0.191***	-0.196***	0.0550*	0.0888***
	(0.0382)	(0.0306)	(0.0170)	(0.0457)	(0.0315)	(0.0251)
Treatment x Post	0.339***	0.00267	0.0780***	-0.270***	0.0706**	0.0612**
	(0.0434)	(0.0330)	(0.0186)	(0.0507)	(0.0341)	(0.0284)
Observations	2313937	2313937	2313937	2497485	2497485	2497485
Control Group: 34-35						
Treatment	0.431***	0.140***	0.0495***	1.047***	0.0719***	0.439***
	(0.0467)	(0.0230)	(0.0126)	(0.0393)	(0.0258)	(0.0201)
Post	0.299***	0.455***	0.138***	-0.235***	0.0676**	0.0775***
	(0.0455)	(0.0324)	(0.0178)	(0.0471)	(0.0328)	(0.0254)
Treatment x Post	0.394***	-0.0812**	0.130***	-0.230***	0.0563	0.0723**
	(0.0504)	(0.0346)	(0.0194)	(0.0521)	(0.0353)	(0.0286)
Observations	2271409	2271409	2271409	2458995	2458995	2458995

Notes: Each panel shows the effects on transitions in and out of permanent contracts of the expansion of the payroll tax credits in 1997. Treatment group is composed of workers aged 25-30, and I use different control groups in each panel. The control group are workers aged 30-31 (first panel), 31-32 (second panel), 32-33 (third panel), 33-34 (fourth panel), and 34-35 (fifth panel). The dependent variables are dummies indicating whether a transition in (first column) or out happened (second and third column). Specification is equation 2. Control variables included are calendar quarter dummies, sex, education, experience, firm size, sector, citizenship, part-time job, and workers' disability. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Windfalls Test

	Young, 1997 Change		Prime-Age, 1997 Change		Young, 2001 Change	
	(1)	(2)	(3)	(4)	(5)	(6)
	Crowd-Out Non-Subsidized Jobs	Subsidized Jobs	Crowd-Out Non-Subsidized Jobs	Subsidized Jobs	Crowd-Out Non-Subsidized Jobs	Subsidized Jobs
Employment						
Treatment	-2.106***	-0.240***	0.498***	0.297***	-0.241***	0.103***
	(0.0549)	(0.0103)	(0.0779)	(0.0116)	(0.0333)	(0.0160)
Post	1.145***	0.365***	0.797***	0.250***	-0.112***	0.662**
	(0.0567)	(0.0136)	(0.0704)	(0.00968)	(0.0351)	(0.0169)
Treatment x Post	-0.810***	1.061***	-0.953***	1.014***	0.355***	-0.768***
	(0.0744)	(0.0188)	(0.104)	(0.0221)	(0.156)	(0.0773)
Observations	3693279	3693279	2178574	2178574	3988262	3988262
New Hires						
Treatment	0.288***	0.0186***	-0.0951***	0.0635***	0.183***	0.699***
	(0.0259)	(0.00236)	(0.0239)	(0.00468)	(0.0240)	(0.0197)
Post	0.252***	0.0977***	0.177***	0.0631***	-0.273***	0.0744***
	(0.0204)	(0.00477)	(0.0204)	(0.00753)	(0.0210)	(0.00783)
Treatment x Post	-0.0227	0.372***	-0.111***	0.249***	0.146***	-0.413***
	(0.0273)	(0.0106)	(0.0274)	(0.0118)	(0.0284)	(0.0149)
Observations	3693279	3693279	2178574	2178574	3988262	3988262
Lay-Offs						
Treatment	0.00127	0.0707***	0.00450***	-0.0412***	0.246***	-0.201***
	(0.000888)	(0.0127)	(0.00123)	(0.0159)	(0.00703)	(0.0126)
Post	0.0157***	0.407***	0.0116***	0.482***	0.112***	-0.00212
	(0.00162)	(0.0144)	(0.00144)	(0.0168)	(0.00610)	(0.0135)
Treatment x Post	0.0616***	-0.107***	0.0480***	0.0522**	-0.0293***	0.0500***
	(0.00311)	(0.0189)	(0.00380)	(0.0245)	(0.0103)	(0.0171)
Observations	3693279	3693279	2178574	2178574	3988262	3988262
Quits						
Treatment	0.00459***	0.0371***	0.00238***	-0.0299***	0.347***	-0.0281***
	(0.000293)	(0.00675)	(0.000786)	(0.00749)	(0.00776)	(0.0102)
Post	0.0140***	0.173***	0.00447***	0.0981***	0.108***	0.00583
	(0.00122)	(0.00794)	(0.000946)	(0.00795)	(0.00626)	(0.0106)
Treatment x Post	0.0661***	0.0165	0.0334***	-0.0309***	-0.0176	0.0594***
	(0.00295)	(0.0108)	(0.00293)	(0.0112)	(0.0114)	(0.0142)
Observations	3693279	3693279	2178574	2178574	3988262	3988262

Notes: The estimates have been multiplied by 100 so that they can be interpreted as effects in percentage points. The treatment group are workers aged 25-30 (columns 1, 2, 5 and 6) and 45-50 (columns 3 and 4). The control group are workers aged 30-35 (columns 1, 2, 5 and 6) and 40-45 (columns 3 and 4). The upper panel shows the employment effects for permanent non-subsidized (windfalls or crowd-out) and permanent subsidized contracts. The next three panels shows the effects on transitions in and out also for both types of contracts. The dependent variables are dummies indicating whether a transition in or out happened (lower panels), or whether the individual was employed (upper panel). I exploit both the expansion of the policy in 1997, and the removal of the policy for young workers in 2001. Specification is equation 2. Control variables included are calendar quarter dummies, sex, education, experience, firm size, sector, citizenship, part-time job, and workers' disability. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Cost-Benefit Analysis

	(1) Sample	(2) Population	(3) Mean Wage	(4) Mean Length	(5) Revenue (millions)	(6) MECF
Young Workers, 1997-2001						
Increase in Bonified Contracts	15629***	390746***	39.27***	385.99***	1220***	
	(3102)	(77545)	(0.1)	(1.7)	(242)	-0.52***
Increase in Windfall Contracts	24740***	618504***	39.27***	385.99***	-724***	
	(3102)	(77545)	(0.1)	(1.7)	(90.8)	
Decrease in UI	7814***	195373***	31.79***	147.27***	887***	(0.15)
	(1550)	(38773)	(0.08)	(1.39)	(176)	
Prime-Age Workers, 1997-2006						
Increase in Windfall Contracts	36992***	924800***	41.81***	977.76***	-4360***	1
	(1623)	(40592)	(0.11)	(5.52)	(191)	

Notes: Sample refers to the mean number of individuals within each group during the year when the policy was in place. Population is the translation of Sample into the population following the construction of the MCVL dataset (elevated by 25). Mean Wage and Mean Length are the average wage and duration for each type of group. Revenue is the estimated money accrued or lost by the government following equations 9-10. MECF is the Marginal Efficiency Cost of Funds.

Table 10: Sorting Into Tax Cut Contracts

	(1)	(2)	(3)
	Younger 30	Older 45	Both
University Education	-0.0184***	-0.0481***	-0.0172***
	(0.00502)	(0.00957)	(0.00449)
Experience	-0.113***	-0.285***	-0.113***
	(0.0146)	(0.0307)	(0.0147)
Log Firm Workforce	-0.0267***	-0.0183***	-0.0266***
	(0.00137)	(0.00156)	(0.00136)
Wage	-0.0515***	-0.213***	-0.0512***
	(0.0156)	(0.0136)	(0.0158)
University Education x Older 45			-0.0296**
			(0.0124)
Experience x Older 45			-0.175***
			(0.0194)
Log Firm Workforce x Older 45			0.00774***
			(0.00190)
Wage x Older 45			-0.163***
			(0.0132)
Observations	86857	28850	115707

Notes: The table displays the characteristics of permanent workers who are hired with a tax cut with respect to permanent workers hired without tax cut. Column 1 shows it for the sample of workers younger than 30 years old. Column 2 shows it for the sample of workers older than 45. Column 3 shows it for both groups of workers and includes interactions with dummies for workers older than 45. Specification is equation 19. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 11: Effects on Transition of Crossing the Birthday Thresholds, Small Firms

	(1) Perm.	(2) Short-Term
Young Workers, Small Firms		
Treatment	0.276***	-0.0499
	(0.0595)	(0.0801)
Observations	1217886	1217886
Senior Workers, Small Firms		
Treatment	0.374***	-0.0752
	(0.0305)	(0.0498)
Observations	2551782	2551782
Young Workers, Large Firms		
Treatment	0.0624	0.0233
	(0.0726)	(0.114)
Observations	427710	427710
Senior Workers, Large Firms		
Treatment	0.0679*	0.0274
	(0.0384)	(0.0673)
Observations	853567	853567

Notes: The estimates have been multiplied by 100 so that they can be interpreted as effects in percentage points. The table displays the effects on entries into permanent and short-term both for young and senior workers, and for small and large firms. The specification is equation 1. The dependent variables are dummies indicating each type of transition. The estimation window is of 12 months on each side of the threshold. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 12: Wage Incidence Across Age Discontinuities, Permanent Contracts with Tax Cut

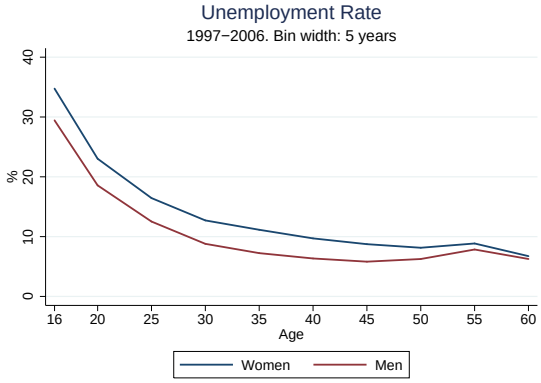
	Treatment: 27.5-30	Control: 30-32.5		Treatment: 45-47.5	Control: 42.5-45	
	(1)	(2)	(3)	(4)	(5)	(6)
	Wage	Wage	Wage	Wage	Wage	Wage
Treatment	-0.0134	0.0171	0.0154	0.194***	0.0700*	0.0409
	(0.0346)	(0.0285)	(0.0276)	(0.0472)	(0.0357)	(0.0351)
Men		0.0423***	0.0523***		0.102***	0.105***
		(0.0140)	(0.0136)		(0.0176)	(0.0172)
University Education		0.354***	0.350***		0.385***	0.403***
		(0.0230)	(0.0222)		(0.0375)	(0.0367)
Disabled		-0.0974	-0.0367		-0.146	-0.0890
		(0.130)	(0.126)		(0.192)	(0.187)
Agriculture		-0.0721	-0.0569		-0.206**	-0.197**
		(0.0793)	(0.0767)		(0.0816)	(0.0798)
Industry		0.0683***	0.0565***		0.0422**	0.0340*
		(0.0173)	(0.0167)		(0.0201)	(0.0196)
Construction		0.0410	0.0346		0.0601**	0.0626**
		(0.0274)	(0.0265)		(0.0269)	(0.0263)
Part-Time		-0.408***	-0.405***		-0.468***	-0.471***
		(0.0267)	(0.0259)		(0.0290)	(0.0283)
Log Firm Workforce		0.00137	0.00268		0.0114***	0.00980**
		(0.00291)	(0.00281)		(0.00403)	(0.00394)
Experience		0.108***	0.0645**		0.109***	0.0308
		(0.0267)	(0.0260)		(0.0251)	(0.0259)
Unemp. Rate		0.00854	0.00896		0.00205	0.00524
		(0.00565)	(0.00546)		(0.00421)	(0.00413)
Previous Wage		0.237***	0.255***		0.289***	0.290***
		(0.0127)	(0.0124)		(0.0162)	(0.0158)
Prov FE	N	Y	Y	N	Y	Y
Quarter FE	N	Y	Y	N	Y	Y
Calendar Month FE	N	Y	Y	N	Y	Y
Pre- and Post Linear Age Slope	N	Y	Y	N	Y	Y
Transition from ST within Firm	N	N	Y	N	N	Y
Observations	3009	3009	3009	1983	1983	1983

Notes: The table displays the effects on wages of crossing the policy thresholds at 30 and 45. The sample is for the period when there was a gap in subsidies across these ages (05/1999-03/2001 at 30 and 05/1997-06/2006 at 45). I select only men because for them the subsidy gap is unambiguous. The specification is equation 22. The estimation window is of 2.5 years on each side of the threshold. Robust standard errors, clustered at the month of birth level, are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

B Figures

Figure 1: Unemployment Rate

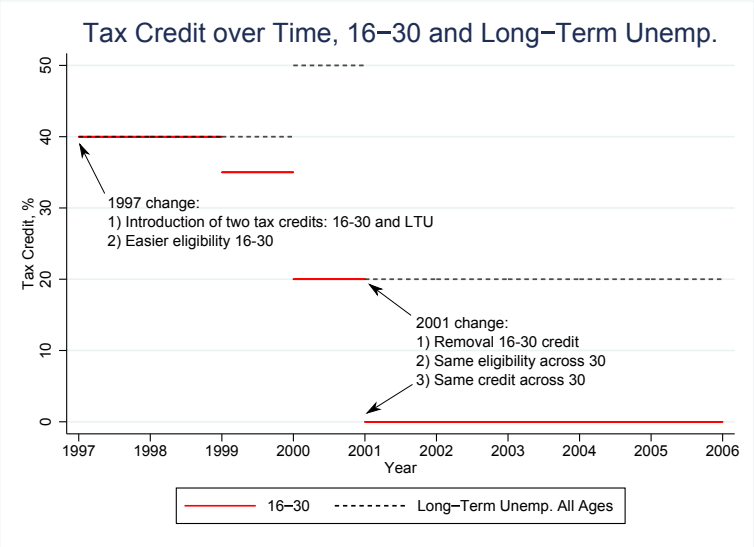
(a)



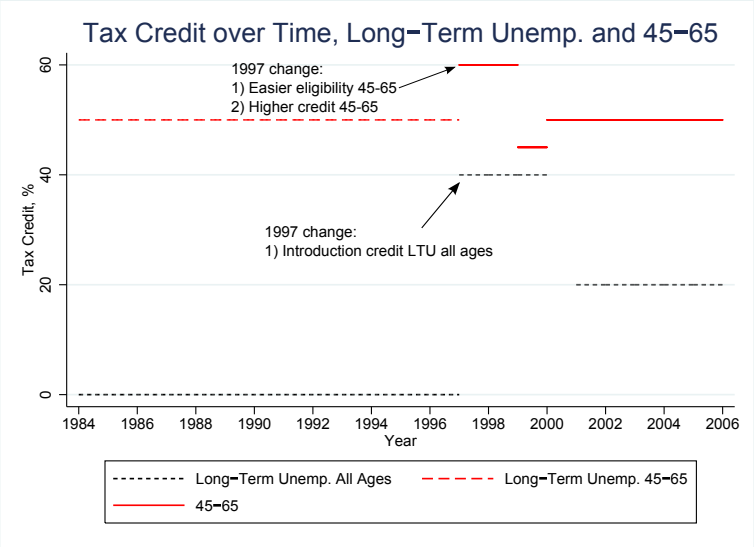
(b)

Notes: The data is from "Encuesta de Poblacion Activa". Figure (a) shows the unemployment rate over time for the main groups of the empirical analysis. The green dashed lines represent the quarter in which the payroll tax cuts were changed. The abrupt changes in the first quarter of 2001, that coincide with the second main policy change are due to changes in the survey question regarding who is considered unemployed. Figure (b) shows the unemployment rate by age and by gender.

Figure 2: Evolution of Payroll Tax Credits



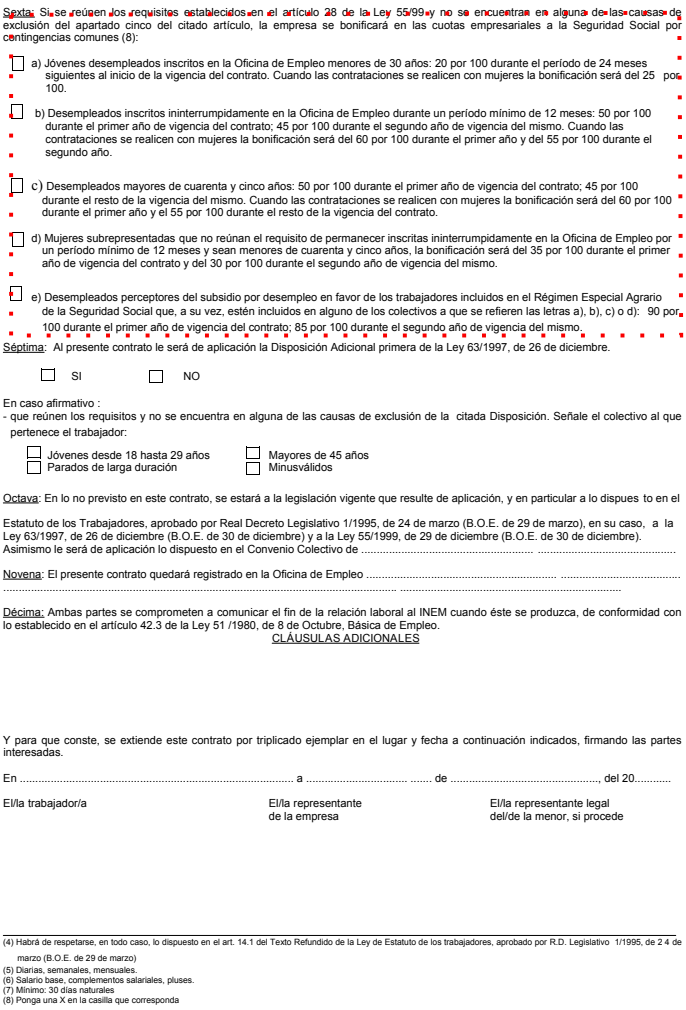
(a)



(b)

Notes: The graphs display the evolution of the payroll tax credits over time. Figure (a) shows the evolution of the credit for workers under 30 years and for the long-term unemployed (regardless of age). Figure (b) shows the evolution of the credit for workers older than 45 and also for the long-term unemployed (regardless of age). Note that the credit for the long-term unemployed is the same on both pictures. I show it in both pictures because it was the credit that could be claimed on the other side of the thresholds and was thus directly affecting the control group.

Figure 3: Example of Labor Contract



Sexta: Si se reúnen los requisitos establecidos en el artículo 28 de la Ley 55/99 y no se encuentran en alguna de las causas de exclusión del apartado cinco del citado artículo, la empresa se bonificará en las cuotas empresariales a la Seguridad Social por contingencias comunes (8):

☐ a) Jóvenes desempleados inscritos en la Oficina de Empleo menores de 30 años: 20 por 100 durante el período de 24 meses siguientes al inicio de la vigencia del contrato. Cuando las contrataciones se realicen con mujeres la bonificación será del 25 por 100.

☐ b) Desempleados inscritos ininterrumpidamente en la Oficina de Empleo durante un período mínimo de 12 meses: 50 por 100 durante el primer año de vigencia del contrato; 45 por 100 durante el segundo año de vigencia del mismo. Cuando las contrataciones se realicen con mujeres la bonificación será del 60 por 100 durante el primer año y del 55 por 100 durante el segundo año.

☐ c) Desempleados mayores de cuarenta y cinco años: 50 por 100 durante el primer año de vigencia del contrato; 45 por 100 durante el resto de la vigencia del mismo. Cuando las contrataciones se realicen con mujeres la bonificación será del 60 por 100 durante el primer año y el 55 por 100 durante el resto de la vigencia del contrato.

☐ d) Mujeres subrepresentadas que no reúnan el requisito de permanecer inscritas ininterrumpidamente en la Oficina de Empleo por un período mínimo de 12 meses y sean menores de cuarenta y cinco años, la bonificación será del 35 por 100 durante el primer año de vigencia del contrato y del 30 por 100 durante el segundo año de vigencia del mismo.

☐ e) Desempleados perceptores del subsidio por desempleo en favor de los trabajadores incluidos en el Régimen Especial Agrario de la Seguridad Social que, a su vez, estén incluidos en alguno de los colectivos a que se refieren las letras a), b), c) o d): 90 por 100 durante el primer año de vigencia del contrato; 85 por 100 durante el segundo año de vigencia del mismo.

Séptima: Al presente contrato le será de aplicación la Disposición Adicional primera de la Ley 63/1997, de 26 de diciembre.

☐ SI ☐ NO

En caso afirmativo :
- que reúnen los requisitos y no se encuentra en alguna de las causas de exclusión de la citada Disposición. Señale el colectivo al que pertenece el trabajador:

☐ Jóvenes desde 18 hasta 29 años ☐ Mayores de 45 años
☐ Parados de larga duración ☐ Minusválidos

Octava: En lo no previsto en este contrato, se estará a la legislación vigente que resulte de aplicación, y en particular a lo dispues to en el Estatuto de los Trabajadores, aprobado por Real Decreto Legislativo 1/1995, de 24 de marzo (B.O.E. de 29 de marzo), en su caso, a la Ley 63/1997, de 26 de diciembre (B.O.E. de 30 de diciembre) y a la Ley 55/1999, de 29 de diciembre (B.O.E. de 30 de diciembre). Asimismo le será de aplicación lo dispuesto en el Convenio Colectivo de .....

Novena: El presente contrato quedará registrado en la Oficina de Empleo .....

Décima: Ambas partes se comprometen a comunicar el fin de la relación laboral al INEM cuando éste se produzca, de conformidad con lo establecido en el artículo 42.3 de la Ley 51 /1980, de 8 de Octubre, Básica de Empleo.

CLÁUSULAS ADICIONALES

Y para que conste, se extiende este contrato por triplicado ejemplar en el lugar y fecha a continuación indicados, firmando las partes interesadas.

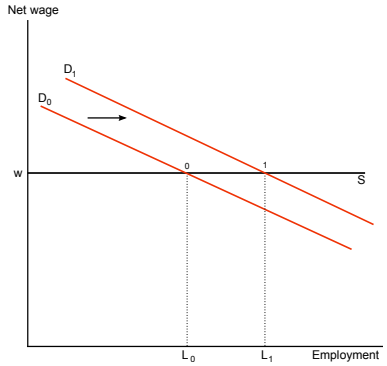
En ..... a ..... de ....., del 20.....

El/la trabajador/a | El/la representante de la empresa | El/la representante legal del/de la menor, si procede

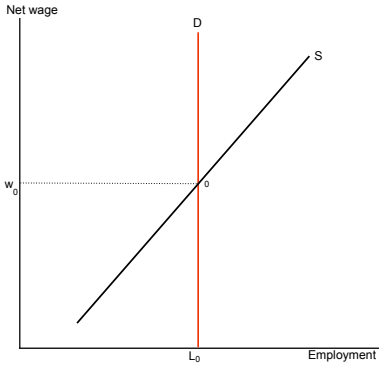
(4) Habrá de respetarse, en todo caso, lo dispuesto en el art. 14.1 del Texto Refundido de la Ley de Estatuto de los trabajadores, aprobado por R.D. Legislativo 1/1995, de 2 4 de marzo (B.O.E. de 29 de marzo)
(5) Diarias, semanales, mensuales.
(6) Salario base, complementos salariales, pluses.
(7) Mínimo: 30 días naturales
(8) Ponga una X en la casilla que corresponda

Notes: The figure shows the back page of a labor contract in Spain after the reform in May 1997. The 6th clause (sexta) has to be filled if the employer hires a worker who falls in one of the categories of subsidized employment. The 7th clause refers to the availability of lower severance payments in case of wrongful dismissal. The 8th clause says that the contract will be under the prevailing Spanish labor law, the 9th clause refers to the employment office where the contract is registered, and the 10th clause bounds each part to declare the end of the job relationship to the employment office when it happens. The lower part of the contract is where the employer and employee have to sign.

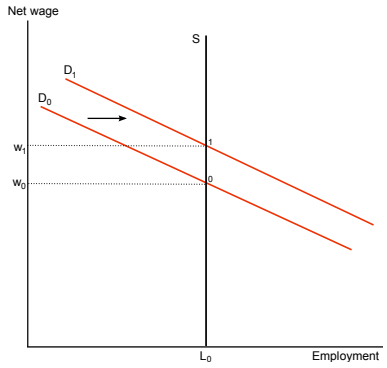
Figure 4: Graphical Representation of Employment and Wage Effects



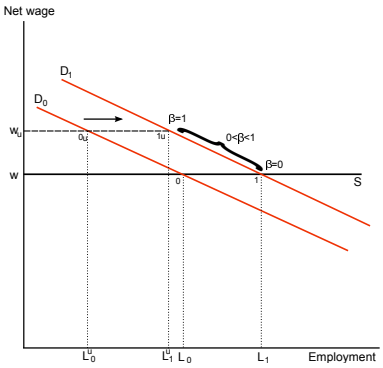
(a) Competite Market: Infinitely Elastic Supply



(c) Competite Market: Inelastic Demand



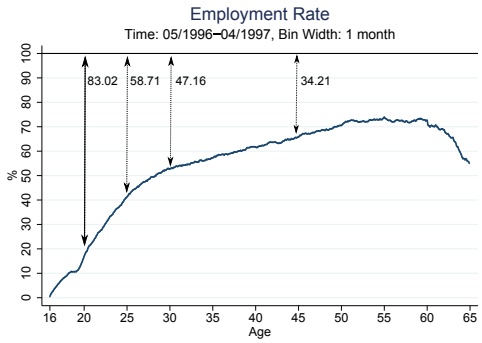
(b) Competite Market: Inelastic Supply



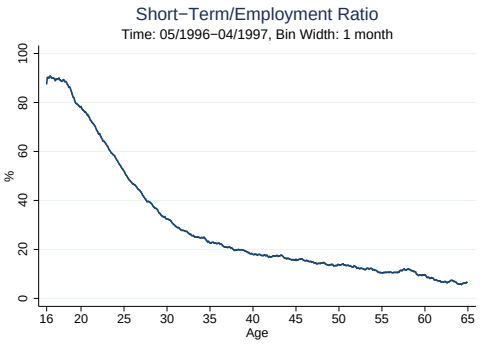
(d) Right-to-Manage Union Model

Notes: Figures (a)-(c) shows representations of a competitive labor market under different assumptions regarding the elasticity of demand and supply. Figure (d) depicts a unionized labor market with different degrees of union power: $0 \leq \beta \leq 1$, where $\beta = 0$ is a situation without union power and $\beta = 1$ is situation in which unions have all bargaining power. In each figure I show the impacts of a shift outwards in demand.

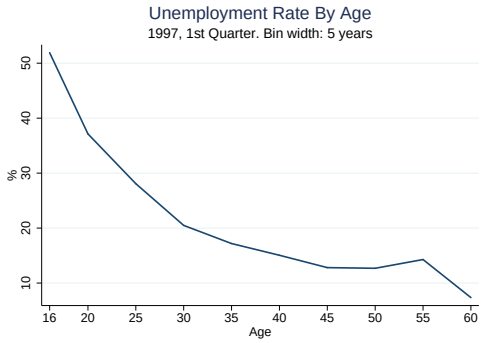
Figure 5: Ratio of Workers in Each Contract Relative to Workers Employed



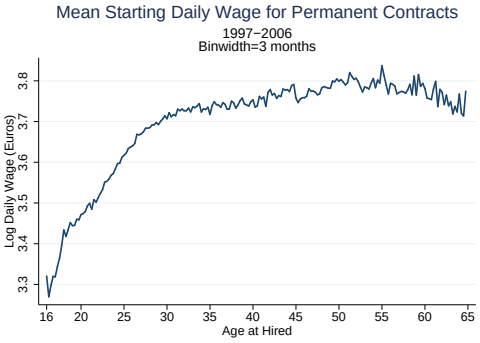
(a)



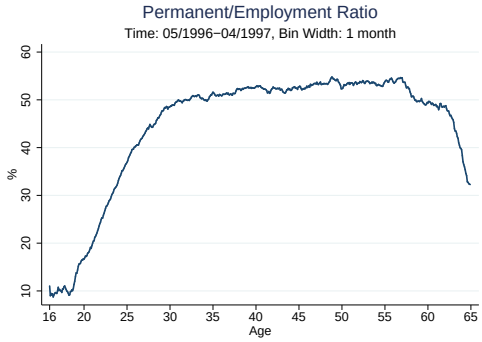
(d)



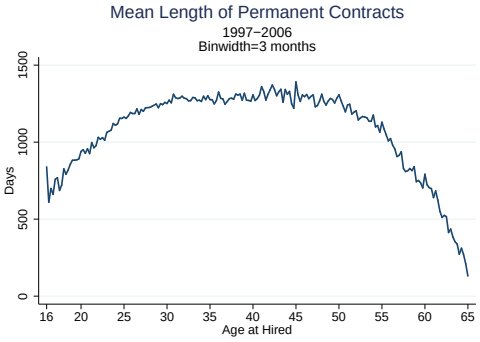
(b)



(e)



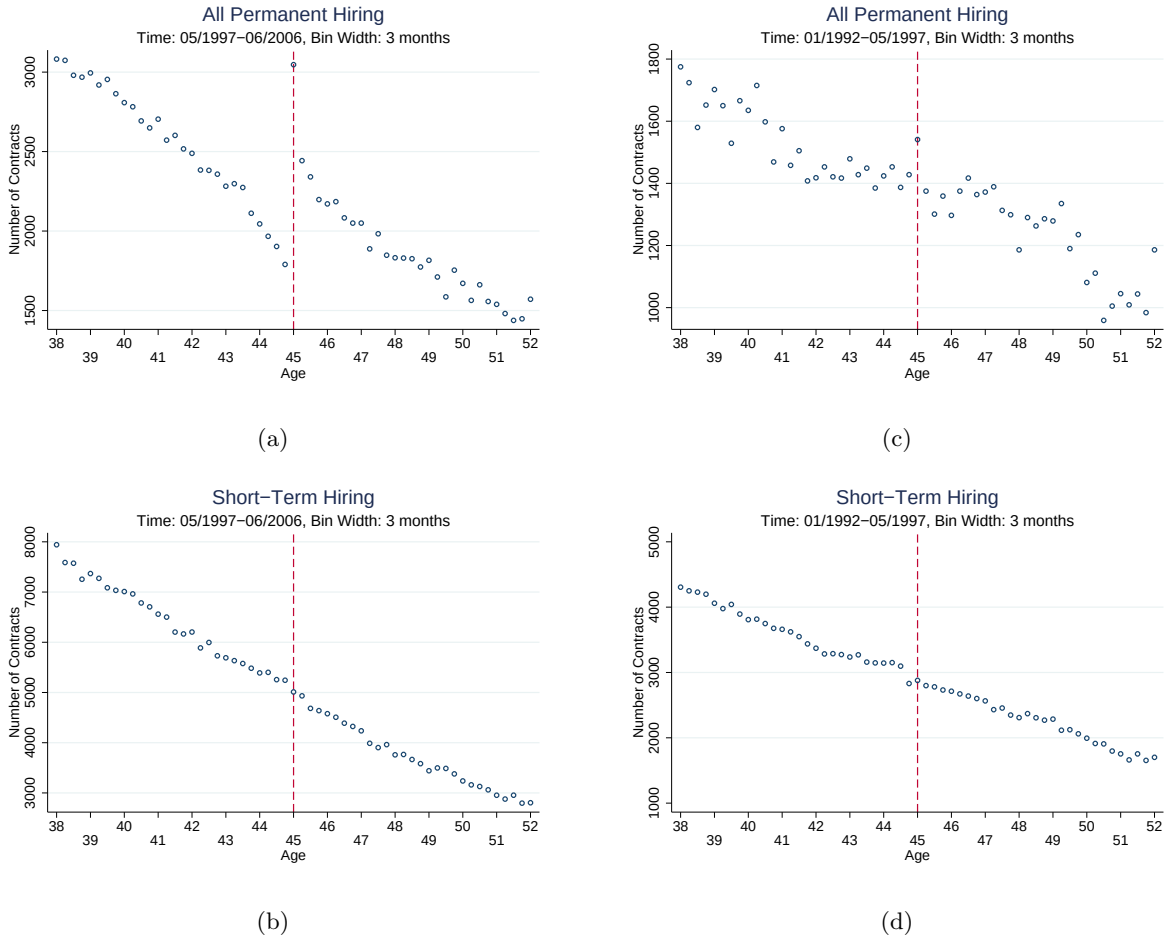
(c)



(f)

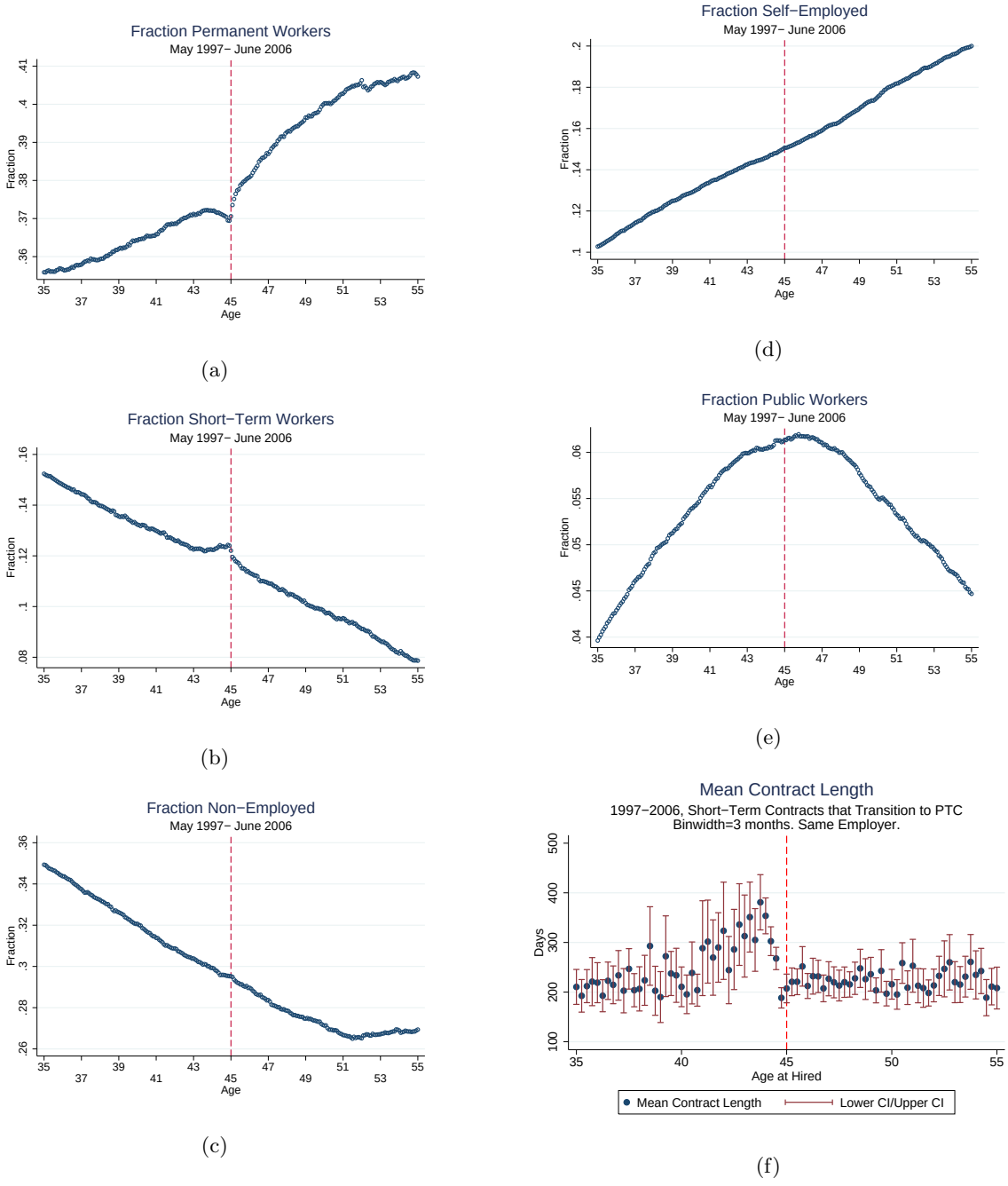
Notes: Figure (a) shows the employment rate by age. Bin width is one month. The solid black line representes the maximum employment rate of 100%. The dashed vertical lines show the distance to the 100% rate for several ages. Figure (b) shows the unemployment rate by age. Bin width is 5 years. Figures (c) and (d) show the ratio of permanent and short-term workers with respect to the number of employed workers for each month-age bin, respectively. Bin width is one month. Figure (e) shows the starting wage in permanent contracts by age at hired. Figure (f) shows the mean contract duration of permanent contracts by age. The period in figures (a)-(d) corresponds to the year before the first policy change (05/1996-04/1997). For figures (e) and (f) the period corresponds to the time on which I focus my analysis (05/1997-06/2006). Data for figures (a), (c)-(f) is from MCVL, and for figure (b) is EPA.

Figure 6: Hiring By Type of Contract, Prime-Age Workers



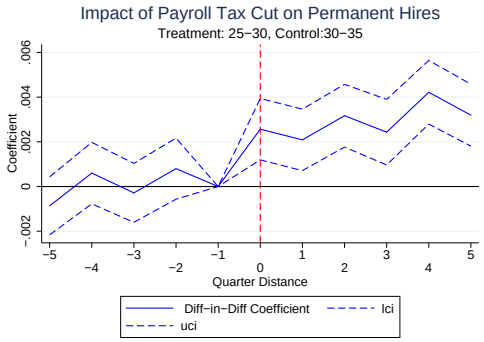
Notes: The figures on the left show the raw hiring data around the threshold at 45 years for two types of contracts: all permanent (including subsidized and not subsidized) and short-term, for the period when the policy was in place (1997-2006). Similarly, figures on the right show hiring around the discontinuity for the period 1992-1997 when the prime-age credit only applied for long-term unemployed workers. The bin width is three months.

Figure 7: Employment Rate, 1997-2006

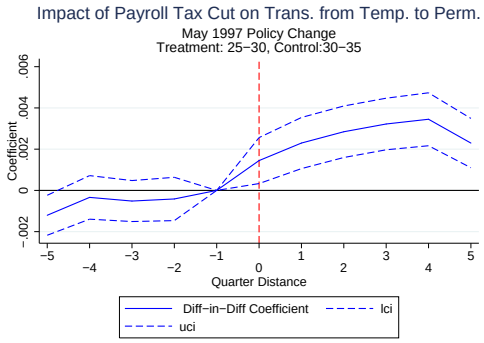


Notes: Figures (a)-(e) show the stocks of workers in permanent, short-term, non-employment, and public jobs, around the age of 45. Figure (f) shows the mean contract length in termporary contracts before being converted to permanent within the same firm. The red dashed line corresponds to the policy threshold.

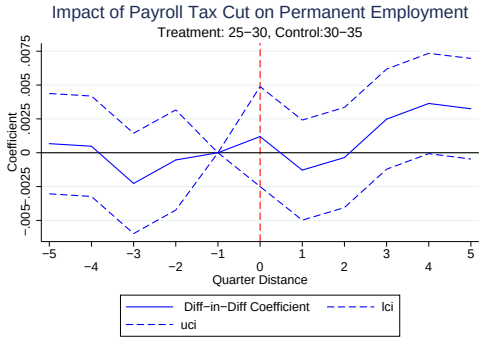
Figure 8: Difference-in-Difference Results for the Impact of the Expansion of Payroll Tax Cuts on Transitions, Young Workers



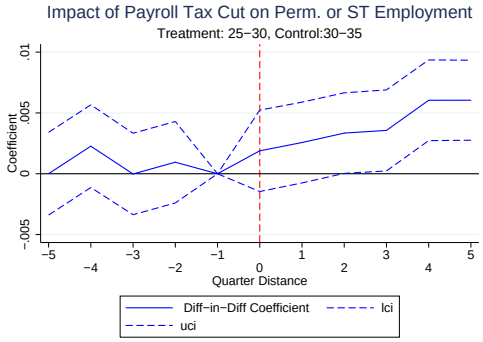
(a)



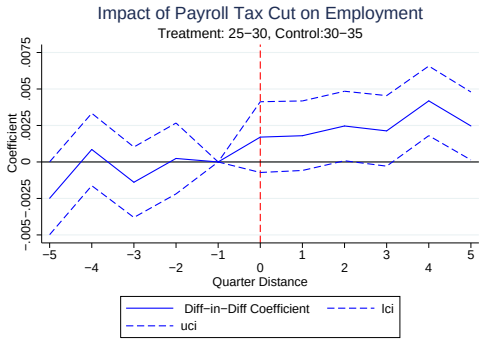
(b)



(c)



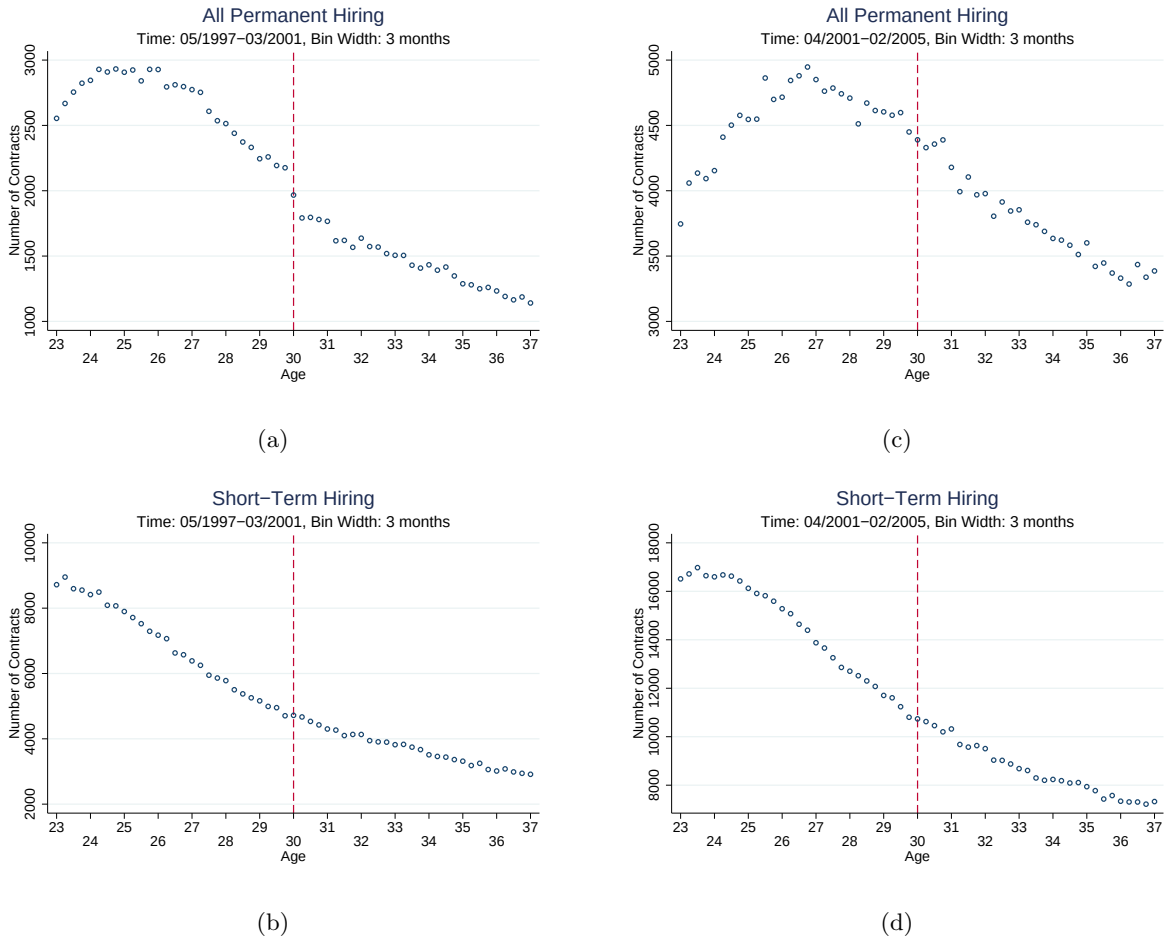
(d)



(e)

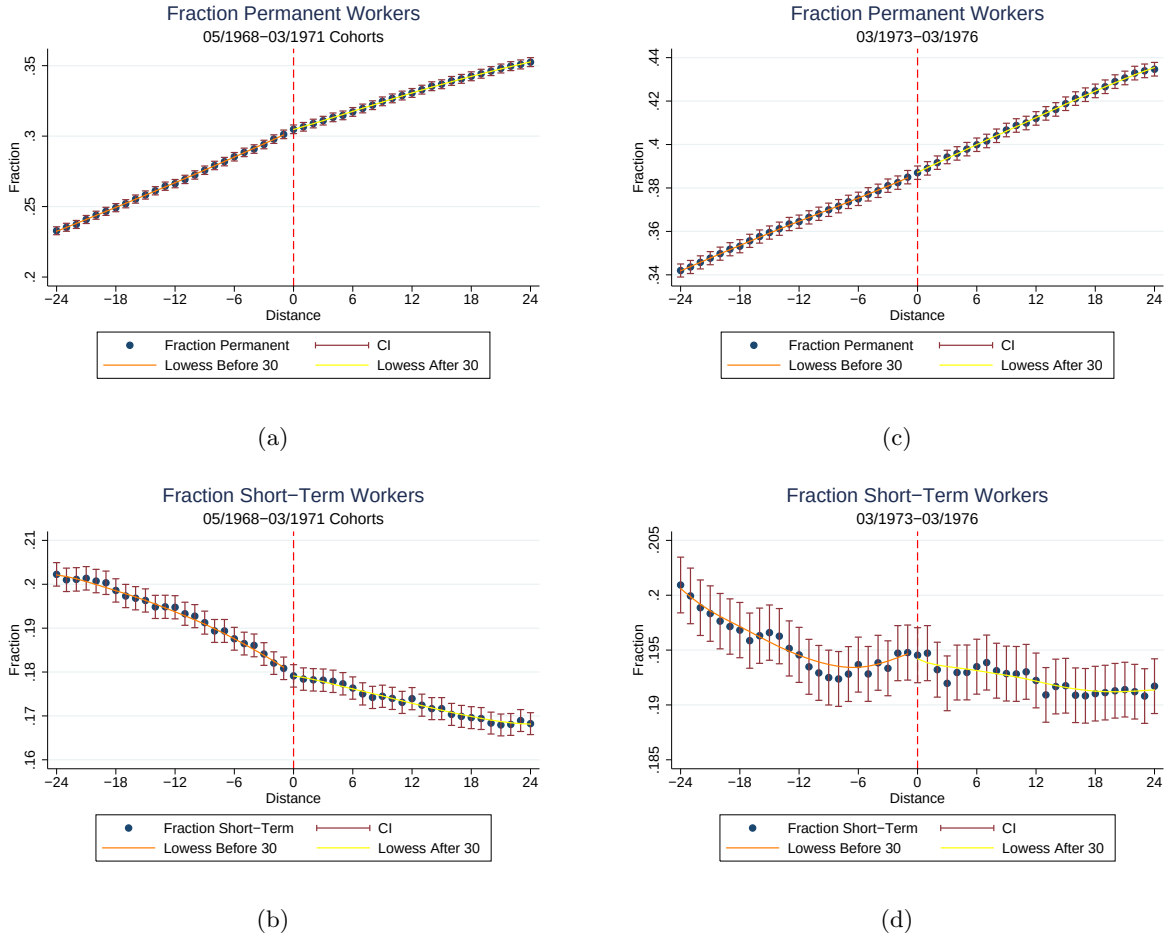
Notes: The graphs display coefficients (blue solid line) from regressing dummies indicating whether a worker was hired as permanent worker (a), transitioned from temporary to permanent within the same firm (b), was permanently employed (c), was permanently or temporarily employed (d) or employed in any contract (e) on time period dummies interacted with a treatment indicator (25-30 years old). Thus, the coefficients are difference-in-difference estimates in each time period. The omitted period is the quarter before the policy change. The specification is equation 3 and it includes time-period dummies, age dummies, and controls sex, education, experience, firm size, sector, citizenship, part-time job, and workers' disability. The red dashed represents the quarter when the policy was expanded, and the blue dashed line are upper and lower confidence intervals at the 95% level.

Figure 9: Hiring By Type of Contract, Young Workers



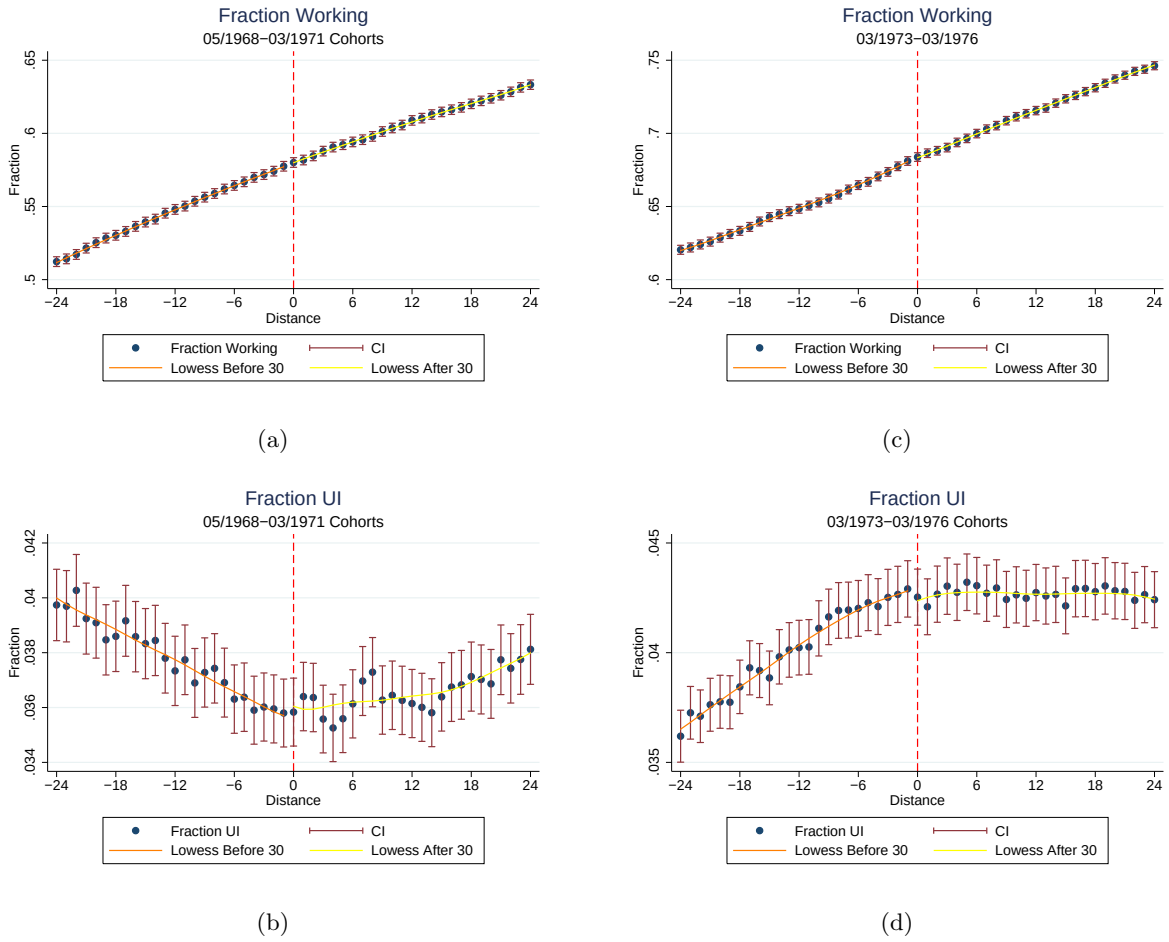
Notes: The figures on the left show the raw hiring data around the threshold at 30 years for two types of contracts: all permanent (including subsidized and not subsidized) and short-term, for the period when the policy was in place (1997-2001). Similarly, figures on the right show hiring around the discontinuity for the period 2001-2005 when the credit for young workers was not in place. The bin width is three months.

Figure 10: Employment Effects Around 30th Birthday



Notes: The figures show the fraction of individuals who are either permanent or short-term workers around their 30th birthday. The red dashed line represents the month of their 30th birthday. Figures on the left are for cohorts that could be hired with a subsidy during the 2 years prior to their 30th birthday (born between 05/1968-03/1971). Figures on the right are for cohorts that could not be hired with a subsidy during the two years prior to their 30th birthday (born between 03/1973-03/1976) and serve as a placebo test. The orange line is a local linear smooth of the fraction of workers on the month distance before their 30th birthday. Similarly, the yellow line is a local linear smooth of the fraction of workers on the months that have passed since their 30th birthday. For cohorts that could be hired as permanent workers with an employment credit, the slope after 30 is significantly less positive than the slope before 30. In the case of short-term workers, the slope after 30 is less negative. This indicates that part of the effect of the policy in the long-run is shifting between types of contracts. None of the slopes after 30 for the placebo groups are significantly different from the pre-30 slope.

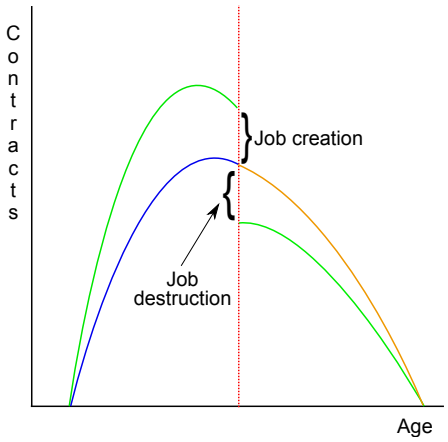
Figure 11: Employment Effects Around 30th Birthday



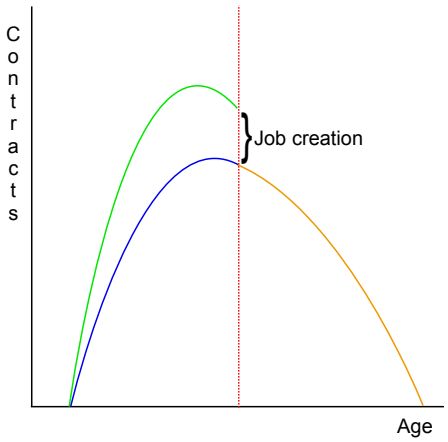
Notes: The figures show the fraction of individuals who are either working in any type of contract (permanent, short-term, self-employed, public worker), or are receiving UI, around their 30th birthday. The red dashed line represents the month of their 30th birthday. Figures on the left are for cohorts that could be hired with a subsidy during the 2 years prior to their 30th birthday (born between 05/1968-03/1971). Figures on the right are for cohorts that could not be hired with a subsidy during the two years prior to their 30th birthday (born between 03/1973-03/1976) and serve as a placebo test. The orange line is a local linear smooth of the fraction of workers on the month distance before their 30th birthday. Similarly, the yellow line is a local linear smooth of the fraction of workers on the months that have passed since their 30th birthday. For cohorts that could be hired as permanent workers with a wage subsidy, the slope of working in any contract after 30 is significantly more negative than the slope before 30. In the case of UI recipients, the slope after 30 is more positive. None of the slopes after 30 for the placebo groups are significantly different from the pre-30 slope. Thus, the wage subsidy increases overall employment of young workers even years the policy was enacted.

Figure 12: Illustration of the Potential Effects

(a)



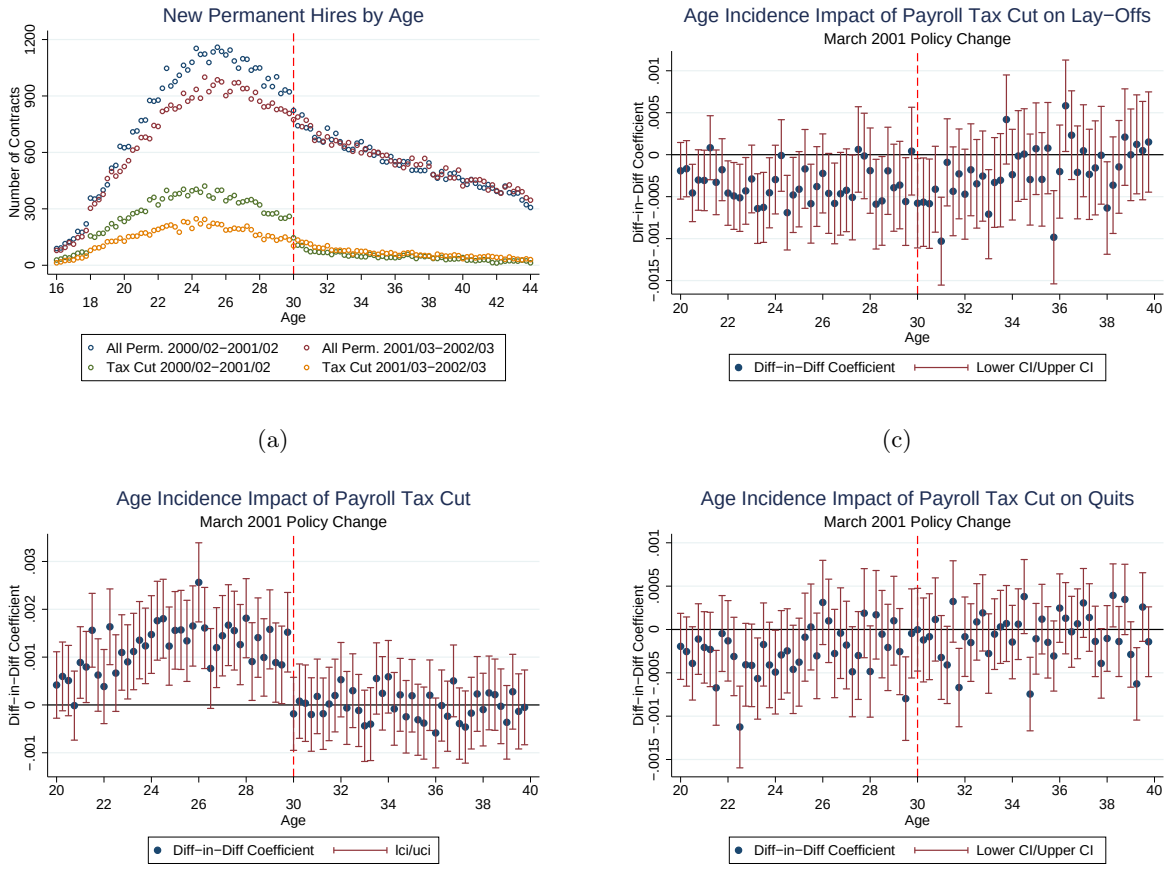
(b)



(c)

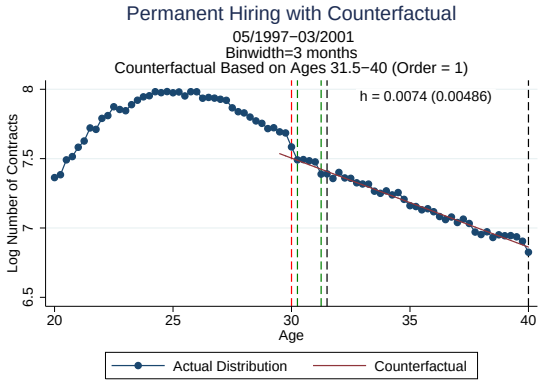
Notes: The figures show three potential scenarios of how the employment credit might affect the age distribution of hiring. The upper figure shows the case when there is no discontinuity. The middle figure depicts a situation when the employment credit generates both job creation and job destruction on each side of the threshol. Finally, the lower figure displays the case when the employment credit only stimulates job creation.

Figure 13: Age Incidence Impacts of Payroll Tax Cuts on Hires, Young Workers

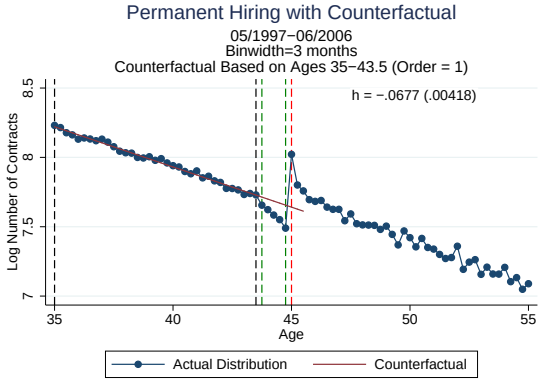


Notes: figure (a) display the raw data on hires at each age. If there were displacement effects, we would expect that the distribution of hiring in 2001 changes as in figure 12b. However, what we see is that the convergence occurs as in figure 12c, consistent with no displacement occurring. Figure (b) shows the difference-in-difference estimates for each age bin of the policy change in 2001. The specification is equation 4. I show the coefficients of the interaction *Pre* x *Age*, where *Pre* is the period 03/2000-03/2001. The bins are 3 months wide. The omitted group are workers 20 years old or less. The x-axis is the age when hired. If there are displacement effects affecting primarily workers over 30, we expect to see significant effects of opposite sign on each side of the 30 year old threshold (depicted with a red dashed line). As can be seen, there is no negative effect on hiring above 30, but there is a positive effect below 30. Similarly, figures (c) and (d) repeat the same strategy for lay-offs and quits, respectively. As can be seen, the effects in the treatment (25-30) and control (30-35) groups are not significantly different.

Figure 14: Missing Hiring in the Proximity of the Thresholds



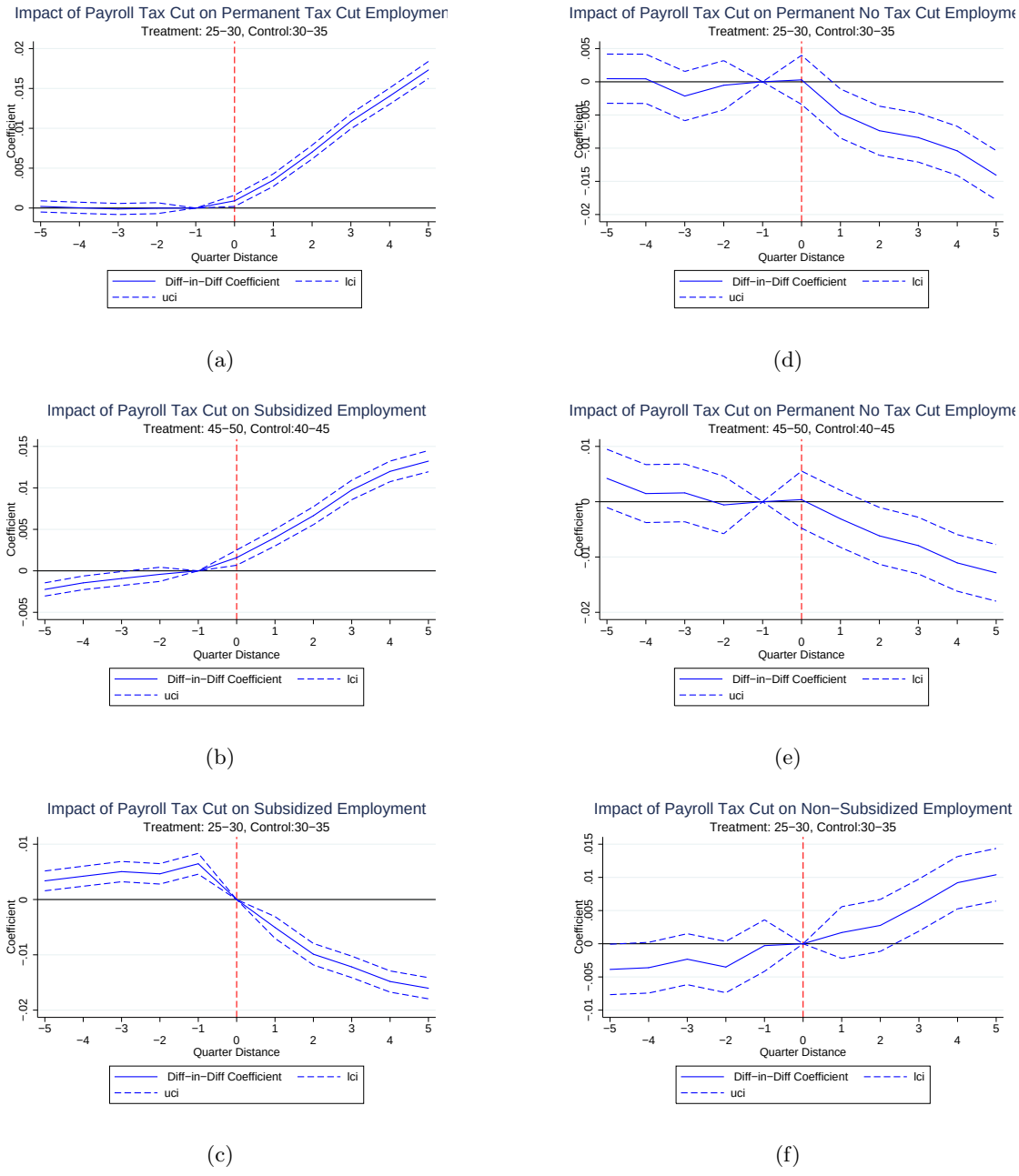
(a)



(b)

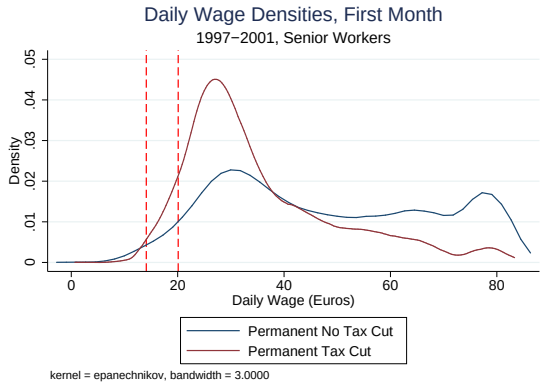
Notes: The figures show the raw hiring data (blue dots) and a counterfactual (red solid line) of what hiring would have looked like in the proximity of the threshold, if the threshold had been moved further to the left (upper figure) or to the right (lower figure). The black dashed lines contain the data points used to construct the counterfactual. The green dashed lines show the area for which I measure missing hiring (hole). The red dashed line represents the location of the policy discontinuity. h is the measure of missing hiring. The standard error is between parentheses.

Figure 15: Windfalls Test, Transition into Permanent Contracts, Young Workers

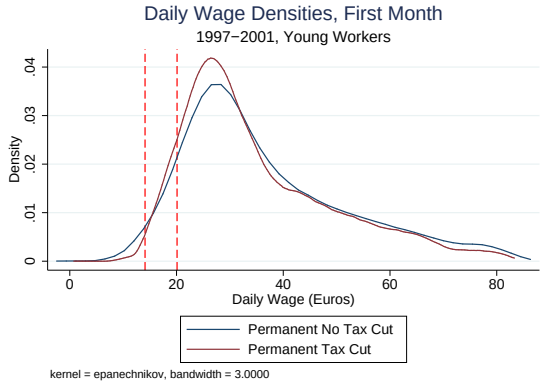


Notes: The figures show the effect on transitions to permanent subsidized (left) and non-subsidized (right) of the policy changes in 1997 (top and middle figure) and 2001 (bottom figure). The solid blue line are the coefficients from regressing dummies indicating subsidized permanent employment (left figures) or non-subsidized permanent employment (right figures) on time period dummies interacted with a treatment indicator. Thus, the coefficients are difference-in-difference estimates in each time period. The omitted period is the quarter before the policy change for (upper and middle) or the quarter when the policy was changed (lower figure). The specification is equation 3 and it includes time-period dummies, age dummies, and controls sex, education, previous wage, experience, firm size, sector, citizenship, part-time job, and workers' disability. The red dashed represents the quarter when the policy was expanded, and the blue dashed line are upper and lower confidence intervals at the 95% level.

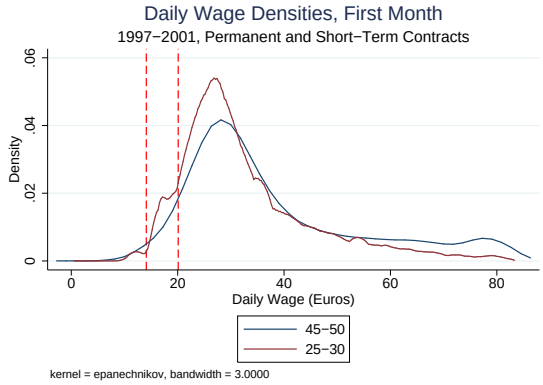
Figure 16: Wage Densities



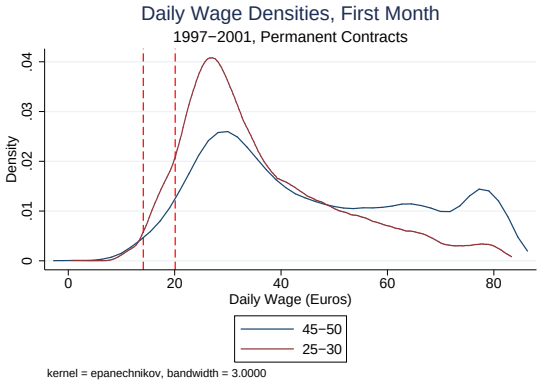
(a)



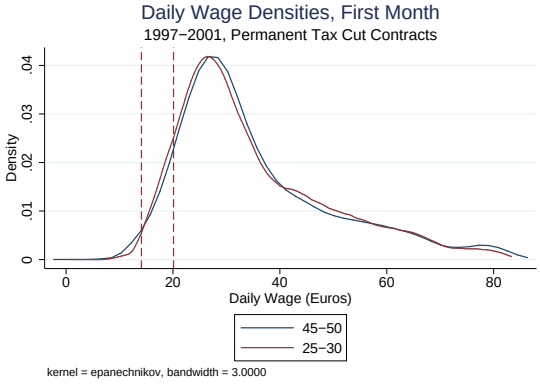
(b)



(c)



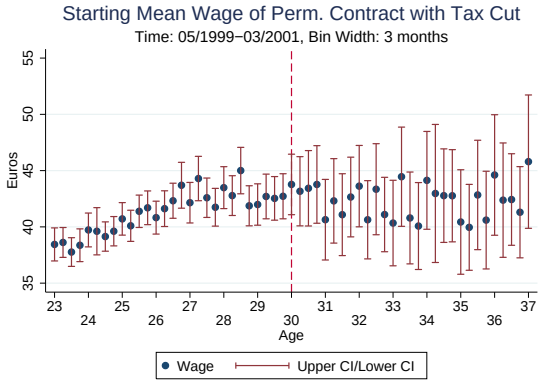
(d)



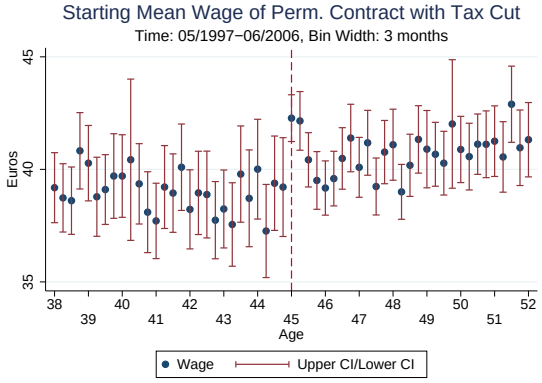
(e)

Notes: The figures display wage densities for different types of contracts and ages. Figure (a) shows it for permanent subsidized and non-subsidized young workers. Figure (b) shows it for permanent subsidized and non-subsidized prime-age workers. Figure (c) shows wage densities both for young and prime-age workers, pooling together permanent and temporary workers. Figure (d) shows it for permanent young workers and prime-age workers. Figure (e) shows it for permanent subsidized young workers and prime-age workers.

Figure 17: Mean Wage by Age



(a)



(b)

Notes: The figure on the left shows the mean daily real starting wage around the threshold at 30 years for permanent contracts with tax cut. Similarly, the figure on the right shows it around the discontinuity at 45. The bin width is three months. The figures correspond to the periods when the policy was in place as a reduction in the tax rate: 1997-2001 (young workers) and 1997-2006 (prime-age workers). The red dashed line indicates the location of the threshold. The only visually apparent change in mean wages is in permanent contracts with a tax cut at 45 years old. After the threshold, mean wages are consistently higher than before the discontinuity.

C Derivations

This section details the derivations of tax incidence formulas. Start from the equilibrium condition:

$$L^d(w(1+t)) = L^s(w) \tag{23}$$

where L^d is labor demand, w is the net wage, t is the employer's payroll tax rate, and $L^s(w)$ is labor supply.

(1) To obtain equation 14:

Suppose $t = 0$ and the tax rate is changed. Differentiating:

$$L^{d'}(dw + wdt) = L^{s'}dw \tag{24}$$

And rearranging:

$$\frac{dw}{dt}\frac{1}{w} = \frac{L^{d'}\frac{w}{L}}{\frac{(L^{s'}+L^{d'})w}{L}} \tag{25}$$

$$\frac{d\log w}{dt} = \frac{-\epsilon_D}{\epsilon_S + \epsilon_D} \tag{26}$$

(2) To obtain equation 15, follow the same steps as above. Suppose $t = 0$. Differentiating:

$$L^{d'}(dW) = L^{s'}dw \tag{27}$$

Rearranging:

$$\frac{dW}{dt}\frac{1}{W} = \frac{L^{s'}}{L^{d'}}\frac{dw}{dt}\frac{1}{W} \tag{28}$$

$$\frac{d\log W}{dt} = \frac{\epsilon_S}{\epsilon_S + \epsilon_D} \tag{29}$$

(3) To obtain equation 16, recall $L = L^d(w(1+t)) = L^s(w)$. Suppose $t = 0$. Differentiating:

$$dL = L^{s'}dw \tag{30}$$

Rearranging:

$$\frac{dL}{dt} = \frac{L^{s'}dw}{dt} = \frac{L^{s'}w}{L}\frac{dw}{wdt}L = \epsilon S\frac{dw}{wdt}L \tag{31}$$

$$-\frac{d\log L}{dt} = \frac{\epsilon_S\epsilon_D}{\epsilon_S + \epsilon_D} \tag{32}$$