CAN FIXED-TERM CONTRACTS PUT LOW SKILLED YOUTH ON A BETTER CAREER PATH? EVIDENCE FROM SPAIN*

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Abstract

By reducing the commitment made by employers, fixed-term contracts can help low-skilled youth find a first job. However, the long-term impact of fixed-term contracts on these workers' careers may be negative. Using Spanish social security data, we analyze the impact of a large liberalization in the regulation of fixed-term contracts in 1984. Using a cohort regression discontinuity design, we find that the reform raised the likelihood of male high-school dropouts working before age 20. However, by increasing the number of employment spells, the reform reduced workers' accumulated employment (up to 2006) by 7% and accumulated earnings by 22%.

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1 Introduction

European countries typically have a high level of employment protection legislation (EPL), making it expensive to dismiss workers on permanent contracts. While EPL does not reduce overall employment, it has an adverse effect on the employment of marginal workers such as youth or low-qualified individuals. To address this problem, a number of European countries have made it easier for firms to use fixed-term contracts with lower firing costs. It was hoped that fixed-term contracts would help improve the labor market outcomes of marginal workers in high EPL countries.

But do fixed-term contracts really help? In theory, fixed-term contracts, by decreasing firing costs, can help workers with uncertain credentials obtain employment. Moreover, fixed-term contracts may help workers accumulate human capital as well as gain contacts that can allow them to obtain better jobs in the future. On the other hand, there is a danger that marginal workers go from fixed-term contract to fixed-term contract, leading to lower employment stability and no progression towards better jobs (Blanchard and Landier, 2002). In theory, the impact of fixed-term contracts on the labor market outcomes of affected workers is therefore ambiguous.

The empirical evidence on the impact of fixed-term contracts and temporary work on the labor market outcomes of affected workers is also mixed. A number of papers examine the stepping stone hypothesis according to which temporary work helps workers land permanent jobs. The evidence at the European level is mixed, with some papers finding evidence of the stepping-stone hypothesis (D'Addio and Rosholm (2005); Booth, Francesconi and Frank (2002), among others) while some other papers find that temporary contracts do not improve access to permanent contracts (Zijl, Van Den Berg and Heyma, 2004). The evidence on temporary help jobs is also mixed with the best identified study finding a somewhat negative impact of temporary work on earnings and employment in the US (Autor and Houseman, 2010), while studies using European data find a positive effect of temporary help jobs on securing permanent employment (e.g. Ichino, Mealli, and Nannicini, 2008).

Even if fixed-term contracts can help workers secure permanent jobs, the long-term effects of easily available fixed-term contracts remain uncertain. Indeed, when workers lose a permanent job, they may be back on a fixed-term contract and their return to stable employment may be delayed. The current literature only addresses the impact of temporary work within a few months to a few years of the first temporary job, and the long-run impact remains unknown.

Another limitation of the existing literature is that it compares people who did and did not use temporary contracts (or temporary help agencies) within a given regulatory framework. This research cannot therefore directly inform us about what happens when the regulation of temporary work becomes less stringent. With a laxer regulation of fixed term contracts, some unemployed workers may get a fixed term contract fairly quickly and then transition to a permanent contract. However, it is possible, that, were fixed term contracts not easily available, the same workers would have transitioned to a permanent contract even more rapidly. Therefore, even if we believe the stepping stone hypothesis that, for an unemployed worker, taking a fixed term contract is better than staying unemployed, this does not imply that making fixed term contracts more easily available will increase the proportion of low skilled workers in permanent contracts and benefit their careers. Overall, both the long-term impact of temporary work on careers, and the overall impact of fixed-term contract regulation remain open questions.

In this paper, we use Spanish social security data to assess the long-term impact of fixed-term contract regulation on employment and earnings. Spain is an ideal ground for our research because fixed-term contract use was liberalized early and Spain subsequently became the OECD country with the highest share of fixed-term contracts (Dolado et al., 2002). To assess the impact of fixed-term contracts, we track cohorts of workers who enter the labor market before and after a 1984 reform that considerably liberalized the use of fixed-term contracts. We focus specifically on male high school dropouts because they are most likely to be affected by the liberalization of this type of contracts.

Using a cohort regression discontinuity design and aggregate data with controls for experience and time effects, we show that, compared to workers who entered the labor market just before the reform, workers who enter the labor market right after the reform experience a larger probability of working before age 20. However, workers who enter the labor market right after the reform also experience both lower accumulated days of work and lower accumulated earnings over their subsequent career up to the year 2006. We estimate the loss in terms of accumulated days of work to amount to 7% (193 days less). The accumulated earnings loss is as high as 22%. Hence, greater fixed-term contract availability at the beginning of a worker’s career has a negative impact on long-run career outcomes mostly because workers are more likely to work under non-permanent contracts well after labor market entrance. This situation further exposes workers to the well known wage penalties associated to both unemployment and temporary work. Finally, we have also found that the adverse impact of fixed-term contracts are concentrated in the first 5 to 10 years of the worker's career, but can persist beyond this time frame for some outcomes. This persistence is especially important for accumulated wages and also for the number of job interruptions suffered by affected workers.

This paper makes two key contributions to the literature. First, while previous literature on the labor market impact of fixed term contracts has relied on regression adjustments and other non-experimental techniques for identification, we use a regression discontinuity design that exploits a large change in Spanish regulation. Second, our paper innovates by examining the long-term impact of fixed-term contracts on young people's career: Social Security data allows us to have more than twenty years of follow up while previous literature examined outcomes over at most a few years. Contrary to most of the previous literature on European labor markets, we find a negative impact of fixed term contracts on labor market outcomes. Our results are most similar to the quasi-experimental study of temporary help jobs in the US (Autor and Houseman, 2010). We conclude that the combination of examining long-run outcomes and using a quasi-experimental design allows us to uncover substantial and previously undocumented negative effects of fixed-term contracts on the long-term labor market outcomes of low-skilled youth.

There have been a number of previous papers examining the use of fixed-term contracts in Spain, but focusing on somewhat different issues from our paper.[1] First, the previous literature has examined the impact of fixed-term contract duration on the probability of finding a permanent job. Most of those papers (see for example Güell and Petrongolo (2007) or García-Pérez and Muñoz-Bullón (2011)) agree that the effect of temporary contract duration on the probability of finding a permanent job is slightly increasing with tenure at the temporary contract but mainly concentrated at specific durations of the temporary contract (6, 12, 24 – the typical duration of these contracts-, and specially at the 36th. month, the maximum duration of such contracts in Spain). Second, the paper by Rebollo (2011) stresses the idea that not only the number of temporary contracts and job

[1] Some recent papers as Bentolila, Cahuc, Dolado and Le Barbanchon (2012) or Costain, Jimeno and Thomas (2010) analyze the effect of temporary contracts in Spain within the current context of economic crisis. The former explores the extent to which the significantly larger increase in unemployment in Spain, versus France, during the ongoing recession can be accounted for by the difference in EPL between the two countries. The latter studies the extent to which the coexistence of permanent and temporary jobs accounts for the volatility of employment in Spain. An also recent paper by Fernández-Kranz et al. (forthcoming) finds that having a permanent contract partially protects Spanish women against the part-time/full-time wage gap.

interruptions are important in determining the chances of a worker to find a permanent contract but also the number of firms for which this person has worked is an important determinant. Therefore, accumulating temporary contracts with the same firm reduces the probability of getting a permanent contract while moving from one firm to another with a temporary contract increases the chances of getting a permanent contract. Third, the literature has examined the impact of fixed-term contracts on firms' outcomes. Using the simulations of a dynamic labor demand model, Aguirregabiria and Alonso-Borrego (2014) evaluate the effects of the same labor market reform we are analyzing: the liberalization of fixed term contracts in 1984. They find that both total employment and job turnover increased due to the reform in Spanish manufacturing firms.

Our paper is also related to a literature that examines the long-term labor market impact of entering the labor market under high unemployment. Kahn (2010), Moulton (2011) and Oreopoulos et al. (2012) all find negative long-run wage effects of entering the labor market under adverse economic conditions. Moulton's paper uses a cohort regression discontinuity design to assess the impact of the Great Depression on subsequent wages on different cohorts. Oreopoulos et al. (2012) identify the long-term costs of graduating in a recessionary period but estimates the model using collapsed aggregate data in order to include better controls for a number of fixed effects such as calendar year or potential experience in the labor market. We use a similar regression discontinuity design as in Moulton but complement the analysis with a combination of the RD design (as in Moulton) with aggregate collapsed data (as in Oreopoulos et al., 2012) in order to identify the long-term effects of the Spanish liberalization of fixed-term contracts. We use the 1984 Spanish reform as a break point and we control for the unemployment rate at labor market entry. The paper proceeds as follows. In section 2, we discuss the history of temporary contracts in Spain. Section 3 describes the data and the empirical strategy. Section 4 presents the results and some discussion and robustness checks are performed in section 5. Section 6 concludes.

2 History and current rules of temporary contracts in Spain

Historical background

During the time of Franco's regime, the Spanish labor market was heavily regulated with a single trade union to which both employers and employees had to belong and with strong labor regulations to protect employment. This meant that, in reality, all jobs were full-time jobs of a permanent nature. After Franco's death in 1975 changes were introduced to relax some of the previous regulations. The most important one was the legalization of free trade unions and the abolishment of the single trade union in 1977.

It was not until 1980 that the strongest modernization of the labor relations system was introduced in Spain with the approval of the Workers' Statute. This law assumed every contract to be an open-ended contract as a general case, whereas temporary contracts were intended to be used only for jobs whose nature was temporary (seasonal jobs, temporary substitution of permanent workers, etc). Furthermore, the Worker's Statute kept most of the restrictions on dismissals. Dismissed permanent workers would receive severance payments that depend on whether the dismissal is fair or unfair. For fair dismissals, severance payments equal to 20 days of salary per year of job tenure with a maximum of one year wages, while for unfair dismissals severance payments amount to 45 days with a maximum of 42 months of wages. The reasons for the dismissal to be considered as fair are twofold: either the firm argues that the employee is incapable of performing his/her tasks or it argues that there are economic or technological reasons that require the dismissal of the worker. If a worker is dismissed under a fair dismissal by the firm but he/she does not agree with the fairness of the process, he/she can sue the firm and a legal process begins in which the firm has to pay the legal costs (in any case) as well as the wages of the worker if the dismissal is finally judged to be unfair by the court. In reality, labor courts effectively ruled most dismissals as unfair and so the costs of the legal process were usually higher for the employer than the severance payment for unfair dismissals. Although temporary contracts had a much lower severance payment (8 days per year of seniority), the restrictions on the use of this type of contracts meant that their use was very limited during the first half of the 1980s (Aguirregabiria and Alonso-Borrego, 2014; García-Pérez and Muñoz-Bullón, 2011).

During the first half of the 1980s, the Spanish unemployment rate experienced a rapid growth and it went over 20%. This event prompted the Spanish government to introduce a new reform in 1984.[2] This was the first reform designed to liberalize the use of temporary contracts and to reduce dismissal costs for this type of contracts. The most important element of the reform is the fact that it eliminated the requirement that the activity associated with a temporary contract had to be of a temporary nature. Therefore, firms whose activities are not of a temporary nature could sign temporary contracts with any type of worker after the reform of 1984. These contracts can be signed for a period between a

[2] The law was passed in August 2nd 1984 and the full implementation of the law was established in a "Real Decreto" in October 1984.

minimum of six months and a maximum of three years. After the three years, the contract cannot be renewed, and the worker must either be fired (in this case, the employer cannot hire any other worker for that job and has to wait for at least one year to hire the same worker) or must be offered a permanent contract by his/her current employer. Furthermore, another advantage of this type of contracts is that firing costs at termination are very low (8 days per year of tenure but they can even be zero in some cases) and this termination cannot be appealed in front of the courts. The reform in 1984 did not alter any of the conditions for permanent contracts explained above (Aguirregabiria and Alonso-Borrego, 2014), which made temporary contracts even more appealing for firms.

As a result of this legislative change, the proportion of male employees aged 15-24 under temporary contracts increased from less than 40% to over 70% in less than five years after the approval of the reform, as can be seen in Figure 1.[3] Between 1985 and 1994, over 95% of all new hires were employed through temporary contracts and the conversion rate from temporary into permanent contracts was only around 10% (Güell and Petrongolo 2007). Thus, the main concern with the liberalization of temporary contracts after 1984 was that it generated a huge segmentation in the Spanish labor market between unstable low-paying jobs and stable high-paying jobs, without helping to reduce unemployment.

Shifting direction in light of these concerns, in 1994 new regulations limited the use of temporary employment contracts to seasonal jobs. In addition, the 1994 reform slightly relaxed dismissal conditions for permanent contracts. In particular, the definition of fair dismissals was widened by including additional "economic reasons" for them. In practice, however, employers continued to hire workers under temporary contracts for all types of jobs and not just for seasonal jobs. This perceived ineffectiveness of the 1994 reform led to a new reform in 1997, which was eventually extended in 2001. As with the 1994 reform, the goal of the 1997 and 2001 reforms was to reduce the use of temporary contracts. The 1997 reform created a new type of permanent contract, with lower severance costs in case of unfair dismissal (33 days per year of seniority) and with fiscal incentives in the first two years of the contract (i.e., reductions of employers' payroll taxes).[4] However, rather than trying to limit the use of temporary contracts by further possibly ineffective regulation, these new reforms widened the employers' incentives to hire workers from certain

[3] The rate of temporary employment in the Spanish labor market as a whole moved from less than 10% to over 30% in the same period. Data in Figure 1 is taken from the Spanish Labor Force Survey, which only reports information on the type of contract (temporary/permanent) from 1987. In the following section, we present evidence to show that the use of temporary contracts was very small before the 1984 reform.

[4] This was the first time (in 1997) since the Workers' Statute in 1980 that severance costs were changed for permanent workers in Spain.

population groups under permanent contracts.[5] The 2001 reform essentially extended the 1997 reform by applying lower subsidies to more worker groups than the previous reform (García-Pérez and Muñoz-Bullón, 2011). The consequences of these subsidies, however, have not been a reduction in the use of temporary contracts or an increase in workers' employment stability but, on the contrary, only negligible effects on both dimensions because of the important side-effects (basically substitution effects) such subsidies have entailed (see García-Pérez and Rebollo, 2009)

Evolution of temporary contracts in Spain

Although the Spanish Labor Force Survey only reports information on the type of contract from 1987, some data from a report published in 1988 (Mateos and Sebastián, 1988) show that temporary contracts represented a very low percentage of the total number of non-agricultural private sector employees in 1983 (below 3% until 1986). We can see in Figure 2 that the number of temporary contracts increased from the last trimester of 1984 when the new legislation on temporary contracts was passed.

On the other hand, the Spanish Labor Force Survey shows that the proportion of temporary contracts as a percentage of total employment was around 10% at the beginning of 1987 and was already around 35% in the mid-1990s. Therefore, very soon after the introduction of the 1984 reform, Spain was the country in Europe with the highest proportion of temporary contracts and this rate has been approximately three times larger than the average in OECD countries and approximately 2.5 times higher than the average in Europe until the second half of the 2000s (see Figure 1).

It has been widely recognized that the group of workers that has been most affected by the widespread use of temporary contracts in the Spanish economy is the youngest group of workers that enters the labor market. If we look at data from the OECD in Figure 1, we can see that the share of temporary contracts for the group of workers aged 15-24 was already 37.5% in 1984 (as opposed to 15.6% for the whole population of workers). If we restrict this age group even further, the Spanish Labor Force Survey shows that temporary contracts represented almost 50% of total employment for the age group 16-19 already by 1987 (as opposed to 15.6% for the whole population).

[5] In particular, the 1997 reform reduced dismissal costs for unfair dismissals by about 25% and payroll taxes between 40% and 90% for newly signed permanent contracts and for conversions of temporary into permanent contracts after the second quarter of 1997 for workers under 30 years-old, over 45 years-old, the long-term unemployed, women under-represented in their occupations, and disabled workers (see, in this respect, Kugler et al. 2005)

Again, data on temporary contracts before the introduction of the 1984 reform for the group of young workers is difficult to find but the same report mentioned above (Mateos and Sebastián, 1988) contains data on the number of apprenticeship contracts (which represent one part of the group of temporary contracts) from 1981 to 1986 as well as data on the percentage of these contracts with respect to the group of employees in the non-agricultural private sectors aged 16-19. We can see in Figure 3 that the percentage of these contracts for the group of individuals aged 16-19 increased from around 2% before 1984 to 15-16% in 1985 and 21% in 1986. Again, this represents only one type of temporary contracts (apprenticeship contracts) and for one group of workers (non-agricultural private sector workers) but it is quite impressive how the use of these contracts has increased since 1984.

Therefore, as our aim in this paper is to disentangle the long-term effects of the 1984 reform we will use a regression discontinuity strategy comparing high-school dropout men who reach the labor market entry age of 16 before and after the reform. As we have seen that this group of individuals is the most likely to be affected by the 1984 reform, we will compare individuals that entered the labor market when temporary contracts were not widely available with individuals that had their first labor market experience when the main restrictions for the use of temporary contracts were abolished. Therefore, even if we do not have individual information on the type of contract held in the first job, we know from previous evidence that just 2% of young workers had a temporary contract before the reform while 21% of them had a temporary contract by 1986 and almost 50% by 1987. Our aim is to understand whether entering the labor market under two very different labor market situations (with or without widely available temporary contracts) had any effect on long-term labor market outcomes.

3 Data and empirical strategy

Data

This study will use the Continuous Sample of Working Lives ("Muestra Continua de Vidas Laborales", MCVL) which is a microeconomic dataset based on administrative records provided by the Spanish Social Security Administration. Each wave contains a random sample of 4% of all the individuals who had contributed to the social security system (either by working or being on an unemployment scheme) or had received a contributory benefit (such as permanent disability, old-age, etc.) during at least one day in the year the sample is selected. Hence, the sample is not including those individuals without any contact to Social Security in such a year. This may create some risks of sample selection, especially among women, immigrants or young workers. Hence, in order to minimize the potential selection effects, we combine the database for 7 waves, from 2006 to 2012. That is, we have everybody that had a relationship of at least one day during this seven year period with the Social Security administration. For them, we have their complete labor market history observed in the data. In our sample, we select the cohorts from 1960 until 1975 so that individuals are aged 31-52 during 2006-2012. Therefore, it is very rare for native men at these ages not to have any relationship of at least one day with the Social Security administration during a seven year period. This may be a potential problem, however, for women or immigrants in the sample as they have a more interrupted and discontinuous labor market trajectory. Therefore, we restrict the analysis to the native male sample.

There is information available on the entire employment and pension history of the workers, including the exact duration of employment, unemployment and disability or retirement pension spells, and for each spell, several variables that describe the characteristics of the job or the unemployment/pension benefits. There is also some information on personal characteristics such as age, gender, nationality and level of education.

Our sample includes all high-school dropout native males who were born between 1960 and 1975. We determine their year of labor market entry to be the year they turn 16 so that we can compare cohorts who experienced the same labor market conditions, except that some entered the labor market in a tightly regulated fixed term contracts regime while others entered the labor market in a laxer fixed term contract regime.[6,7] It is important to realize that we are not comparing a whole career spent under restrictive or lax fixed term contract use, but two careers, both exposed to lax fixed term contract regulation for most years, but that differ in the fixed term contract regulation in the first year of the career. As the reform was introduced in 1984, individuals born between 1960 and 1968 are used to determine what happens with a tight regulation of fixed-term contracts at entry whereas individuals born in 1969 and 1975 are used to assess what happens with a lax regulation of fixed-term contracts as they reach the age of labor market entry after the implementation of the reform. We include only high-school dropouts and exclude individuals who have higher

[6] The minimum legal age for working in Spain was 14 before 1980 and 16 since that year. In the model with aggregate data we include fixed effects that account for differences in labor market experience.

[7] As individuals can enter the labor market after the age of 16, we impose an extra sample restriction to include only those workers that begin working before age 21 in order to gain in sample homogeneity. Therefore, we interpret these results as an “intent-to-treat” estimation. In the robustness check section we implement models with diferent age restrictions.

levels of education because individuals with higher level of education enter the labor market well above age 16[8].

We have 21.676 native males in the sample, 13.349 workers born between 1960 and 1968 and 8.327 born between 1969 and 1975.[9] Descriptive statistics of the variables included for our sample can be found in Table 1.

We will look at the effect of the reform on several long-term labor market outcomes measured over the individual's labor market career and up to 2006. Although we have data until 2012, we take the year 2006 to evaluate the outcomes because we want to avoid the years of the deep financial and economic crisis in Spain, which began in 2008 and is still strongly affecting the economy.[10] These outcomes include the number of days worked, the number of non-permanent employment spells and the logarithm of the accumulated wage over the career.

Empirical strategy: Individual data

As explained in the previous sections, our strategy relies on the use of a regression discontinuity design. The year of birth is our running variable and we use the liberalization of fixed term contracts in 1984 as the cutoff point. We will compare high school dropout native males aged 16 in 1983 (cohort born in 1967) with high school dropout native males aged 16 in 1985 (cohort born in 1969). As explained above, the law was passed in August 1984 and was fully implemented from October 1984. Therefore, in our baseline model we have excluded the cohort born in 1968 as it was only partially affected by the law. In any case, we also estimate the same model considering the cohort of 1968 as the last pre-reform cohort and we also implement some robustness checks with trimester data excluding only the third and fourth trimester of the 1968 cohort.

We take individuals who are high-school dropouts and compare the labor market outcomes of those who turn 16 before the introduction of the reform to the outcomes of individuals

[8] Furthermore, it is very difficult to determine the age at which individuals typically finish higher levels of education, such as university. This represents another reason to restrict our sample to high school dropouts.

[9] We have a large number of missing values for wages because wages were not properly measured (or were missing) in this dataset before 1990. This is the reason why the number of individuals in the wage regressions is 14.747. These workers with no information on wages are basically those born in the 1960-1963 cohorts. If we reestimate our basic models for the 1964-1973 cohorts, we get almost the same results, although we need different trend specifications (third-order polynomial trend) to capture the dynamic of the model as a result of using fewer cohorts.

[10] A recent paper by Nagore and Van Soest (2014) finds that individuals who are most likely to be affected by the current economic crisis in Spain are males, those aged 16-24 and 40-51 years old and those who are less qualified or work in manual occupations. This result reinforces our idea of stopping the period of analysis in 2006 (before the economic crisis) as our sample is restricted to young high-school dropout men who are around their 40's during the current economic crisis.

who turn 16 after the reform is implemented. We analyze the effect of the reform on a set of long-term career variables measured at the individual level and in the same year, 2006. We stop the follow up period in the same year for all cohorts to ensure that workers who entered the labor market just before and just after the 1984 reform faced the same labor market conditions after the first few years in the labor market.

We adapt the identification strategy in Moulton (2011) and use the following regression discontinuity model to estimate the effect of reaching the labor market entry age when the generalization of fixed-term contracts is implemented:

$$
\begin{aligned}
&Outcome_i = \alpha + \beta_1(BirthYear_i - C) + \beta_2(BirthYear_i - C)\mathrm{I}(BirthYear_i \geq C) + \\
&\beta_3\mathrm{I}(BirthYear_i \geq C) + SectorFE_i + UnemRateEntry_j + \beta_4(BirthYear_i - C)^2 + \\
&\beta_5[(BirthYear_i - C)\mathrm{I}(BirthYear_i \geq C)]^2 + \varepsilon_i
\end{aligned} \tag{1}
$$

The model includes a linear and quadratic time trend consisting of a birth year cutoff (C) subtracted from the individual's birth year $(BirthYear_i - C)$ which estimates the trend in the outcome variable analyzed, for all birth cohorts. β_1 is the slope of the pre-cutoff trend. There is a second time trend $(BirthYear_i - C)\mathrm{I}(BirthYear_i \geq C)$ which includes an additional indicator variable that equals 1 when the individual is born at or after the cutoff. Therefore, the slope of the post-cutoff trend is $\beta_1 + \beta_2$. The variable that captures the effect of the policy is an indicator variable that equals 1 if the individual is born at or after the cutoff, $\mathrm{I}(BirthYear_i \geq C)$ so that β_3 is the shift in the intercept of the post-cutoff trend which represents the effect of the treatment. Therefore, β_3 represents the trend "jump". β_4 and β_5 introduce the quadratic time trend in the model and are included to increase the fit of the pre and post cutoff trends.[11] Specifically, our baseline specification includes the unemployment rate of the region (j) where the individual lives at the time of labor market entry (at age 16).[12] Some of the specifications also include dummies for the sector of the economy in which the individual works in 2006 (or in the last job if not working in 2006)

[11] We have also estimated the models with only a linear trend. However, the graphs of the real data and the predicted data from the model show that the fit of the model is much better with the quadratic than with the linear trend. Therefore, we only report the results with the quadratic trend. The graphs and regressions with the linear trends are available from the authors on request.

[12] We include the unemployment rate of 17 Autonomous Communities in Spain. This information is obtained from the Spanish National Institute of Statistics (INE). Ceuta and Melilla are included in the Andalusia region.

to better control for possible differential labor market characteristics for different sectors of the economy.[13]

In order for the effect to be correctly identified, regression discontinuity models require that the running variable (in our case the year of birth) cannot be manipulated so that the probability of being on each side of the discontinuity is exogenous. This is obviously satisfied in our sample for the year of birth. However, as we also select individuals that are high-school dropouts in order to assume that they enter the labor market at age 16, it could be the case that the reform affected the probability of individuals studying more or less. In this case, we would observe a jump in the number of individuals in our sample for the cohort entering the labor market after the implementation of the reform. We can see in Figure 4 that this is not the case, as the percentage of men of each cohort who are high-school dropouts, and therefore, are in our sample, declines over time. This is a steady decline due to the progressive increase in the amount of education for each subsequent cohort and we can observe that there is no discontinuity for individuals born in 1969. In Figure 5 we can see that the share of individuals who find their first job at age 15 or before (red line) strongly decreases for the cohorts analyzed here being this share almost zero for those born after 1965. A similar picture can be observed from Figure 6 which plots the average age of labor market entry by education. The reason for this is the change in the legal working age in 1980. But we can also observe in Figure 5 that the percentage of male high school dropouts finding their first job at age 16 (blue line) substantially increase for cohorts being born after 1968. This could be another consequence of the reform that liberalized the use of temporary contracts in 1984.

Empirical strategy: Aggregate data

Even if it uses individual level data, the previous approach has the problem that it does not properly take into account two types of factors. First, changes in labor market conditions that occurred through the analyzed period may have differentially affected different cohorts. Second, the change in the legal working age that was introduced in Spain (in 1980) before the liberalization of fixed-term contracts has affected the real experience accumulated by different cohorts.

Therefore, in an attempt to control for these two potential confounding factors that may be biasing our estimates in the individual level analysis, we follow Oreopoulos et al. (2012) and estimate a new specification in which we collapse the individual data at the level of

[13] A detailed description of the sectors of the economy included in the model is given in the Appendix.

birth cohort (c), calendar year (t) and years of real labor market experience (e) and we introduce these factors as fixed effects in the regression. We measure real labor market experience as the difference between calendar year and the year in which the worker had his first employment spell.[14] Therefore, we run the following specification (weighing by cell size):

$$\begin{aligned}\bar{y}_{ct} &= \alpha + \beta_1(BirthYear_c - C) + \beta_2(BirthYear_c - C)reform_c + \beta_3 reform_c + \\ &+ \beta_4(BirthYear_c - C)^2 + \beta_5(BirthYear_c - C)^2 reform_c + \phi_t + \gamma_e + u_{ct}\end{aligned} \qquad (2)$$

Where, θ_c is the cohort fixed effect, ϕ_t is a calendar year fixed effect and γ_e is a real labor market experience fixed effect. The coefficient of interest is, as in the model with individual data, β_3, the shift in the intercept of the post-cutoff trend which represents the effect of the treatment. Thus, this new specification allows us to estimate the effects after controlling for real experience as well as changes in macroeconomic conditions from 1984 until 2006. We estimate this model for the same outcomes as before. The interpretation of the results is a bit different than in the individual model. For example, in the case of earnings the results will tell us the effect of the reform (in percentages) on annual earnings (on average over the career) after accounting for macro variables and real labor market experience.

4 Results

Individual data

We are interested in evaluating the long-term impact of entering the labor market under a regime where fixed term contracts are easy for firms to use. Specifically, we would like to answer the following question: does greater availability of fixed term contracts lead to more days of employment and higher accumulated earnings in the long run?

In this section, we use a cohort regression discontinuity design with individual data, estimating the model described by equation (1).

We first investigate whether the reform led to a greater number of fixed-term contracts. In Table 2 we show that men who entered the labor market in 1985 accumulated all other things equal more non-permanent contracts between labor market entry and 2006 than men

[14] We also did some tests with a "potential experience" variable calculated as the distance to the year when the worker is first able to work (14 before 1980 and 16 afterwards) but results were very similar and we believed that real experience was capturing in a more detailed way the labor market history of the worker.

who entered the labor market in 1983. This effect is statistically significant: entering the labor market under a lax fixed term contract regulation increases the number of non-permanent contracts by between 1.27 and 1.92, depending on whether we control for sector dummies or not.[15] This result implies that men who entered the labor market under lax fixed term contracts regulation were more likely to be on non-stable employment even years after their entry into the labor market.

Before examining long-run employment effects, we start by showing the impact of the introduction of fixed term contracts on the initial labor market insertion of youth. Labor market conditions were deteriorating rapidly prior to the reform (Figure 1). Thus, each cohort had a lower probability of entering the labor market by age 19 than the previous cohort (Figure 7).[16] However, just at the time when the liberalization of fixed term contracts took place (in 1984), this trend was reversed as the data in Figures 5 and 6 also showed us. From 1984 onwards, each subsequent cohort had a greater probability of being in the labor market by age 19 than the previous. Is this change due to the reform introduced in that year? Table 2 is saying that, once we control for a quadratic term in the dependent variable, the effect of the policy change is estimated to be positive and significantly different from zero. Those low educated native male workers who began working just after the 1984 reform are 14% more likely to work before age 20. Thus, we may conclude that the reform facilitated the integration in the labor market for young people, something that was more difficult before 1984.

We now look at employment outcomes over the long-run. Results for the number of days worked show that men who entered the labor market under a lax fixed term contract regulation accumulated, all other things equal, fewer days of employment from their entry into the labor market and up to 2006. This can be seen both in graphs (Figure 8) and in the regressions. The regression in Table 3 indicates that entering the labor market under a lax regulation of fixed term contracts results in around 315 fewer days worked (339 days if sector dummies are not considered). This happens because, as shown above, under a lax

[15] In order to capture a deeper understanding about instability in labor market careers, we include in this measure not only all fixed-term contracts but also all self-employment and special regimes' employment spells. The latter deals with employment in agriculture, fisheries, training programmes, etc.

[16] Note that the declining trend showed in this and the following graphs is due to each cohort having less labor market history by 2006 because each cohort is one year younger than the previous one. The key point for identifying the effect of the reform in these long-run labor market outcomes is to analyze whether there is a jump up or down in the outcome variable for the generation entering the labor market under laxer fixed term contract regulation.

fixed term contract regulation, more fixed term contracts are used, which means that people lose employment more often. [17]

In the specification above, we include all cohorts from 1960 until 1975 except the 1968 cohort (as it was partially affected by the policy change). However, the reform was approved in August of 1984 and the law was binding only from October 1984 so that the cohort of 1968 was hardly affected by the reform. Thus, we run the same model including this cohort as the last pre-reform cohort. The results of this model are also reported in Table 3 and we can see that the size of the effect slightly decreases and we get an estimate of 201 days less of employment for the cohort born in 1969 with respect to the cohort of 1968. In order to make sure that the trends are correctly captured even if we exclude one or more cohorts and that we are correctly estimating the effect of the reform, in the second column of Table 3 we can see that when we exclude an extra cohort (1969) the size of the estimated effect is very similar (306 days of employment) than the results excluding only the partially affected cohort.

Apart from employment we also look at accumulated earnings up to 2006. Graphs (Figure 9) and regressions (Table 4) both show that men who entered the labor market under a laxer regulation of fixed term contracts accumulated lower earnings.[18] The earning loss is estimated to be 18.3% of total accumulated wages when we consider all cohorts in the model (first column of Table 4). As before, when we exclude the cohort of 1968 in order to get a clearer effect of the reform by comparing a cohort that was completely affected by the reform with a cohort that was completely unaffected by the reform, we get a stronger effect of 36% of total accumulated wages.[19] Can this earning loss be explained by the lower days of employment accumulated by the cohort entering the labor market during a lax fixed-term contract regulation (cohort born in 1969)? If we estimate the effect of the reform in percentage points, we get that the reform made affected workers to spend 7% less days in employment. Given the estimated wage gap, we can see that about 1/5 of the large earnings loss experienced by the cohort entering the labor market after the reform could be explained by the fewer days of work accumulated by this cohort. The rest might be

[17] We have also estimated an additional model, available upon request, that computes the effect of the reform on the total number of employment spells. This model predicts between 1.27 and 1.92 more employment spells for those workers beginning working after 1984.

[18] Among individuals with a fixed-term contracts some differences in wage growth patterns can also exist as there are diferent types of fixed-term contracts with diferent durations. In this line, the paper by Amuedo-Dorantes and Serrano-Padial (2007) shows that job-stayers with fixed-term contracts are more able to narrow their wage gap with respect to workers with permanent contracts (than job-movers with fixed-term contracts).

[19] Again the results are very stable when we exclude an extra cohort and the wage loss is estimated to be 34%.

connected to the larger number of work interruptions found in Table 2 among treated workers[20] and also to the well-known wage gap between permanent and temporary workers observed in Spain since the beginning of the nineties (See Jimeno and Toharia, 1993). Furthermore, as documented in García Pérez and Rebollo (2005), Spanish workers suffer important wage losses (of about 11%) due to involuntary job interruptions.

We now perform some robustness tests for our main results. As explained above, in our sample we include all native male who are high-school dropouts as we determine their year of labor market entry to be the year they turn 16. However, it could be the case that some of the individuals in our pre-reform group do not effectively start working at the age of 16 even if they are high-school dropouts. If that is the case, some of the individuals in our pre-reform group may have begun working at later ages once the reform was already adopted.[21] In this sense, our results above may be interpreted as an "intent-to-treat" lower bound estimate of the effect of the policy. Therefore, in order to explore the robustness of our results to delayed labor market entry, we perform two additional tests: first, we further restrict the sample to include only those individuals who began working before the reform (for our pre-reform cohort) or after the reform (for our post-reform cohorts) at the ages of 14-17 (first panel in Tables 5 and 6). As expected, the results for both the accumulated number of days worked (Table 5) and accumulated wages (Table 6) become slightly stronger. In the last column of Table 5 we can see that the number of days worked decreases by 348 days (212 if we include all cohorts; see second column of Table 5) and in the last column of Table 6 we can see that accumulated wages decrease by 54% (21% if we include all cohorts; see second column of Table 6).[22] Second, we apply an even stricter criterion and we include only those individuals in the control group that are effectively observed as working before the implementation of the reform in 1984 (second panel in Tables 5 and 6). The results of this second test show much stronger effects of the reform for both accumulated number of days worked (906 days less of employment; see the last column of the second panel in Table 5) as well as for accumulated wages (50%; see the last column of the second panel in Table 6). This last set of results have to be taken with caution as, when imposing extra restrictions to our sample, we may be incurring in sample selection bias as our treatment and control groups may not be formed by similar individuals. In any case, the results of these two tests can be interpreted as an upper bound

[20] The result in footnote 17 is also supporting this view.
[21] In fact, as we can identify the age of the first labor market experience, we know that only 15% of male high-school drop-outs in the 1967 cohort get a first job at age 16.
[22] The effect over the number of non-permanent employment spells almost trippled in this restricted sample.

estimate of the effects of the policy as the point estimates in our baseline results are likely to be attenuated due to the fact that some workers belonging to cohorts we labeled "pre-reform" only entered the labor market after the reform.

Aggregate data

Part of the effects that we find when using individual data can be due to differences in the labor market conditions from 1984 to 2006 that could be affecting in a different way cohorts before and after the reform. Apart from that, we could also think that pre-reform cohorts can be affected, not only by the liberalization of fixed-term contracts in 1984 but also by the change in the minimum working age introduced in 1980, which changes the amount of labor market experience that these cohorts can accumulate. Therefore, in order to control for these two potential confounding factors, we estimate an alternative specification with aggregate data for each cohort, calendar year and years of real labor market experience and include these three factors as fixed effects in the regression. We thus estimate the specification described by equation (2) above.

Tables 7, 8 and 9 confirm our previous results and point to the existence of a slight upward bias (i.e. overestimating the impact of the reform) when using individual data without controlling for time and experience effects. Therefore, the results of the models with aggregate data and controls for time and experience fixed effects show lower effects of the reform than the previous results with individual data. For days worked, we can see in table 7 that we get an estimate of 8.8 days of employment less, on average, for each of the 22 years of our sample (from 1984 to 2006) between the pre-reform cohort and the post-reform cohort. This corresponds to 193 fewer employment days[23], which is slightly lower than the 315 days estimated with the model with individual data. As before, when we include all cohorts we get milder results; in particular we estimate that the reform decreased by 132 the number of days worked over the entire period (6 days on average per year by 22 years). In fact, if we perform the same estimation with aggregate data but without any fixed effect we obtain that the reform lowered the number of days worked by 8.67 days on average (Table 7), which results in 190.7 days accumulated over our sample period. Now, this estimate is extremely close to the 201 days obtained with the model with individual data including all cohorts and no time or experience controls. Similarly, Table 8 shows that the first post-reform cohort has, on average, 22% lower earnings when controlling for time and experience fixed effects, which represents a smaller earnings loss

[23] 8.8 multiplied by 22 years gives an average of 193 days of employment.

than the one estimated with individual data and no fixed effects (36%). When we include all cohorts in the estimation, the wage loss is reduced to 13% as a result of the introduction of the reform (with individual data the number was 18%). In this case also, if we perform the estimation with aggregate data and no fixed effects, we obtained a wage loss of 15.3% which is closer to the 18% obtained with the model with individual data, including all cohorts and no fixed effects. Finally, Table 9 shows that the first post-reform cohort has, on average, 0.055 more non-permanent contracts per year, between labor market entry and 2006, than the last pre-reform cohort. This corresponds to an average of 1.21 more non-permanent contracts between labor market entry and up to 2006. Again, these results after controlling for labor market conditions during these years and differences in the age of labor market entry are very similar to the result obtained with individual data, between 1.27 and 1.92.[24]

We next explore the timing of the impact of the liberalization of fixed-term contracts: are the impacts equal across workers' careers or are they concentrated in the early years? To address this question, we re-estimate each model by interacting the reform dummy with years of experience (last column in Tables 7-9). In this case, the effect of the reform is found to be non-significant but the interactions show that this effect is present during at least the first 5-9 years of experience. Indeed, Figure 13 shows that the effect of the reform on the number of days worked is between 15 and 42 days per year during the first 5 years of work experience. The same is obtained for annual earnings. In this case the wage penalty associated to the reform is present during the first 9 years of experience (Figure 14). Finally, the effect of the reform on the number of fixed-term employment spells (about 0.04) is quite permanent during at least 20 years of experience (Figure 15). [25]

5 Discussion and robustness checks

Our results differ from some of the findings in prior literature. In particular, our results differ from the ones in Aguirregabiria and Alonso-Borrego (2014) in the sense that they find an increase in job turn-over, as we do, but also an increase in total employment. This may be due to the fact that they analyze the firm's maximization problem for a sample quite

[24] These results correspond to Figures 10, 11 and 12 for days worked, wages and non-permanent employment respectively.

[25] We have also found an increase in the probability of suffering from a working accident whereas the probability of entering the permanent disability system is not affected by the reform. These results are obtained using a different dataset (register data on work accidents, coming also from the Social Security administration), Results are available upon request.

different from the one we have.[26] They find that these firms react to the reform by substituting permanent workers by temporary ones, all this process resulting in a 3.5% increase in total employment but a null effect on productivity. They also find an increase in turnover what makes easier to exit from unemployment, especially for high-qualified workers who may be working more in equilibrium as the former paper finds. But, at the same time, this process makes it much easier for low-skilled workers to be unemployed, which results in a less stable career for this type of workers (the ones we analyze in this paper). Thus, in a similar flavor to the results from Aguirregabiria and Alonso-Borrego (2014), we do find an increase in employment at earlier ages, that we interpret as the reform facilitating the initial labor market insertion of young workers. However, we also find that the reform fostered a more unstable labor market career for low-skilled workers due to the increase in the number of non-permanent employment spells. Therefore, we go a step further from the current literature in providing evidence of the negative long-term effects of the introduction of temporary contracts.

To test the robustness of our results and in order to get one step closer to the idea behind the regression discontinuity literature, we perform the same estimations with aggregate data but at the trimester instead of at the yearly level and we include controls for trimesters and experience fixed effects. This method allows us to compare even more similar individuals and to drop only the third and fourth trimester of the 1968 cohort instead of disregarding the observations of this entire cohort.[27] As we increase the number of observations, we do not have to include all cohorts from 1960 to 1975 and, thus, we only include cohorts from 1965 to 1971. Table 10 shows the results for the number of days worked when we include all trimesters in the regression. We can see that the effect of the reform after controlling for time and experience fixed effects is 5.8 days less of employment, which is almost the same effect than with the yearly data (5.9). Therefore, this similarity between both results with more or less cohorts included reinforces the idea that we are able to capture the trends in a very detailed manner. When we run the model but excluding the third and fourth trimester of the 1968 cohort we get a decrease in days worked of 10.4 days per year which represents a stronger effect than when we exclude the whole 1968 cohort (8.78).

[26] They analyze the results of introducing temporary contracts with a sample of 2,356 Spanish manufacturing firms during the period 1982-1993. Hence, they focus on a group of firms larger than the average for the Spanish economy and where the typical worker is a medium-high qualified one with a permanent contract, both before and after the 1984 reform. As Table 1 shows, we have just 17.4% of workers in our sample working in manufacturing firms.

[27] As we know that the reform was passed in August 1984 and came into force from October 1984.

A somehow different picture arises for earnings when we run the model with trimester instead of yearly data. As we can see in Table 11, the results are negative and highly significant but the effect is half the size of the results with yearly data. When we exclude some of the cohorts and include only more similar individuals we find that the wage loss due to the reform amounts to 6.2% or to 10.8% if we exclude the third and fourth trimester of the cohort of 1968.

Finally, we perform a number of placebo tests in which we simulate the policy to take place in a number of pre and post reform years (from 1966 to 1972). We use the specification from equation (2) with aggregate data. The results of these regressions for both the accumulated number of days worked and accumulated wages for the baseline sample as well as for the sample restricted to include only those who begin working before the reform in the control group are plotted in Figures 16 to 19. As can be seen in these figures, there is no clear effect when we simulated that the reform first affected the cohorts that turned 16 before 1984. This result is especially strong when we restrict the sample to include only those workers that effectively entered the labor market before the reform. These placebo tests reinforce our conclusion that the liberalization of fixed-term contracts use had a negative effect on the long-run employment and earnings outcomes for male high school dropouts.

6 Conclusion

In this paper, we have investigated the impact of entering the labor market under a lax regulation of fixed-term contracts on subsequent labor market outcomes. Using Spanish social security data, we found that cohorts of native male high school dropouts who entered the labor market under a lax regulation of fixed-term contracts experienced worse labor market outcomes than cohorts that just preceded them. Specifically, entering the labor market under a lax regulation of fixed-term contracts leads to 193 (7%) fewer accumulated days at work and a 22% reduction in accumulated wages in the long run. This is the case despite the fact that the reform increased the probability of working before age 20. The source of those negative effects on long-term outcomes is likely to be the larger probability of working under non-permanent contracts well after labor market entry, which also makes it more likely to be subject to the well known wage penalties due to unemployment and temporary work (see, for example, Jimeno and Toharia (1993), García Pérez and Rebollo (2005) or Fernández-Kranz et al. (forthcoming)).

Hence, our findings suggest that, on balance, making fixed-term contracts more readily available reduced the welfare of low skilled workers. Even though these contracts seem to allow low skilled workers to secure a first job more rapidly, the long-run consequences are negative. By promoting less stable employment, fixed-term contracts ultimately reduce the long-run employment and earnings prospects of low skilled workers. Far from being a stepping stone, fixed-term contracts are a stumbling block for the career of low skilled workers.

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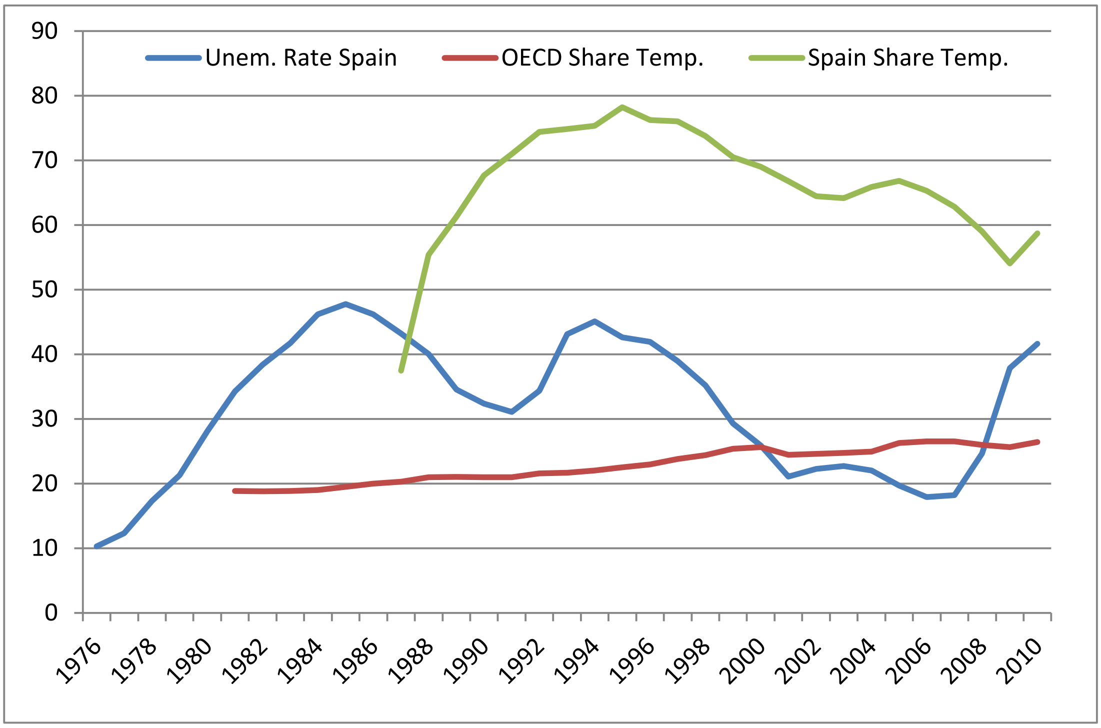
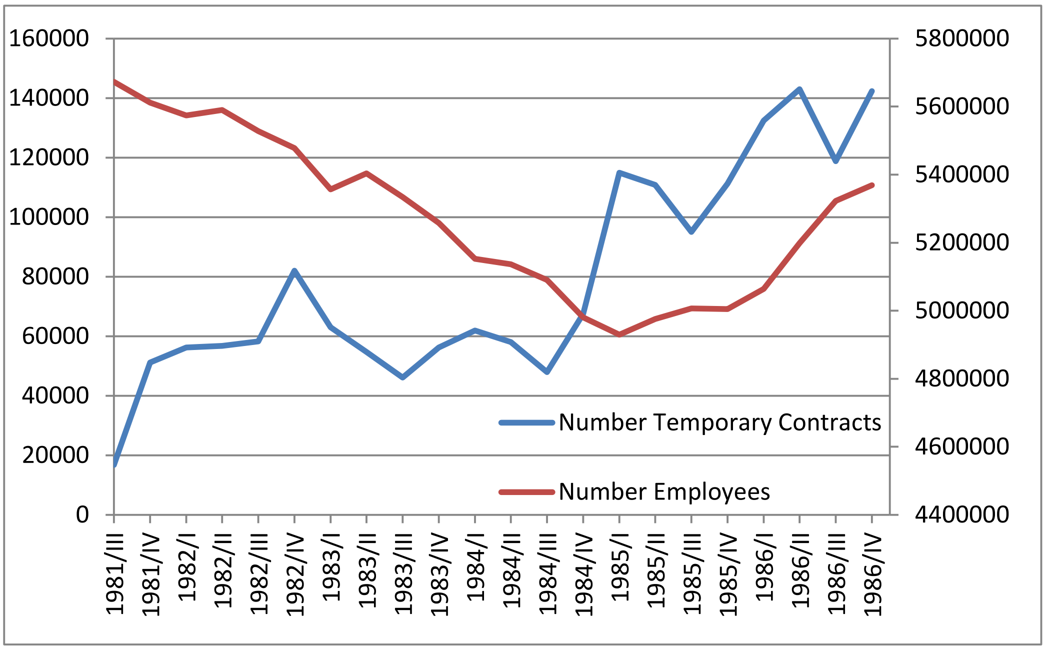
Figure 1. Unemployment rate in Spain for workers with less than 25 years old; share of temporary contracts for men aged 15-24 in Spain and average share of temporary contracts for men aged 15-24 in the OECD.



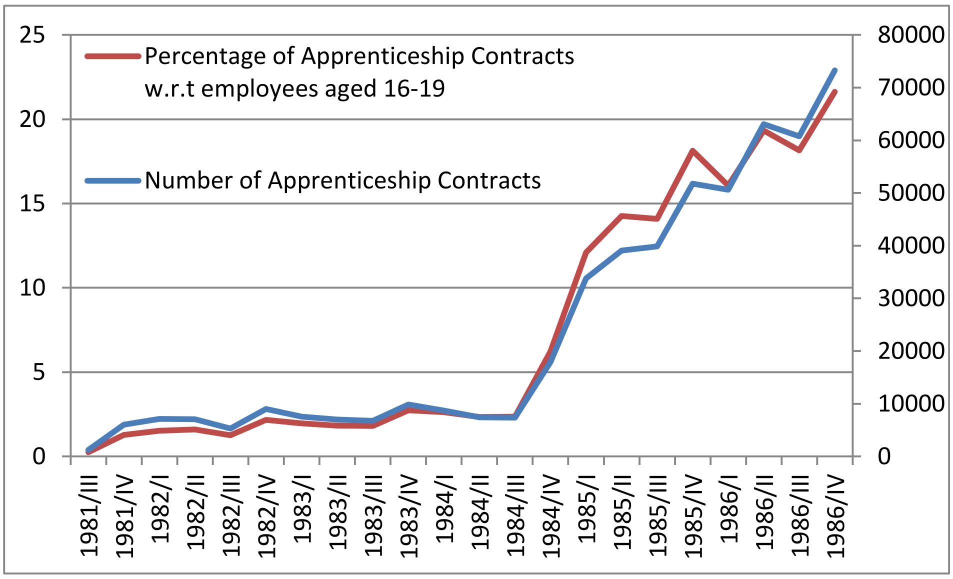
Source: Own elaboration from data from the OECD and the Spanish National Institute of Statistics (EPA).

Figure 2. Total number of temporary contracts and total number of employees in non-agricultural private sectors.



Source: Own elaboration from data from the Spanish National Institute of Statistics (EPA).

Figure 3. Number of apprenticeship contracts and percentage of apprenticeship contracts with respect to employees in non-agricultural private sectors aged between 16 and 19 years old.



Source: Own elaboration from data from the Spanish National Institute of Statistics (EPA).

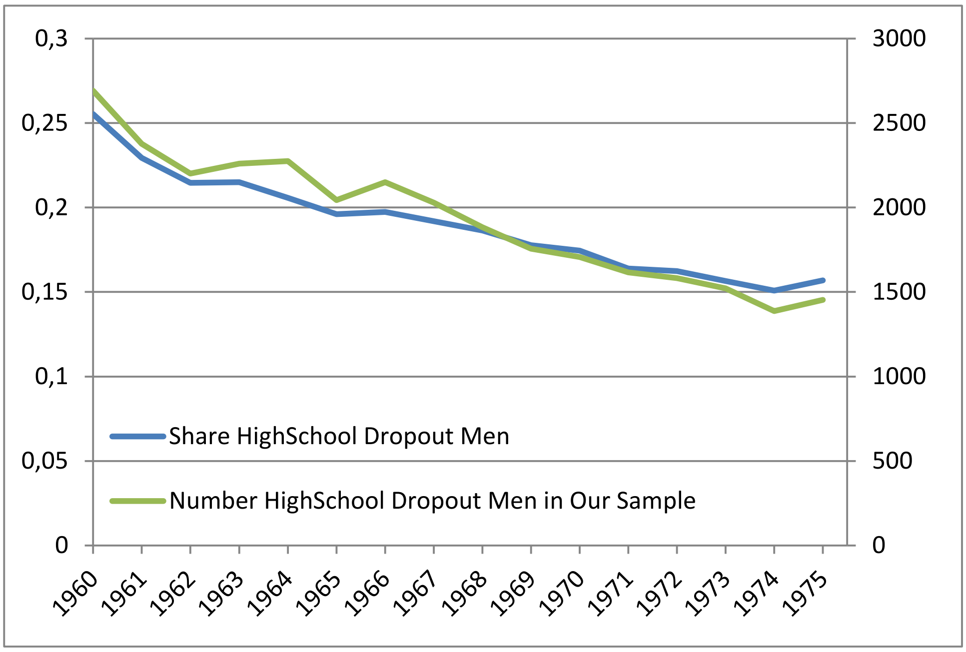
Table 1. Descriptive Statistics[28].

Variable	Obs	Mean	Std. Dev.	Min	Max
Unempl. Rate Region at Entry	21676	16.13	7,68	1,42	34,59
Andalucia	21676	0,243	0,429	0	1
Aragon	21676	0,018	0,136	0	1
Asturias	21676	0,017	0,132	0	1
Baleares	21676	0,021	0,144	0	1
Canarias	21676	0,067	0,251	0	1
Cantabria	21676	0,010	0,102	0	1
Cast.Leon	21676	0,049	0,216	0	1
Cast.Mancha	21676	0,057	0,233	0	1
Catalunya	21676	0,136	0,343	0	1
Valencia	21676	0,129	0,335	0	1
Extremadura	21676	0,040	0,196	0	1
Galicia	21676	0,056	0,230	0	1
Madrid	21676	0,079	0,270	0	1
Murcia	21676	0,032	0,176	0	1
Navarra	21676	0,007	0,087	0	1
PaisBasco	21676	0,022	0,149	0	1
Rioja	21676	0,005	0,072	0	1
High Tech. Manufacturing	21676	0,093	0,290	0	1
Low Tech. Manufacturing	21676	0,071	0,257	0	1
High Tech. Services	21676	0,034	0,183	0	1
Trade	21676	0,122	0,327	0	1
Hotels and Catering services	21676	0,063	0,243	0	1
Other Low Tech. Activities	21676	0,103	0,304	0	1
Construction	21676	0,331	0,470	0	1
Public Admin. & Real State	21676	0,028	0,166	0	1
Missing	21676	0,014	0,120	0	1

Source: Muestra Continua de Vidas Laborales (MCVL).

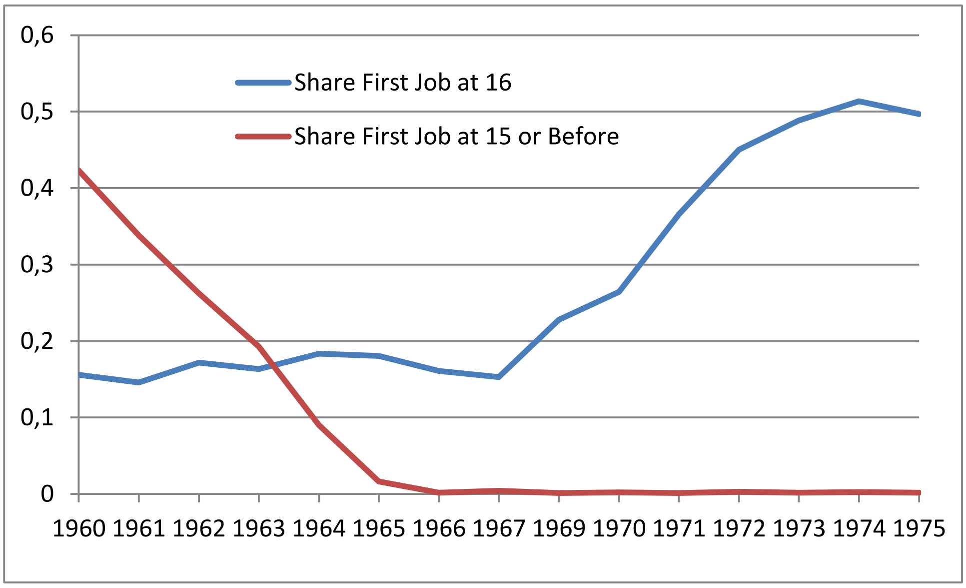
[28] See table 12 in the appendix for a more detailed explanation of each sector.

Figure 4. Total number of men in our sample for each birth cohort and share of males who are high-school dropouts for each cohort.



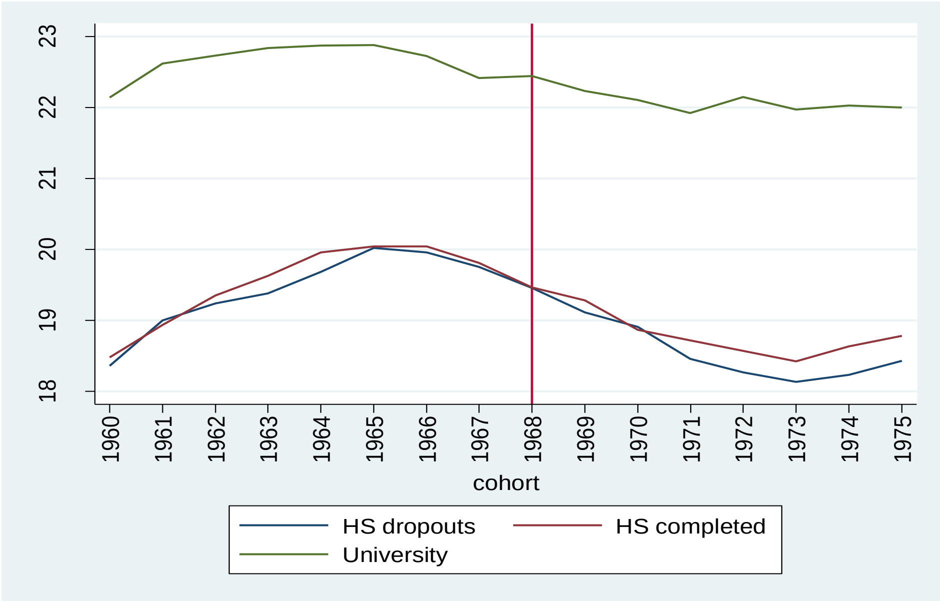
Source: Muestra Continua de Vidas Laborales (MCVL).

Figure 5. Share of individuals who find the first job at age 16 by cohort and share of individuals who find the first job at age 15 or before by cohort.



Source: Muestra Continua de Vidas Laborales (MCVL).

Figure 6. Age at labor market entry by education.



Source: Muestra Continua de Vidas Laborales (MCVL).

Table 2. Accumulated number of non-permanent employment spells from labor market entry until 2006 and probability of working before age 19, individual-level data.

	Number of non-permanent employment spells	Number of non-permanent employment spells	Prob. Working before age 19	Prob. Working before age 19
effect	1.92***	1.27***	0.14***	0.14***
	(0.60)	(0.34)	(0.04)	(0.04)
trend	-1.46***	-1.11***	-0.04*	-0.04*
	(0.23)	(0.12)	(0.02)	(0.02)
posttrend	1.45***	1.00***	0.12***	0.12***
	(0.27)	(0.15)	(0.03)	(0.03)
trend2	-0.08***	-0.06***	-0.00***	-0.00***
	(0.02)	(0.01)	(0.00)	(0.00)
posttrend2	0.03	0.02	-0.00***	-0.01***
	(0.02)	(0.02)	(0.00)	(0.00)
urentry	0.25***	0.16***	-0.02***	-0.02***
	(0.04)	(0.03)	(0.01)	(0.01)
sector2		X		X
Constant	4.58***	-1.22	0.94***	0.84***
	(1.25)	(1.74)	(0.16)	(0.19)
Observations	21,674	21,674	21,746	21,746
R-squared	0.08	0.26	0.11	0.13

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Number of days worked until 2006, individual-level data.

	ALL COHORTS		EXCLUDING 68&69 COHORTS		EXCLUDING 1968 COHORT	
effect	-205.21***	-201.30***	-348.08***	-306.54***	-339.34***	-315.26***
	(69.23)	(59.67)	(65.63)	(56.37)	(57.55)	(54.37)
trend	-173.37***	-165.72***	-125.01***	-124.58***	-124.99***	-124.60***
	(29.89)	(27.12)	(26.91)	(25.99)	(26.90)	(25.99)
posttrend	-179.35***	-183.50***	-222.68***	-230.49***	-228.17***	-225.02***
	(44.08)	(37.83)	(54.23)	(44.36)	(42.45)	(37.76)
trend2	6.72***	7.43***	10.54***	10.68***	10.54***	10.68***
	(2.05)	(1.87)	(1.70)	(1.66)	(1.70)	(1.66)
posttrend2	10.10**	9.33**	5.63	6.84	6.34	6.13*
	(4.55)	(3.47)	(6.25)	(4.24)	(4.43)	(3.33)
ur	-42.12***	-43.13***	-42.40***	-43.39***	-42.40***	-43.38***
	(7.46)	(6.97)	(7.53)	(7.04)	(7.53)	(7.03)
Sector		X		X		X
Constant	6,040.96***	4,617.74***	6,181.09***	4,735.86***	6,181.27***	4,735.73***
	(204.25)	(188.00)	(209.45)	(180.52)	(209.32)	(180.43)
Observations	21,676	21,676	21,676	21,676	21,676	21,676
R-squared	0.38	0.43	0.38	0.43	0.38	0.43

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4. Accumulated earnings (logs) until 2006, individual-level data.

	ALL COHORTS		EXCLUDING 68&69 COHORTS		EXCLUDING 1968 COHORT	
effect	-0.20***	-0.18**	-0.35***	-0.34***	-0.37***	-0.36***
	(0.07)	(0.07)	(0.08)	(0.06)	(0.07)	(0.06)
trend	0.05	0.04	0.11***	0.10***	0.11***	0.10***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
posttrend	-0.16***	-0.15***	-0.24***	-0.23***	-0.23***	-0.22***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
trend2	0.00*	0.00*	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
posttrend2	0.01**	0.01**	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
ur	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Sector		X		X		X
Constant	12.32***	12.16***	12.51***	12.34***	12.50***	12.34***
	(0.08)	(0.11)	(0.09)	(0.12)	(0.09)	(0.12)
Observations	14,793	14,747	14,793	14,747	14,793	14,747
R-squared	0.04	0.08	0.05	0.08	0.05	0.08

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5. Number of days worked until 2006, individual-level data. Sample restricted to include only those who began working at ages 14-17 or to include in the control group only those who begin working before the reform.

	ALL COHORTS		EXCL. 68&69 COHORTS		EXCL. 1968 COHORT	
Age First Job 14-17	-296.86***	-212.67**	-384.48***	-257.24*	-452.30***	-347.97**
	(100.53)	(95.21)	(125.87)	(131.70)	(116.17)	(129.88)
Control Group: First Year Employment Before Reform	-921.75***	-887.81***	-954.67***	-897.57***	-946.40***	-905.91***
	(77.60)	(75.26)	(107.41)	(104.91)	(99.69)	(100.32)

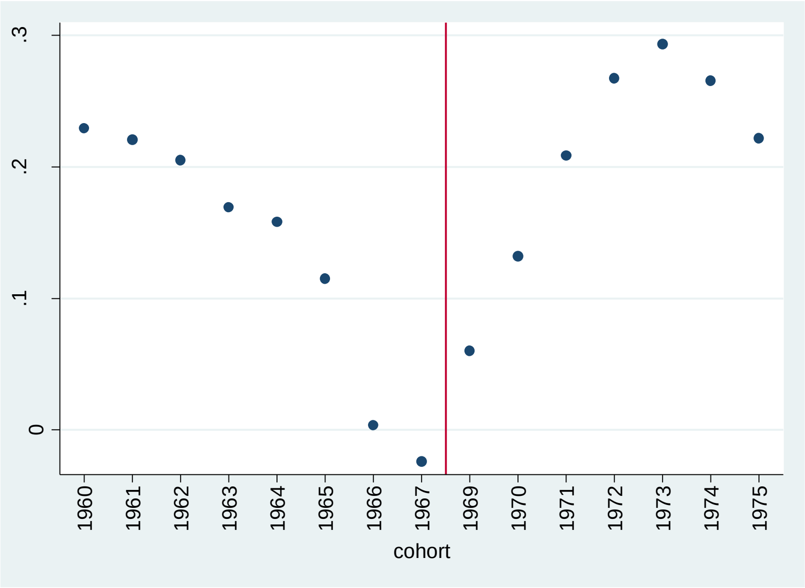
Note: The regressions include linear and quadratic pre and post-reform trends and controls for the regional unemployment rate at the time of entry. The first column of each outcome does not include dummies for the sector of the economy while the second column of each outcome includes sector dummies.

Table 6. Accumulated earnings (logs) until 2006, individual-level data. Sample restricted to include only those who began working at ages 14-17 or to include in the control group only those who begin working before the reform.

	ALL COHORTS		EXCL. 68&69 COHORTS		EXCL. 1968 COHORT	
Age First Job 14-17	-0.22**	-0.21**	-0.50***	-0.44***	-0.57***	-0.54***
	(0.08)	(0.08)	(0.06)	(0.05)	(0.03)	(0.04)
Control Group: First Year Employment Before Reform	-0.43***	-0.40***	-0.49***	-0.47***	-0.51***	-0.49***
	(0.07)	(0.08)	(0.05)	(0.05)	(0.04)	(0.05)

Note: The regressions include linear and quadratic pre and post-reform trends and controls for the regional unemployment rate at the time of entry. The first column of each outcome does not include dummies for the sector of the economy while the second column of each outcome includes sector dummies.

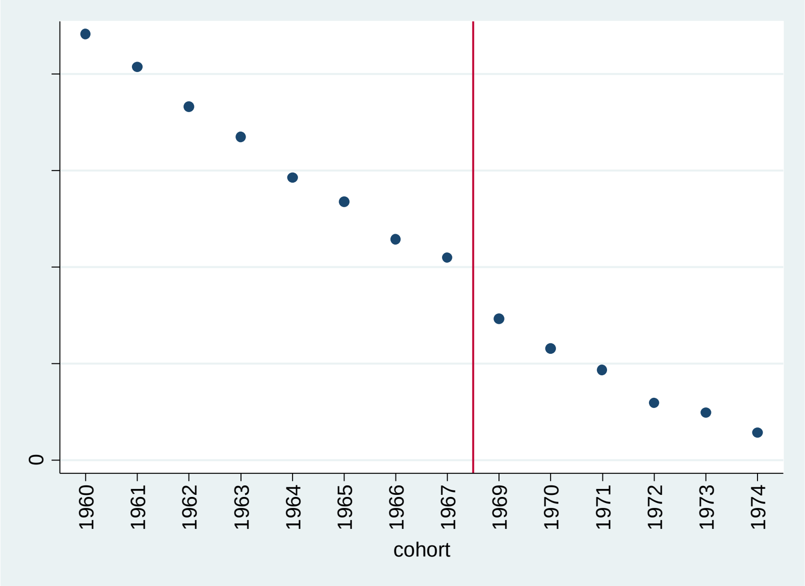
Figure 7. Plot of Beta coefficients for the probability of having the first job at age 19 or before.



Note: The coefficients are from a regression of the probability of having the first job at age 19 or before with dummies for the cohorts 1960-1975 (excluding the cohort of 1968).

Source: Muestra Continua de Vidas Laborales (MCVL).

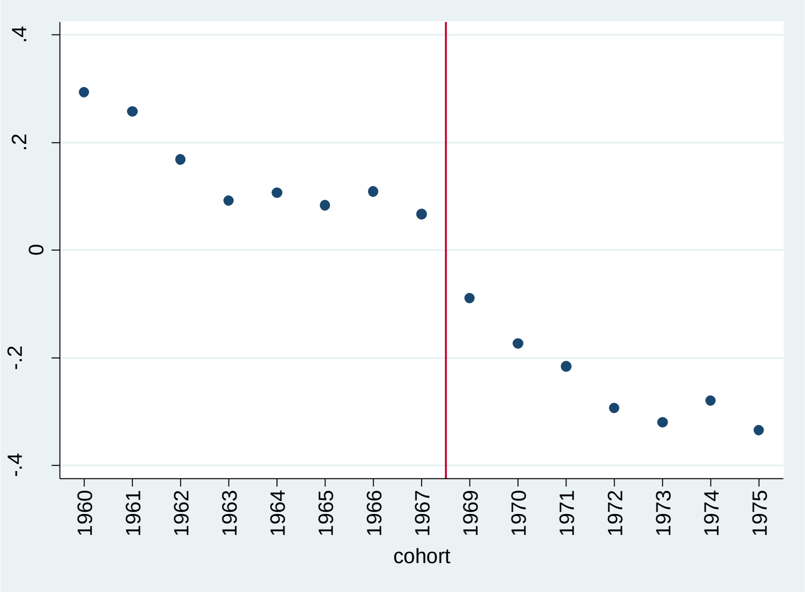
Figure 8. Accumulated number of days worked until 2006. Beta coefficients of the cohort dummies.



Note: The coefficients are from a regression of the accumulated number of days worked until 2006 with dummies for the cohorts 1960-1975 (excluding the cohort of 1968).

Source: Muestra Continua de Vidas Laborales (MCVL).

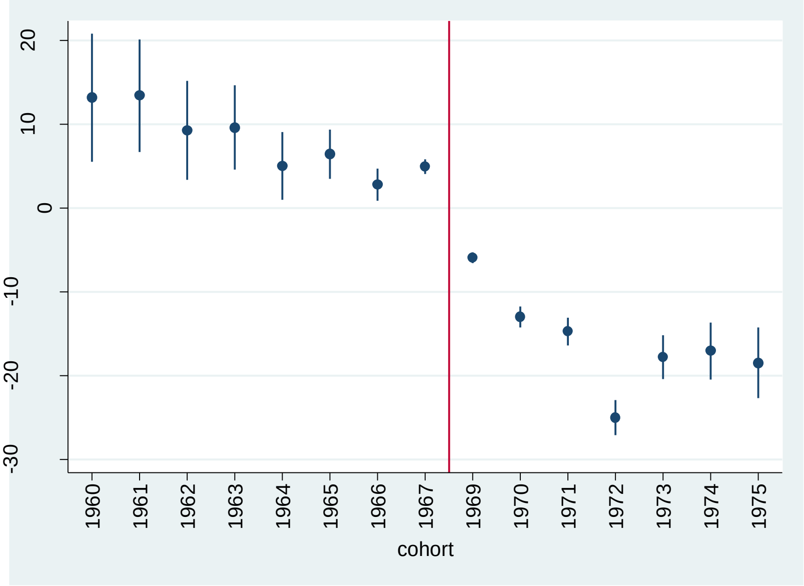
Figure 9. Logarithm of accumulated earnings until 2006. Beta coefficients of the cohort dummies.



Note: The coefficients are from a regression of the logarithm of accumulated wages until 2006 with dummies for the cohorts 1960-1975 (excluding the cohort of 1968).

Source: Muestra Continua de Vidas Laborales (MCVL).

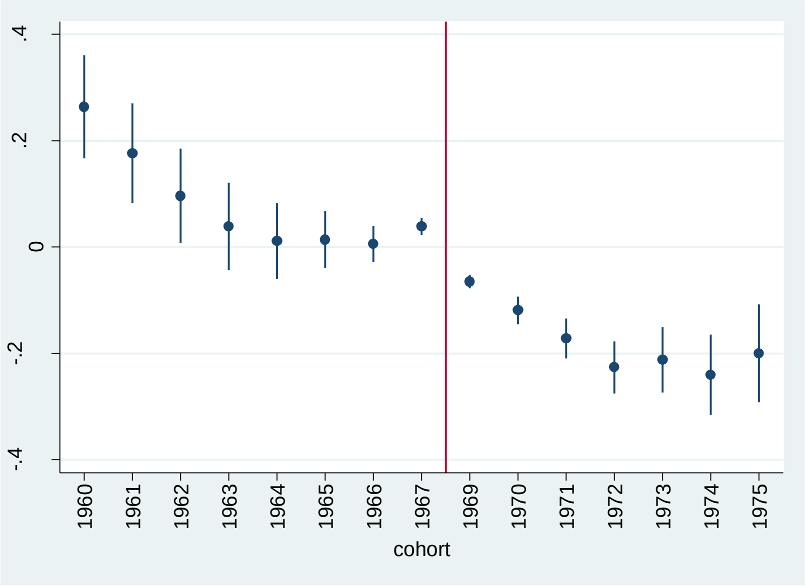
Figure 10. Annual number of days worked, aggregate data. Beta coefficients for the cohort dummies.



Note: The coefficients are from a regression of the number of days worked for the cohorts 1960-1975 (excluding the cohort of 1968) and controlling for real experience.

Source: Muestra Continua de Vidas Laborales (MCVL).

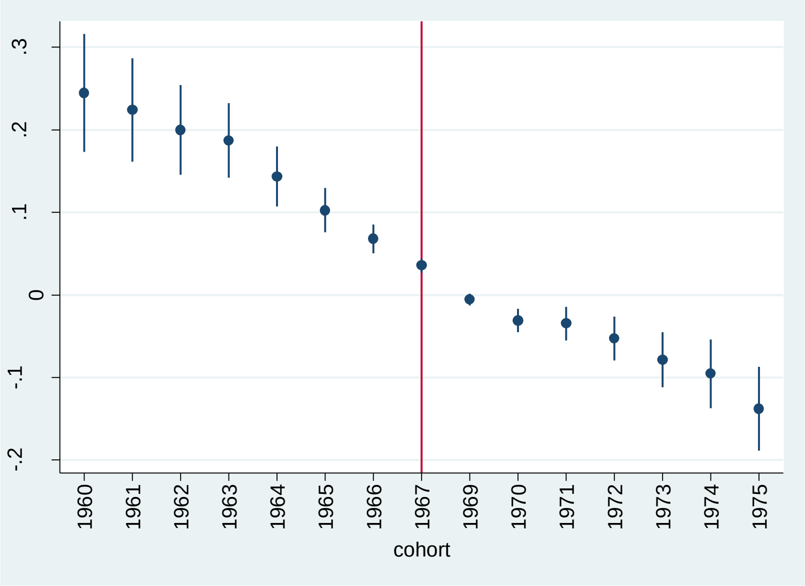
Figure 11. Logarithm of annual earnings, aggregate data. Beta coeficients for the cohort dummies.



Note: The coefficients are from a regression of the logarithm of annual wages for the cohorts 1960-1975 (excluding the cohort of 1968) and controlling for real experience.

Source: Muestra Continua de Vidas Laborales (MCVL).

Figure 12. Annual number of non-permanent employment spells, aggregate data. Beta coeficients for the cohort dummies.



Note: The coefficients are from a regression of the annual number of non-permanent employment spells for the cohorts 1960-1975 (excluding the cohort of 1968) and controlling for real experience.

Source: Muestra Continua de Vidas Laborales (MCVL).

Table 7. Number of days worked per year; collapsed dataset.

	No FE	Time FE	Experience FE	Experience & Time FE	+ Excluding 68&69 cohorts	+ Excluding 1968 cohort	interactions with Experience
effect	-8.670***	-5.238**	-9.841***	-5.971**	-9.443*	-8.779**	-3.569
	(2.590)	(2.065)	(2.916)	(2.350)	(4.772)	(3.722)	(5.328)
reformrex1							-15.772**
							(6.730)
reformrex2							-22.489**
							(8.662)
reformrex3							-31.751***
							(7.594)
reformrex4							-42.019***
							(7.031)
reformrex5							-18.738**
							(6.403)
Constant	257.469***	65.701***	334.642***	188.594***	195.963***	196.006***	191.591***
	(2.499)	(1.827)	(2.969)	(12.989)	(13.990)	(13.948)	(13.660)
Observations	2,080	2,080	2,080	2,080	2,080	2,080	2,080
R-squared	0.033	0.686	0.712	0.822	0.822	0.822	0.831

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8 . Logarithm of yearly earnings; collapsed dataset.

	No FE	Time FE	Experience FE	Experience & Time FE	+ Excluding 68&69 cohorts	+ Excluding 1968 cohort	interactions with Experience
effect	-0.153***	-0.109***	-0.171***	-0.130***	-0.201***	-0.220***	-0.125***
	(0.041)	(0.031)	(0.047)	(0.040)	(0.021)	(0.018)	(0.030)
reformrex1							-0.318***
							(0.051)
reformrex2							-0.167**
							(0.075)
reformrex3							-0.158**
							(0.068)
reformrex4							-0.251***
							(0.055)
reformrex5							-0.147**
							(0.050)
reformrex6							-0.158***
							(0.044)
reformrex7							-0.138***
							(0.039)
reformrex8							-0.090**
							(0.032)
reformrex9							-0.043*
							(0.024)
Constant	9.196***	7.818***	9.980***	9.641***	9.740***	9.739***	9.711***
	(0.040)	(0.069)	(0.056)	(0.081)	(0.075)	(0.075)	(0.076)
Observations	1,457	1,457	1,457	1,457	1,457	1,457	1,457
R-squared	0.027	0.831	0.860	0.906	0.907	0.907	0.911

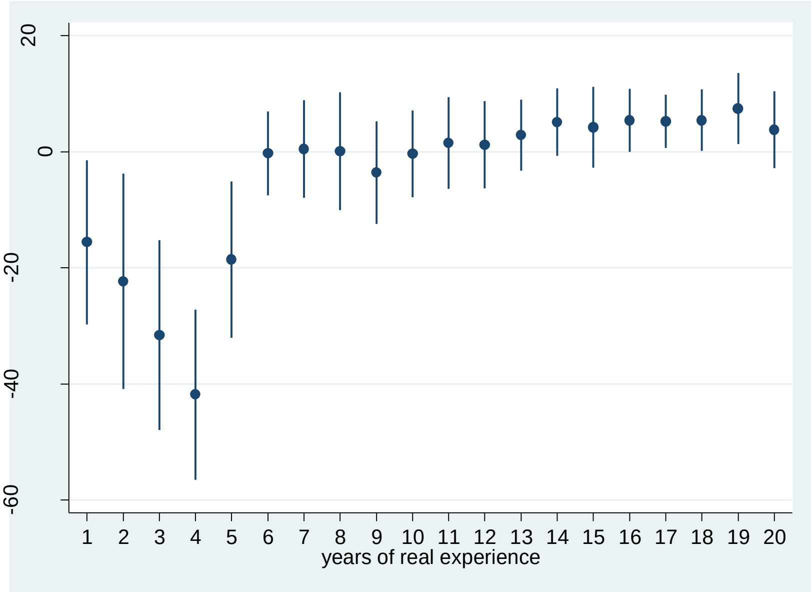
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9. Number of non-permanent contracts per year; collapsed dataset.

	No FE	Real Exp. FE	Time & Real Exp. FE	+ Excluding 68&69 cohorts	+ Excluding 1968 cohort	interactions with Experience
effect	0.011	0.008	0.041***	0.037**	0.055***	0.017
	(0.006)	(0.007)	(0.010)	(0.014)	(0.014)	(0.018)
trend	-0.002	0.001	-0.046***	-0.052***	-0.052***	-0.052***
	(0.002)	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)
posttrend	0.011***	0.009**	0.037***	0.054***	0.042***	0.043***
	(0.003)	(0.004)	(0.006)	(0.007)	(0.007)	(0.007)
trend2	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
posttrend2	0.002***	0.002***	-0.000	-0.001**	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
reformrex1						0.069***
						(0.018)
reformrex2						0.027
						(0.019)
reformrex3						0.004
						(0.024)
reformrex4						0.019
						(0.023)
reformrex5						0.045*
						(0.022)
reformrex6						0.028
						(0.022)
Observations	2,080	2,080	2,080	2,080	2,080	2,080
R-squared	0.201	0.226	0.913	0.914	0.913	0.914

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

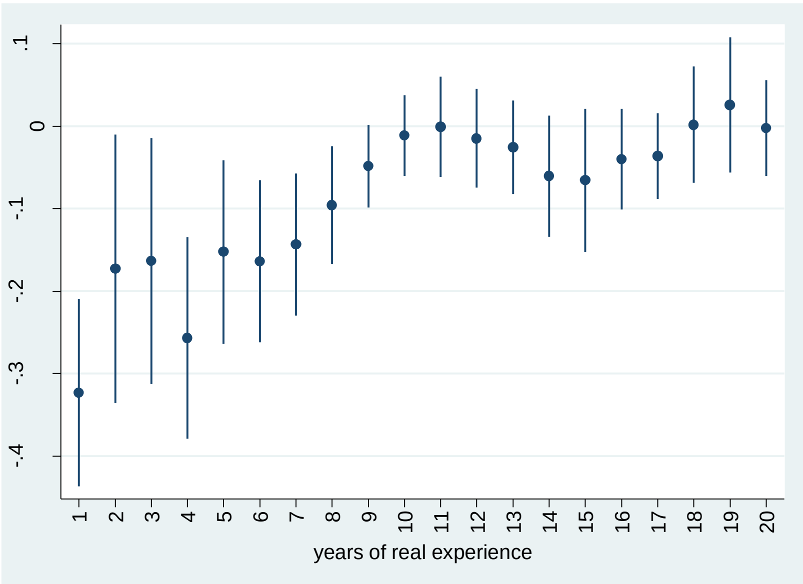
Figure 13. Annual number of days worked, aggregate data. Beta coeficients for years of real experience dummies.



Note: The coefficients are from a regression of the annual number of days worked for the cohorts 1960-1975 (excluding the cohort of 1968) and controlling for cohort dummies.

Source: Muestra Continua de Vidas Laborales (MCVL).

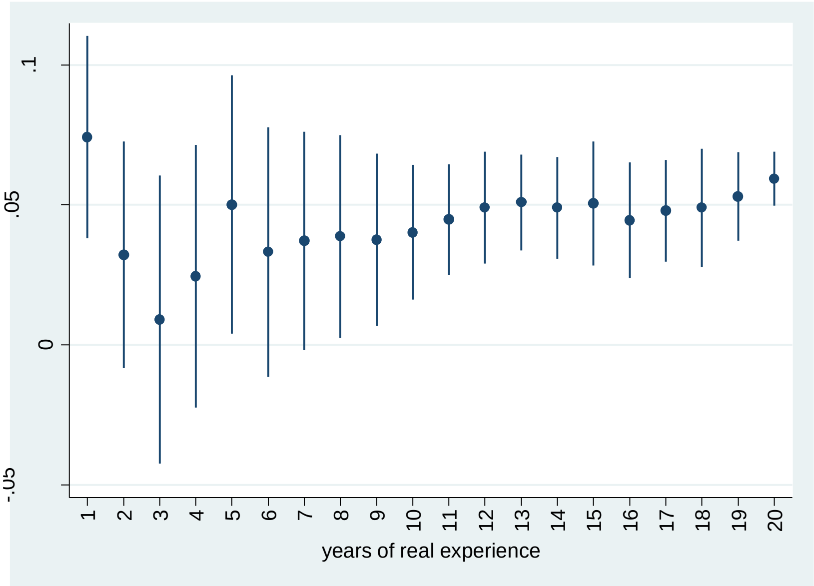
Figure 14. Logarithm of annual earnings, aggregate data. Beta coeficients for years of real experience dummies.



Note: The coefficients are from a regression of the logarithm of annual wages for the cohorts 1960-1975 (excluding the cohort of 1968) and controlling for cohort dummies.

Source: Muestra Continua de Vidas Laborales (MCVL).

Figure 15. Annual number of non-permanent employment spells. Beta coefficients for years of real experience dummies. Regression of the number of non-permanent employment spells.



Note: The coefficients are from a regression of the annual number of non-permanent employment spells for the cohorts 1960-1975 (excluding the cohort of 1968) and controlling for cohort dummies.

Source: Muestra Continua de Vidas Laborales (MCVL).

Table 10. Number of days worked per trimester; collapsed dataset by trimester.

	No FE	Time FE	Exper. FE	Exper. & Time FE	+ Exclud. 68 cohort	+ Exclud 68-3 & 68-4	interactions with Exper.
effect	-5.791*	-5.650	-5.847*	-5.817*	-12.773***	-10.396***	-7.247
	(2.792)	(2.882)	(2.707)	(2.757)	(2.102)	(2.271)	(3.838)
reformrex1							-1.320
							(4.122)
reformrex2							-4.384
							(7.315)
reformrex3							-20.217**
							(7.644)
reformrex4							-33.274***
							(5.269)
reformrex5							-16.174**
							(4.656)
reformrex6							-11.438
							(7.237)
reformrex7							-6.589
							(6.246)
reformrex8							5.535
							(5.960)
reformrex9							7.393
							(4.959)
Observations	2,460	2,460	2,460	2,460	2,460	2,460	2,460
R-squared	0.014	0.705	0.737	0.780	0.782	0.782	0.790

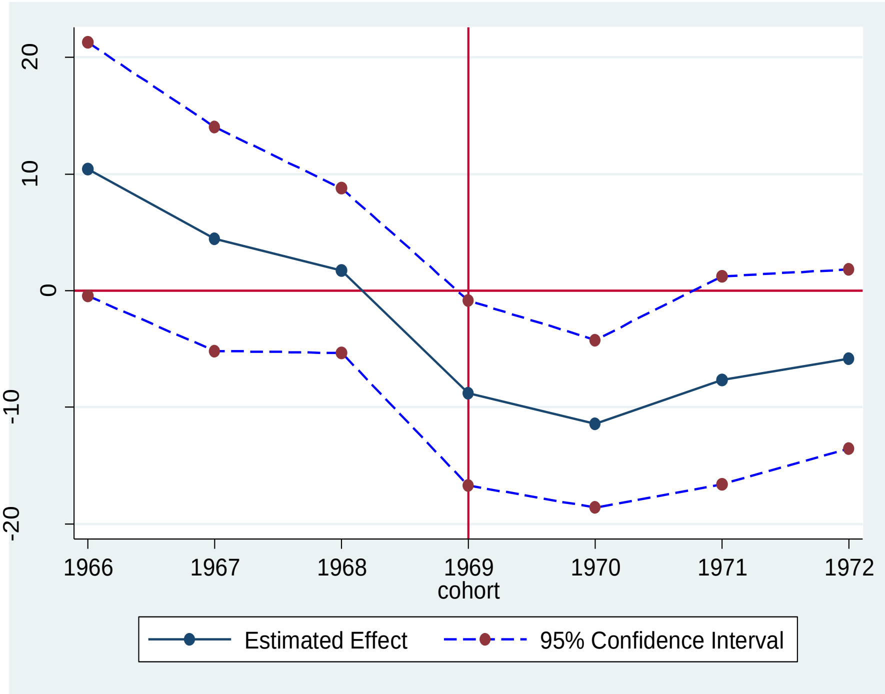
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11. Logarithm of earnings by trimester; collapsed dataset by trimester.

	No FE	Time FE	Exper. FE	Exper. & Time FE	+ Exclud. 68 cohort	+ Exclud. 68-3 & 68-4	Interact. Exper.
effect	-0.068**	-0.061**	-0.065**	-0.062**	-0.141***	-0.108***	-0.011
	(0.025)	(0.023)	(0.025)	(0.024)	(0.021)	(0.023)	(0.021)
reformrex1							-0.286***
							(0.066)
reformrex2							-0.214***
							(0.041)
reformrex3							-0.232***
							(0.052)
reformrex4							-0.241***
							(0.049)
reformrex5							-0.122***
							(0.029)
reformrex6							-0.103***
							(0.025)
reformrex7							-0.058
							(0.029)
reformrex8							-0.003
							(0.037)
reformrex9							0.023
							(0.040)
Observations	2,460	2,460	2,460	2,460	2,460	2,460	2,460
R-squared	0.007	0.844	0.815	0.873	0.875	0.874	0.883

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

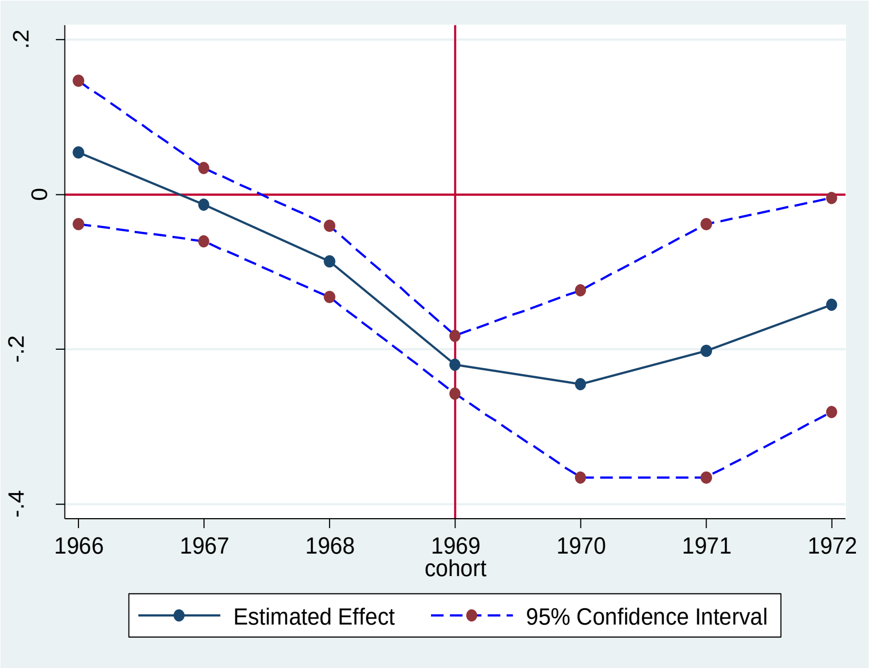
Figure 16. Placebo tests for the number of days worked. Baseline sample. Regressions with aggregate data simulating the policy effect for cohorts born from 1966 to 1972. True effect for cohort born in 1969.



Note: The coefficients are from a regression of the number of days worked for the cohorts 1960-1975 and controlling for real experience.

Source: Muestra Continua de Vidas Laborales (MCVL).

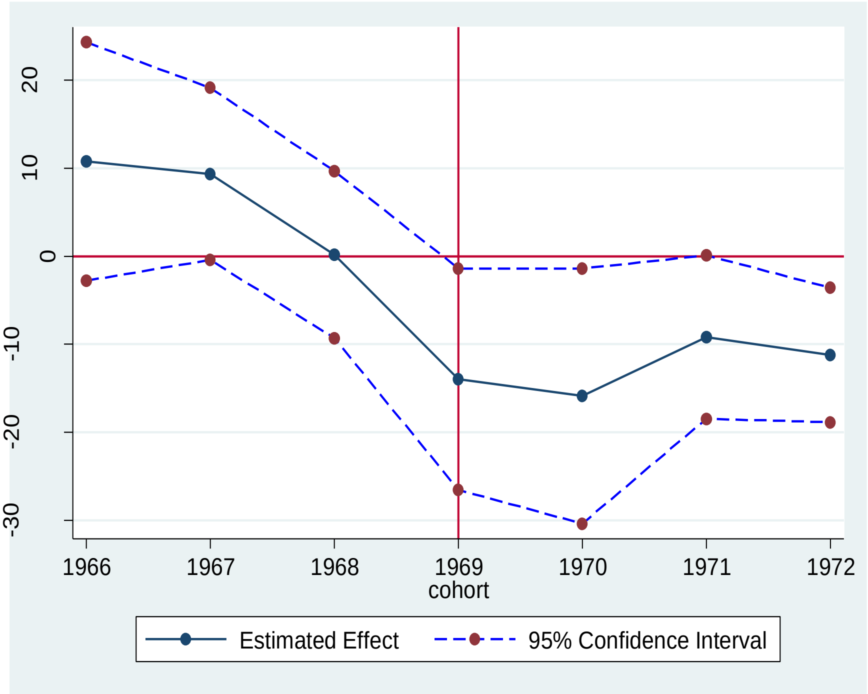
Figure 17. Placebo tests for the logarithm of yearly earnings. Baseline sample. Regressions with aggregate data simulating the policy effect for cohorts born from 1966 to 1972. True effect for cohort born in 1969.



Note: The coefficients are from a regression of the logarithm of yearly wages for the cohorts 1960-1975 and controlling for real experience.

Source: Muestra Continua de Vidas Laborales (MCVL).

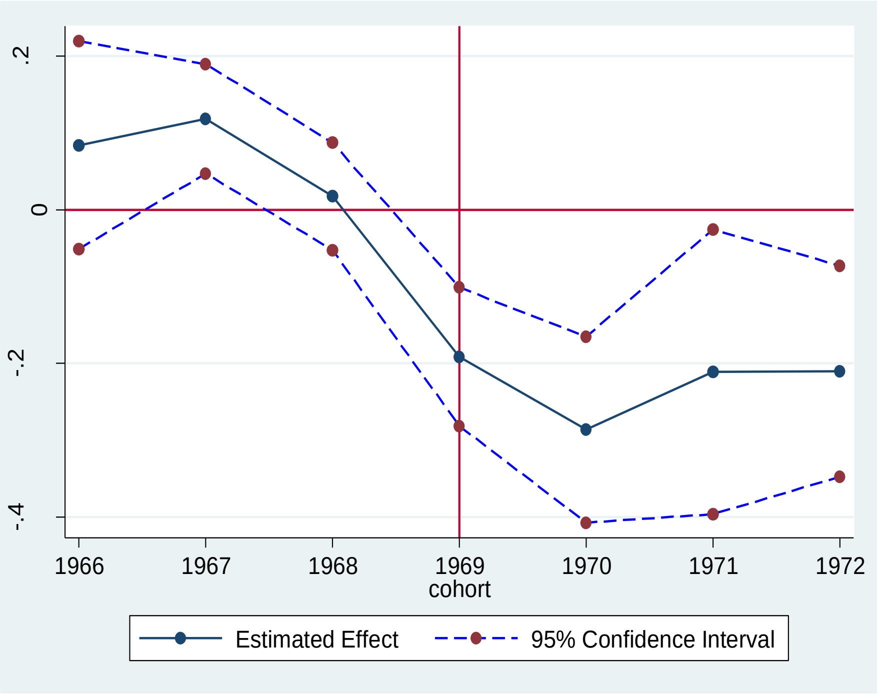
Figure 18. Placebo tests for the number of days worked. Sample restricted to include in the control group only those who begin working before the reform. Regressions with aggregate data simulating the policy effect for cohorts born from 1966 to 1972. True effect for cohort born in 1969.



Note: The coefficients are from a regression of the number of days worked for the cohorts 1960-1975 and controlling for real experience.

Source: Muestra Continua de Vidas Laborales (MCVL).

Figure 19. Placebo tests for the logarithm of yearly earnings. Sample restricted to include in the control group only those who begin working before the reform. Regressions with aggregate data simulating the policy effect for cohorts born from 1966 to 1972. True effect for cohort born in 1969.



Note: The coefficients are from a regression of the logarithm of yearly wages for the cohorts 1960-1975 and controlling for real experience.

Source: Muestra Continua de Vidas Laborales (MCVL).

APPENDIX

Table 12. Aggregation of sectors in the economy.

Sector of the economy	Description of sector
High Tech. Manufacturing	Manufacturing using capital intensive and ICT technologies (Chemical industry, electrical industry, etc.)
Low Tech. Manufacturing	Manufacturing with low capital intensity (Furniture, Plastics, etc.)
High Tech. Services	Transport, Telecommunication, Finances, Education, Health, etc.
Trade	Wholesale trade, retail.
	Hotels, restaurants, etc.
Other Low Tech. Activities	Labor intensive Services (water and waste treatment, personal services), Agriculture, Energy Industry
Construction	Building activities
Public Administration & Real State	All public activities but health and education. Real State activities
Missing	No information about sector of activity

Source: Muestra Continua de Vidas Laborales (MCVL).