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Abstract

The latest labor market reform in Spain, approved in December 2021, introduced unprecedented restrictions on the use of temporary contracts. This paper uses administrative records from the Continuous Sample of Working Lives (MCVL) to examine the reform's impact on the quality of contracts and the labor market outcomes for young labor market entrants. The empirical strategy relies on a difference-in-differences design that exploits persistent differences in the prevalence of temporary employment at the provincial level. To this end, we construct a novel indicator that takes into account the sectoral composition of employment in each province. The results show that the reform substantially increased the probability of entrants accessing the labor market through an open-ended contract. This almost eliminated the pre-existing differences in this outcome between entrants in provinces with a high or low prevalence of temporary employment prior to the reform. In contrast, the reductions in the corresponding gaps in employment duration and earnings are much more modest. We attribute these differences to the decline in the duration of regular open-ended jobs following the reform, which was more pronounced in the provinces with a high prevalence of temporary employment prior to the reform.

JEL codes: J08, J21, J41, J63.
Keywords: labor market reform, dual labor markets, temporary contracts, youth employment

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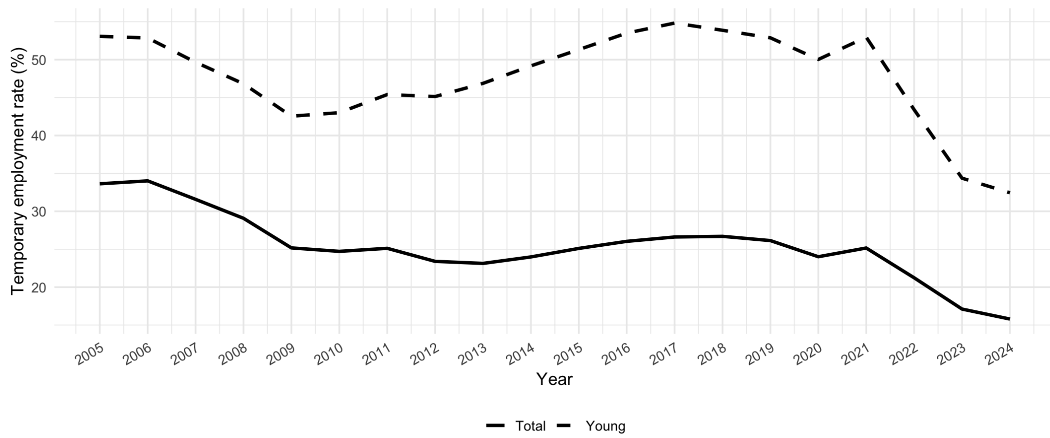
1 Introduction

Dual labor markets and their adverse effects on employment stability—particularly among young workers—have been a recurring theme in the research of Samuel Bentolila. For decades, the Spanish labor market has been characterized by a pronounced segmentation stemming from the dual nature of its employment protection legislation (EPL). The large gap in the protection afforded to temporary and permanent contracts generated one of the highest shares of temporary employment in Europe and an unusually high level of labor turnover, often involving repeated employment spells in the same position (Felgueroso et al., 2018). After a series of largely ineffective reforms, the 2021 labor market reform is the first serious attempt to eradicate this anomaly by introducing unprecedented restrictions on the use of temporary contracts. Following its implementation, the proportion of temporary employment declined by over 10 percentage points (pp). Therefore, the reform achieved its objective of reducing the incidence of temporary employment. However, the impact of the reform on *de facto* employment stability remains an open question.

In this study, our attention is on on young people entering the labor market. Using administrative records from the Continuous Sample of Working Lives (MCVL), we analyze the impact of the 2021 reform on young entrants' access to regular open-ended contracts, employment duration, and earnings. Focusing on youth was a natural choice. Prior to the reform, young workers had by far the highest rate of temporary employment of all age groups (see Figure 1). Rather than functioning as a screening device or stepping stone toward permanent employment, temporary contracts were primarily used to reduce labor costs and provide firms with a buffer against demand fluctuations. Consequently, young workers were disproportionately exposed to prolonged periods of insecure employment with low wages. A large body of literature shows that the resulting delays in the transition from education to stable employment hinder the accumulation of human, with negative consequences that may persist throughout the working life (see, for example, García-Pérez et al., 2019; Cabrales et al., 2017). By shortening the transition period from education to stable employment, the reform thus has the potential to enhance future labor productivity.

The main challenge we face is the universal nature of the 2021 reform. The reform took full effect at the same time, in April 2022, for all firms and workers simultaneously. Consequently, there are no unaffected units that can serve as a control group. In such cases, a standard approach is to exploit cross-sectional heterogeneity in exposure to the reform —understood as differences in treatment intensity across units— to estimate its differential impact across groups. In this study, we exploit persistent differences in the incidence of temporary employment across provinces prior to the reform. Specifically, we construct a novel province-level indicator that captures the excess use of temporary contracts. This indicator is defined as the difference between a province's actual temporary employment share in a province and the counterfactual share that would result from applying to that province the sector-specific temporary employment shares observed, on average, in the rest of the country. Sectoral averages are computed by weighting each province's employment share in national employment. The resulting measure of the excess use of temporary jobs displays substantial variation between provinces, is stable over time, and is a strong predictor of both the

Figure 1. Adult versus youth temporary employment shares, 2005-2024



Source: Own calculations based on the Spanish Labor Force Survey (EPA).
Notes: The temporary employment share is defined as the percentage of workers with a temporary contract over the total number of salaried employees in the public and private sector. The young population corresponds to individuals aged 30 or younger.

proportion of entrants who obtained a permanent contract prior to the reform and of the subsequent improvement in this outcome.

Our empirical strategy employs a difference-in-differences design. In the baseline specification, labor market entrants are divided into two groups according to their province of entry. Those entering the labor market in provinces with a positive value of our indicator — signaling the excessive use of temporary contracts prior to the reform — form the treatment group, while those entering the labor market in provinces with a negative value form the control group. In a first step, we document the reform's effect on contract type. Specifically, we estimate the reform's impact on the probability that an entrant obtains a permanent versus a temporary contract at the beginning of their first employment spell. After establishing improvements in the access to permanent contracts, we then proceed to estimate the effects of these improvements on subsequent labor market outcomes. Our analysis focuses on the number of days worked, the probability of experiencing an employment spell lasting more than six months (the maximum legal duration of probationary periods) and labor earnings, proxied by Social Security contribution bases. We analyze these outcomes both during the first employment spell and during the twelve months following labor market entry.

Our empirical approach differs in important respects from the standard methodology used to evaluate EPL reforms. The conventional approach exploits observed differences in turnover rates across sectors or occupations as a source of variation in treatment intensity. It rests on the assumption that EPL reforms are expected to have larger effects in sectors and occupations with "intrinsically" higher turnover rates. In contrast, our strategy relies on residual variation in the incidence of temporary employment across provinces after netting out differences in sectoral employment composition. We interpret a positive residual as an indicator of firms' market power. Intuitively, lower competitive pressure reduces firms' incentives to offer permanent contracts, given their higher costs and lower flexibility compared to temporary contracts. This interpretation is supported by recent evidence showing that greater monopsonistic power is associated with reduced access to permanent contracts (Bassanini et al., 2025).

The results show that the reform generated a genuine overhaul in hiring patterns and rather modest improvements in the labor market outcomes for new entrants. After the reform, the share of young workers starting their careers with a permanent contract increased by approximately 40 pp after the reform. Our estimates indicate that the reform's impact on access to permanent contracts was 6.4 pp larger for individuals in the treatment group, thereby eliminating 87% of the gap existing before the reform. For regular open-ended contracts ("contratos indefinidos ordinarios"), the differential effect shrinks to to 4 p.p., equivalent to 55.5% of the pre-reform gap. We also find positive and statistically significant effects on each of the labor market outcomes; however, in relative terms these effects are substantially smaller than those on contract type. Specifically, we find that the reform reduced the gap in the average duration of entrants' first spell between the control and treatment groups by 40 percent. Similarly, the reform reduced the difference in the probability of a duration above six months by 32 percent. Finally, the differential effect on cumulative earnings during entrants' first spell corresponds to 14 percent of the corresponding pre-reform gap. In all three cases, the effects are small in absolute terms. We find no significant differences by educational attainment, though there is some indication that the effects were slightly smaller for entrants with medium-low levels of education. Furthermore, the impact of the reform appears to diminish over the first twelve months.

The smaller improvements in labor market outcomes are explained by the drop in the average duration of the contracts signed after the reform. For regular open-ended contracts, the average duration decreased by about 100 days, with an estimated difference of around 30 days between the treatment and control groups. The shorter duration of the open-ended contracts is a logical consequence of the reform, but it may also reflect a change in firms' and workers' separation decisions. Under the new rules, permanent contracts are extended to jobs that are more intrinsically unstable than those previously covered by permanent contracts. One would also expect a decline in the average quality of job matches because firms can no longer be as selective when granting permanent contracts as they were before to the reform. It is even possible that some workers who are offered a permanent contract would have preferred a temporary contract in order to qualify for unemployment benefits at the end of the contract, a right unavailable following a voluntary separation under a permanent contract.

Although we cannot estimate the causal impact of the reform on the expected duration or cause of separation for separate contract types, we do not observe an increase in the share of permanent jobs ending in dismissal, either during the probationary period or afterward. Indeed, among entrants, voluntary quits are the most frequent cause of separation, accounting for roughly 60 percent of separations both before and after the reform. What we do observe is a decline, for each cause of separation, in the number of days elapsed between the start of regular open-ended contracts and separation. The overall result is a decline in the survival rate of standard open-ended contracts. Specifically, our estimates, based on a discrete-time duration model, show reductions in survival rates at three, six, nine, and twelve months. In the latter case, the survival rate declines from approximately 75 percent before the reform to below 60 percent in the post-reform period.

Our study provides the first comprehensive evaluation of the 2021 labor market reform. The results confirm preliminary findings of earlier studies. Conde-Ruiz et al. (2025) analyze administrative data with daily information on hires and separations for the universe of Social Security contributors. After reconstructing daily flows between employment and non-employment by contract type, the study finds no evidence of significant changes in labor turnover following the reform. While the evidence presented is extensive, the analysis is descriptive and does not permit a causal interpretation. In a related vein, Calvo et al. (2024) document a sustained increase in voluntary separations since 2013 and highlight a rise in the propensity to quit among workers with permanent contracts following the 2021 reform. In contrast to this aggregate evidence, our descriptive results do not point to a particularly pronounced increase in quits among new labor market entrants, but rather to shift forward in time in voluntary separations.

Next, there is some evidence that previous EPL reforms benefitted youth in Spain. For example, Elias and Redondo (2025) study the 1997 Spanish reform, which lowered non-wage labor costs for new open-ended hires of workers below age 30 and above age 45, while leaving the costs of temporary contracts unchanged. Exploiting a combination of sector-level variation in pre-reform exposure to temporary employment and age-based discontinuities at eligibility thresholds, they show that the reform induced a substantial substitution from temporary to open-ended contracts among young workers, while no significant substitution is observed among older workers. They further document wage increases even in non-subsidized jobs, which they interpret as evidence of improved outside options and enhanced bargaining power for young workers following the expansion of access to permanent employment. Finally, they find an increase in temporary employment among workers older than 30, consistent with a reallocation of tasks within firms.

Closest to our work is the preliminary study by the International Monetary Fund (2024) on the effects of the 2021 reform. Using flow data from the Spanish Labour Force Survey, the authors compare labor market outcomes across region–sector–occupation cells before and after the reform. Consistent with the prior literature on EPL reforms, their identification strategy relies on the premise that the reform should have had larger effects in cells with higher "natural" turnover rates—occupations in which technological or demand-related factors, such as seasonality, generate intrinsically high job creation and destruction rates. In line with our findings, the study concludes that the reform substantially reduced the incidence of temporary employment, with uncertain effects on job stability. A key limitation of the IMF analysis is limited statistical power: the estimates are based on fewer than 400 observations, whereas we use individual-level data for more than 177,000 labor market entrants. Moreover, their identification strategy relies on structural sectoral differences in turnover, which are difficult to modify. By contrast, our measure of excess temporary employment explicitly nets out these sectoral differences, allowing us to assess whether the reform reduced the discretionary use of temporary contracts by firms operating in less competitive local labor markets.[1] The remainder of the paper is organized as follows. Section 2 provides the institutional background of the Spanish labor market and the 2021 labor reform. Section 3 describes the data used in the analysis and the construction of the main variables used in the analysis. Section 4 presents the methodology, while Section 5 discusses the key identification challenges. Section 6 summarizes the main results, followed by an in-depth analysis of the drop in the duration of regular open-ended jobs in Section 7. Finally, Section 8 concludes.

2 Institutional background

The origins of Spain's dual labor market are usually traced back to the labor market reform of 1984, which introduced major liberalisation in the area of temporary hiring by creating the Employment Promotion Temporary Contract. This allowed firms to hire workers on a temporary basis without the need of a justified cause. While the reform contributed to a rapid expansion of employment, it also led to a sharp rise in temporary contracts (García-Pérez et al., 2019). Young workers were particularly affected by this institutional configuration, as temporary contracts became the predominant entry point into employment. However, they were certainly not the only affected group. By the early 90s more than 30% of the workforce was employed on a temporary contract, making Spain the country with the largest share of non-standard employment in Europe.

Subsequent reforms aimed to address this issue by narrowing the difference in EPL between open-ended and temporary contracts, as well as offering incentives for hiring for open-ended positions. Ten years after its introduction, the 1994 reform abolished the employment-promotion temporary contract, formally reinstating the principle of causality in temporary hiring. However, enforcement was weak and the share of temporary jobs remained at the highest level in Europe.

The 1997 reform, based on a social agreement between the government and the social partners, tried a different strategy. It sought to promote stable employment by introducing a new open-ended contract — employment-promotion permanent contract — with reduced dismissal costs relative to the ordinary permanent contract. The reform also included hiring subsidies targeted at specific groups with weaker labor market attachment, including young workers and the long-term unemployed. Despite these measures, labor market duality persisted and temporary employment continued to play a central role in firms' adjustment strategies, particularly for younger cohorts entering the labor market.

The next major reform took place in 2012, in the middle of the Great Recession. It introduced substantial changes to employment protection legislation with the aim of increasing firms' internal flexibility through adjustments in wages and hours rather than through layoffs. Among its main provisions, the reform reduced compensation for unfair dismissal from 45 to 33 days of wages per

[1] Also using MCVL data, García-Perez (2025) documents a significant increase in access to a first permanent contract after the reform, improvements in the stability of early career trajectories—reflected in longer employment durations—and an increase in average contribution bases during the first year, primarily associated with a higher number of days worked.

year of service, lowered the cap on total compensation, and expanded the scope for working-time reductions or wage adjustments. The reform also introduced specific hiring incentives for small firms, including a permanent contract with a one-year trial period accompanied by economic incentives for hiring young unemployed workers. In addition, apprenticeship and training contracts—traditionally used as an entry route for young workers—were expanded and made more flexible. Despite these measures, the reform did not substantially reduce labor market turnover or the extensive reliance on temporary contracts (García-Pérez and Domènech, 2019). While the proportion of temporary contracts fell during the crisis due to the disproportionate loss of temporary jobs, firms increasingly began to use short-term temporary contracts, often re-employing the same workers, giving rise to record numbers of contracts (Felgueroso et al., 2018). Around 40% of these newly-signed contracts lasted less than a month.

Against this background, the 2021 labor reform (Royal Decree-Law 32/2021) introduced a different policy approach. Rather than focusing on dismissal costs or incentives for permanent hiring, the reform targeted the regulatory framework governing temporary employment. Its central objective was to modify the institutional incentives that had historically favored the intensive use of temporary contracts and to promote more stable employment relationships.

One of the most significant changes was the elimination of the temporary contract linked to a specific project or service (*contrato de obra y servicio*), which had long been one of the main channels through which firms relied on temporary employment. Prior to the reform, this contract allowed firms to link employment to the completion of a specific project with autonomy within the firm and could last up to three years. In practice, this design often facilitated long-lasting temporary employment relationships.

Following the reform, temporary employment was essentially limited to two contract types: a contract for production-related circumstances and a replacement contract. The production-related contract is intended to cover temporary increases in firms' activity and distinguishes between two situations. First, temporary hiring may respond to unforeseeable production circumstances, in which case contracts can last up to six months within an eighteen-month period, extendable to twelve months through collective agreements. Second, firms may use temporary contracts to address foreseeable and short-lived increases in activity—such as seasonal campaigns or temporary peaks in demand—through a specific modality limited to 90 days per calendar year, which cannot be used continuously. These provisions were designed to reinforce the principle of causality and limit the use of temporary contracts to genuinely temporary production needs. Moreover, the reform also tightened the rules governing the chaining of temporary contracts[2] and introduced a surcharge on social security contributions for short-duration contracts.[3]

[2]Under the previous legislation, a worker acquired permanent status after accumulating 24 months of employment within a 30-month period with the same firm. The reform reduced this threshold substantially: since 2022, a worker becomes permanent after accumulating more than 18 months of employment within a 24-month period. This change restricts firms' ability to rely on successive temporary contracts over extended periods.

[3]In addition, the reform introduced financial disincentives for the use of very short-term contracts, which had become widespread in some sectors. Temporary contracts lasting less than 30 days are now subject to an additional social security contribution equivalent to roughly three times the daily contribution for common contingencies. This

Finally, the 2021 includes no measures to make ordinary open-ended contracts more attractive to firms. Instead, it widens the scope for the use of intermittent open-ended contract (*contrato fijo-discontinuo*). Firms can temporarily suspend these contracts at no cost in periods of low activity. Prior to the reform, this contract type was mainly used for seasonal activities. The reform broadened its scope considerably, allowing its use for intermittent activities and by subcontractors and temporary work agencies. The intention was to replace part of the recurrent use of temporary contracts with permanent but discontinuous employment relationships.[4]

Overall, the 2021 labor reform represents the most ambitious attempt to restrict the use of temporary jobs.

3 Data

Our empirical analysis uses records from the Continuous Sample of Working Lives (MCVL). The MCVL is an administrative dataset compiled by the Spanish Social Security Administration. It includes the employment records and social security contribution bases for an unstratified random sample of 4 percent of all affiliates. In particular, for each employment spell of an individual, we observe the exact start and end dates, the contract type, the monthly social security base wage and detailed job characteristics, including the part-time coefficient, the firm's location and sector of activity and the professional category (*grupo de cotización*).

We restrict our sample to individuals who first registered with the Social Security system between 2014 and 2023, and who were under 30 years old at the time of registration. We keep track of their labor market outcomes in the twelve months following their first registration. During this first year, some workers may experience multiple employment episodes (*spells*) due to turnover, interruptions, or contractual changes. In order to obtain a clean and coherent definition of labour market entry, the job characteristics of each entrant are assigned based on their first recorded contract. This includes the contract type and sector of activity. Our initial panel includes all individuals appearing in any of the annual MCVL waves between 2014 and 2023, including those who subsequently exit the sample due to labor market withdrawal, transitions to uncovered regimes, or emigration.

Throughout the analysis we restrict attention to salaried workers in the private sector. That is, we exclude self-employed workers as well as public-sector employees, as their hiring procedures are governed by specific institutional mechanisms that are not directly comparable to those in the private sector. We also remove individuals whose first contract corresponds to a training contract or a temporary contract for replacement (*contrato de interinidad*), as these contract types are subject to distinct regulatory frameworks that could distort the interpretation of the results.

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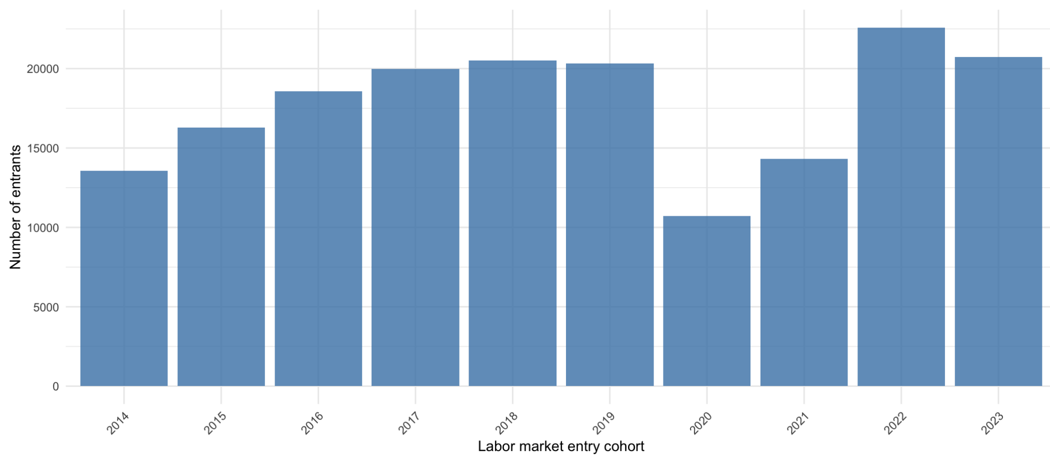
surcharge applies irrespective of the exact duration of the contract and aims to discourage the repeated use of extremely short contracts.

[4]Finally, the reform modified the system of training contracts, which are particularly relevant for young labor market entrants. The previous modalities were replaced by two new types: the work-study training contract, with a maximum duration of two years and a stronger link to formal education, and the contract for obtaining professional practice, which can be concluded within three years after completing studies and has a maximum duration of twelve months.

The resulting dataset has the structure of a repeated cross-section in which each entry cohort is observed once. The final sample consists of 177,541 individuals. The annual cohort sizes are growing over time, as shown in Figure 2, with a transitory decline in 2020 coinciding with the COVID-19 pandemic, followed by a rebound in the years thereafter.

Figure 2. Annual entry cohort sizes



As we compare the outcomes of entry cohorts before and after the reform, we need to ensure that these cohorts be comparable. Table 1 compares the characteristics of the labor market entrants in our sample when we pool the pre- and post-reform cohorts. A key variable in the analysis is educational attainment, which we group into four categories[5]. The reported Cohen statistics in column 4 show that the differences for most characteristics are small when we account for sample size.[6] The only notable exception concerns the educational attainments of entrants, where we observe a decline in the share of entrants with higher education and a corresponding increase in the share of entrants with intermediate education levels. There are some factors that attenuate the potential impact of this shift. First, the evidence reported in Table A.1 shows similar shifts in the provinces in the treatment and control groups, suggesting the presence of a common shock rather than a differential change in educational attainments that could be associated with the reform. Second, in the analysis we explicitly account for potential heterogeneity by level of education in the effects of the reform. Finally, given the potential measurement error in the educational attainment of entrants in the MCVL, we also report the results when we proxy the level of education by the contribution group the entrant is assigned to in the first spell.

[5]Educational attainment is obtained from the Continuous Municipal Register (INE), which records the highest level of education declared by the individual. Specifically, low education corresponds to codes 10–22 (individuals without formal education or with incomplete primary education), lower–mid education to codes 30–32 (lower secondary education or intermediate vocational training), upper–mid education to codes 40–43 (upper secondary education or advanced vocational training), and higher education to codes 44–98 (university and postgraduate degrees).

[6]For continuous variables, we report Cohen's d, defined as the difference in means normalized by the pooled standard deviation, while for categorical or binary variables we use Cohen's h. Unlike conventional hypothesis tests, which depend critically on sample size, Cohen's effect sizes provide a scale-free measure of the economic magnitude of differences, facilitating comparisons across variables.

Table 1. Descriptive statistics: pre- vs post-reform entry cohorts

Variable	Category	Pre	Post	Post–Pre	p-value	Cohen
Region	Comunidad Valenciana	10.452	10.821	0.368	0.031	0.012
	Cataluña	19.550	19.552	0.002	0.998	0.000
	Canarias	4.313	4.277	-0.036	0.759	-0.002
	Madrid	18.773	19.702	0.929	0.000	0.024
	Murcia	3.779	3.448	-0.332	0.002	-0.018
	Andalucía	16.265	15.989	-0.276	0.177	-0.008
	Extremadura	1.531	1.349	-0.182	0.007	-0.015
	Galicia	4.088	4.020	-0.067	0.547	-0.003
	Castilla-La Mancha	3.575	3.198	-0.377	0.000	-0.021
	Islas Baleares	3.083	3.374	0.290	0.003	0.016
	Castilla y León	3.686	3.702	0.016	0.893	0.001
	País Vasco	4.035	4.002	-0.034	0.768	-0.002
	Navarra	1.345	1.217	-0.128	0.045	-0.011
	Aragón	2.623	2.469	-0.154	0.081	-0.010
	Cantabria	1.005	1.039	0.034	0.555	0.003
	Asturias	1.246	1.307	0.061	0.331	0.005
	La Rioja	0.651	0.536	-0.115	0.009	-0.015
Sex	Male	51.859	51.084	-0.774	0.005	-0.015
	Female	48.141	48.916	0.774	0.005	0.015
Immigrant	Yes	19.598	24.383	4.785	0.000	0.116
Education	Low	10.026	9.241	-0.784	0.000	-0.027
	Lower-middle	31.237	39.605	8.368	0.000	0.175
	Upper-middle	28.937	36.534	7.597	0.000	0.162
	High	29.801	14.620	-15.181	0.000	-0.370
Labor outcomes	Temporary contract (%)	85.826	46.184	-39.642	0.000	-0.875
	Intermittent open-ended (%)	1.380	20.762	19.382	0.000	0.711
	Open-ended (%)	12.794	33.054	20.260	0.000	0.493
	Days worked, first spell	100.225	94.313	-5.913	0.000	-0.051
	Days worked, first year	176.065	185.584	9.519	0.000	0.072
	First spell $\geq$ 6 months	0.330	0.332	0.003	0.326	0.005
	Any spell $\geq$ 6 months (year)	0.216	0.193	-0.023	0.000	-0.057
	Contribution base, first spell	3660.380	3753.344	92.964	0.001	0.018
	Total contribution base, first year	5562.679	6155.468	592.790	0.000	0.098
N		134,236	43,305			

Note: Means and shares are calculated as annual averages for the pre-reform period (2014–2021) and the post-reform period (2022–2023).

Besides the shift in educational attainments, we also observe an increase in the relative share of immigrant workers after the reform. This shift coincides with the gradual normalization of migration flows following the COVID-19 pandemic and is not perfectly balanced across provinces. However, we will show that our results do not change when we restrict the sample to natives.[7]

Finally, the lower panel documents the sharp shift in hiring patterns among entrants. Prior to the reform, temporary contracts accounted for 86% of all initial contracts. In the post-reform period, this figure dropped to 46.2%. This drop is mirrored by an increase of approximately 20 pp in the shares of regular and discontinuous open-ended contracts.

4 Methodology

As mentioned earlier, the main challenge in identifying the causal effects of the 2021 reform lies in its universal nature. To overcome this, we exploit the persistent pre-reform differences across provinces in the prevalence of temporary employment. The underlying premise is that, although all provinces are subject to the same regulatory framework, the reform is more stringent for firms in provinces with structurally high levels of temporary employment.

To quantify the geographical variation in the prevalence of temporary jobs, we build a counterfactual measure based on within-sector differences in the prevalence of temporary employment. In a first step, we compute, for each province and sector of activity, the average temporary employment rate in the same sector in the rest of the provinces in the period 2014-2019.[8] Using these sectoral rates, we then derive, for each province, a counterfactual temporary employment rate defined as the share of temporary employment that would result if the share of temporary employment in each of its sectors were equal to the corresponding average share in all other provinces.[9] This counterfactual rate represents the level of temporary employment that would be expected in a province if its reliance on temporary contracts were determined solely by its sectoral composition. If this counterfactual share is lower than the actual share of temporary employment, we conclude there was an excess of temporary employment in this province before the reform.

Formally, let p denote a province and s a sector of economic activity. Next, let τ_s^{-p} denote the national average temporary employment rate in sector s, computed leaving out province p itself, and let $\omega_{p,s}$ denote the share of sector s in total employment in province p. Our counterfactual temporary employment rate for province p is then defined as follows:

$$\widehat{\tau}_p = \sum_s \omega_{p,s}\, \tau_s^{-p} \tag{1}$$

[7]The results for native workers are reported in Table A.2 in the Appendix.

[8]The years 2020 and 2021 are excluded from the pre-reform period due to the exceptional disruptions associated with the COVID-19 pandemic, which may have altered the sectoral composition of employment and thus distorted the measurement of structural provincial differences in temporary employment.

[9]For each province, national sectoral rates are calculated excluding the province's own contribution to the national aggregate (*leave-one-out*), in order to avoid mechanically incorporating province-specific behavior into the counterfactual measure.

while the excess prevalence of temporary employment in that province is defined as the difference between the observed temporary employment rate in province p, τ_p, and its counterfactual value:

$$\text{Excess}_p = \tau_p - \widehat{\tau}_p. \tag{2}$$

Figure 3. Excess prevalence of temporary employment by province

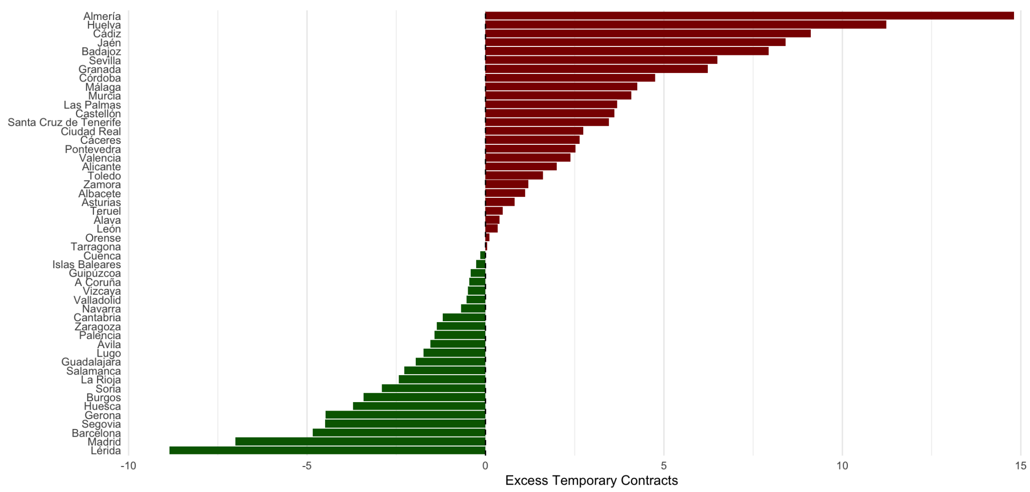
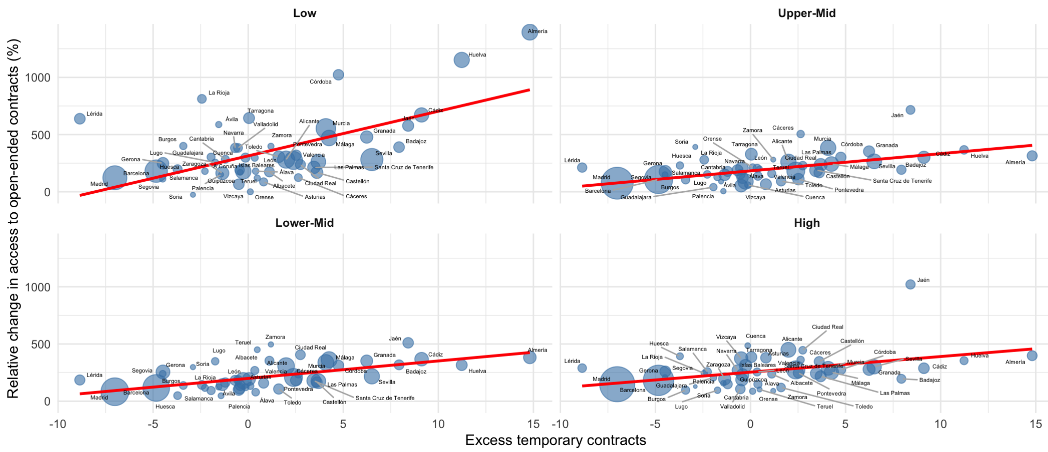


Figure 3 shows the resulting ranking of provinces. The red bars correspond to provinces with an excess prevalence of temporary employment in the pre-reform period. Young entrants in these provinces constitute our baseline treatment group. By contrast, entrants in provinces with a negative value of Excess_p constitute our baseline control group. Figure A.1 in the Appendix shows that our measure of excess prevalence of temporary employment is remarkably stable throughout the 2014–2019 period. Taken together, both figures suggest that Excess_p captures structural pre-reform differences across provinces.

Figure 4. Relative change in access to regular open-ended contracts



Notes: The figure shows for each province the relationship between pre-reform excess temporary employment and the relative change in access to regular open-ended contracts after the labor reform, disaggregated by education level. The relative change is calculated as the percentage variation between the post-reform period (2022–2023) and the pre-reform period (2014–2021), normalized by the pre-reform level. Each point represents a province, with its size proportional to the province's relative weight in the pre-reform period, measured by the number of labor market entrants. Solid lines represent weighted linear fits based on this provincial weight, giving greater relevance to provinces with larger sample sizes.

Additionally, the evidence reported in Figure 4 reveals a positive correlation between the our measure of excess pre-reform temporary employment and the percentage changes in the access to regular open-ended contracts (measured as the share of entrants whose first contract is of this type) following the reform. The positive correlation is especially strong for entrants with low educational attainments.[10] The objective of our empirical analysis is to estimate the extent to which these differences can be attributed to the labor market reform and to assess how the resulting improvement in the access to permanent contracts affects the subsequent labor market outcomes of entrants.

4.1 Empirical strategy

We use a standard difference-in-differences (DiD) design to estimate the differential impact of the reform depending on the excess prevalence of temporary employment entrants face in their province. Our baseline specification uses a discrete treatment dummy which takes the value of one for all entrants in provinces with a positive value of Excess_p.[11] The estimated equation is as follows:

$$Y_{it} = \beta_0 + \beta_1 \left(post_t \times treated_i \right) + \sum_k \beta_k \, X_{it} + \alpha_p + \lambda_t + \varepsilon_{it}, \tag{3}$$

[10] When absolute changes in the rate of access to permanent employment are considered, this relationship becomes much less pronounced and the slopes flatten, as shown in Figure A.2.

[11] Table A.2 in the Appendix reports the alternative model specifications employed in the analysis, combining different definitions of treatment (binary and continuous) with various sets of controls, trends, and fixed effects. The main empirical specification corresponds to column 4 of that table.

For binary outcomes, we estimate a nonlinear specification using a logit model. In this case, the probability that individual i experiences outcome $Y_{it} = 1$ is given by:

$$\Pr(Y_{it} = 1) = \Lambda\left(\beta_0 + \beta_1(post_t \times treated_i) + \sum_k \beta_k X_{it} + \alpha_p + \lambda_t\right), \tag{4}$$

where $\Lambda(\cdot)$ denotes the logistic cumulative distribution function, $\Lambda(x) = \frac{e^x}{1+e^x}$.

Y_{it} denotes the outcome observed for individual i in year t. We consider the effect on the type of contract, the duration of employment and earnings. The type of contract is captured by a binary variable that takes the value 1 if the entrant obtains (i) a open-ended contract or (ii) a regular open-ended contract; The duration of the spell is captured by (iii) the total number of days worked and (iv) a binary indicator equal to 1 for spells lasting six months or more; Finally, earnings are measured by (v) the logarithm of total income proxied by the Social Security contribution bases.[12] Our baseline outcomes refer to entrants' first employment spell, but we also report the results referring to the first twelve months after entry[13]

The coefficient of interest is β_1. It captures the differential effect of the labor market reform on entrants in provinces with excess prevalence of temporary employment in the pre-reform period relative to entrants in provinces with a negative value for Excess_p. The vector X_{it} includes individual-level controls such as age at entry, immigrant status, educational attainment, gender, and month of entry, as well as macroeconomic controls such as annual provincial employment growth. The term α_p denotes province fixed effects, and λ_t year fixed effects, which absorb aggregate shocks common to all provinces.

The interpretation of the interaction coefficient depends on the nature of the outcome variable. For binary outcomes—both access to permanent contracts and the indicator of experiencing a spell lasting at least six months—models are estimated using logit specifications. In this case, the coefficient on $post_t \times treated_i$ represents the differential change in the log-odds of accessing permanent employment or experiencing a spell of at least six months following the reform for entrants in treated provinces relative to those in non-treated provinces, net of structural territorial differences and common time effects.

In the case of days worked, the model is estimated by ordinary least squares. The interaction coefficient is then interpreted as the differential change in the expected number of days worked during the reference period associated with the reform, comparing individuals entering the labor market in more exposed provinces with those entering in less exposed provinces. Since this variable

[12]For spells that last longer than a year we consider the earnings during the first twelve months

[13]Outcomes defined at the level of the first spell refer exclusively to the individual's initial contract: in particular, the indicators for access to a standard open-ended or to any permanent contract measure whether the first contract has that nature; the indicator for a spell lasting at least six months equals one if the duration of the first contract reaches or exceeds that threshold; and days of employment and contribution bases are computed as the total accumulated under that first contract during the 365 days following entry. Annual outcomes aggregate the individual's entire employment experience during the first year since entry, summing days worked and contribution bases across all contracts observed within that period. Similarly, the annual indicator for experiencing a spell lasting at least six months equals one if the individual experiences at least one employment spell of that minimum duration within the 365 days following labor market entry.

is measured in levels, the coefficient has a direct interpretation in units of time.

Finally, for the contribution base accumulated during the first year, the outcome is expressed in logarithms. In this case, the coefficient on $post_t \times tratado_i$ can be interpreted as a semi-elasticity: to a first approximation, a value β_1 implies a differential percentage change of approximately $\beta_1 \times 100$ in the contribution base accumulated during the first 365 days following labor market entry in the provinces more exposed to the reform.[14]

5 Identification

In a DiD design with repeated cross-sections, the identification assumptions are more demanding than in panel settings (Heckman, Ichimura, and Todd, 1997; Abadie, 2005; Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). In both cases, identification relies on the core assumption of parallel trends. However, with repeated cross-sections, the estimand compares group-time averages rather than individual changes. As a result, an additional requirement is that the composition of the groups be stable over time, or that any compositional changes are fully captured by observed covariates that can be controlled for. Under these conditions, the change in the average outcome of the treated group relative to the control group identifies the treatment effect on the treated at the group level provided there are no anticipatory effects nor spillovers and assignment to treatment is exogenous.

Stable composition

Table A.1 shows the average characteristics of the treatment and control groups before and after the reform. The assumption of stable composition is satisfied for all co-variates except immigrant status and entrants' educational attainment. Close inspection of the data shows that the fall in the proportion of university graduates and the increase in the proportion of entrants with intermediate levels of education are of a similar magnitude in both groups. These common trends do not pose a threat to our identification strategy. Nevertheless, we also provide estimates using the contribution group of entrants' first job as a proxy for educational attainment, just in case. Furthermore, we account for heterogeneity in the treatment by educational level and estimate our baseline specification on subsamples of entrants with the same level of education. In the case of immigrants the changes are not balanced. To check the robustness of our results we therefore re-estimated our baseline specification on the subsample of natives.

Exogeneity

To achieve a maximum degree of exogeneity, our baseline specification focuses on the outcomes during the first employment spell and excludes firm-level covariates. The two co-variates that could potentially be affected by the reform in ways that differ across provinces are the age and province of entry. Changes in the mobility pattern of entrants would pose a problem if the reform reduced the mobility of the entrants with the highest chances on a permanent job from the provinces in the treatment group to one of the provinces in the control group. Reassuringly, the descriptive evidence

[14]The exact percentage effect is given by $100 \times (e^{\beta_1} - 1)$.

presented in Table A.1 shows that the mean entry age for each level of education is constant over time and virtually the same for the treatment and control group. Nevertheless, we also present results for the subsample of individuals who enter the labor market in their province of birth.

No anticipation effects

The 2021 reform was approved on December 28, five days after the Government reached an agreement with the social partners. The reform did not come as a total surprise. The negotiations started months earlier and the reform had to comply with the conditions laid down in the *Plan de Recuperación, Transformación y Resiliencia* (PRTR) and be implemented before the end of 2021.[15] To minimise the risk of anticipation effects, our baseline sample excludes the individuals who entered the labor market for the first time in the three months between October and December 2021. In addition, for outcomes based on duration —days of employment, cumulative earnings during the first year, spells of more than six months— we exclude the entire 2021 cohort to avoid contamination of our outcome variables. These outcomes are constructed over a 365-day window following entry. Thus, individuals who start their first job in the last months of 2021 may spend a substantial portion of their first employment year in 2022, fully overlapping with the post-reform period.

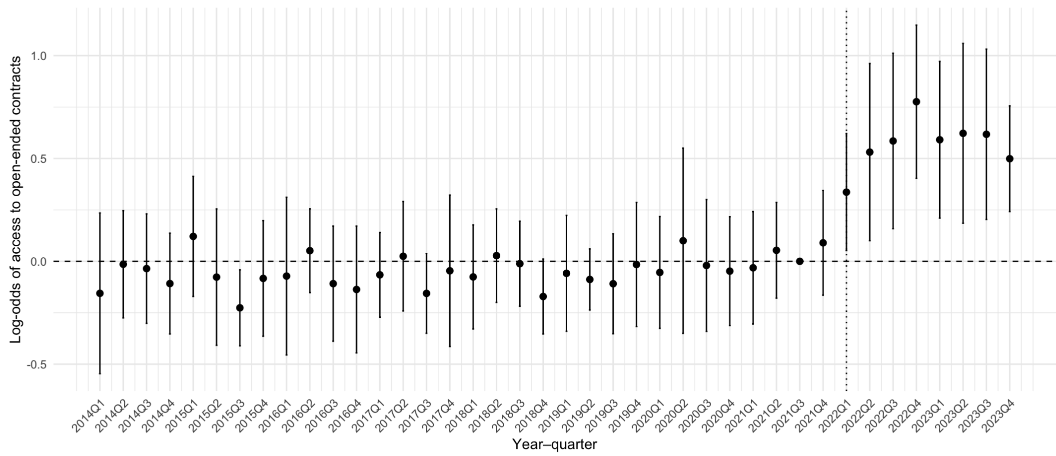
Parallel trends

To verify the parallel trends assumption, we use a standard event study design. The objective is to rule out any differential trends in the access to open-ended contracts prior to the reform.[16] Our event study specification includes interactions between the treatment dummy and quarter fixed effects for the entire sample period. The third quarter of 2021 is used as the base period as the fourth quarter is excluded from our dataset to avoid anticipation effects.

[15] The measures to deal with the excessive use of temporary contracts are laid down in Component 23 of the PRTR. For details, see https://planderecuperacion.gob.es/politicas$_{y}$$componentes/componente - 23 - nuevas - politicas - publicas - para - un - mercado - de - trabajo - dinamico - resiliente - e - inclusivo$

[16] Figures A.3 to A.10 in the Appendix provide visual and descriptive evidence on the plausibility of the parallel trends assumption for access to a standard open-ended contract in the first job and for the remaining labor market outcomes.

Figure 5. Differential effect of the labor reform on access to open-ended contracts



Notes: The figure shows the estimated coefficients from the *event study* model capturing the differential effect of the labor reform on the probability of accessing an open-ended contract (regular or intermittent open-ended) as the entry-type contract to the labor market. The reported coefficients correspond to log-odds estimated from a logit model. Since the treatment is interacted with year–quarter dummies, each coefficient can be interpreted as the differential effect of the reform in a specific quarter of a given year, relative to the pre-reform reference period (third quarter of 2021). Shaded bands represent 95% confidence intervals.

Figure 5 presents the results when we consider access to any open-ended contract (see Figure A.11 in the Appendix for access to a regular open-ended contract). Reassuringly, all the estimated coefficients for the pre-reform period are close to zero and statistically insignificant. In contrast, beginning in 2022, coinciding with the implementation of the labor market reform, the estimated coefficients become positive and increase progressively over subsequent quarters. This pattern is consistent with a gradual adjustment process in hiring decisions and suggests a persistent impact of the reform on access to permanent contracts in the more exposed provinces.

6 Results

This section presents our main results.

6.1 Access to open-ended contracts

We begin by analyzing how the 2021 labor market reform affected labor market entrants' access to permanent contracts at the start of their first job. Improved access to open-ended jobs is a prerequisite for an improvement in the stability of employment after the reform. Moreover, we want to be able to compare the relative magnitudes of the changes in entrants' contract type and in their subsequent labor market outcomes.

Columns (1) and (3) in Table 2 show the results for our baseline logit specification when we use access to any open-ended contract or access to regular open-ended contracts as our outcome

Table 2. Differential effect of the labor reform on access to open-ended contracts in the first job

	Logit				LPM			
	(1) Any open-ended contract	(2) Any open-ended contract	(3) Regular open-ended	(4) Regular open-ended	(5) Any open-ended contract	(6) Any open-ended contract	(7) Regular open-ended	(8) Regular open-ended
Post × Treated	0.636***	0.739***	0.576***	0.489***	0.068**	0.089***	0.042***	0.029*
	(0.172)	(0.125)	(0.090)	(0.073)	(0.029)	(0.020)	(0.013)	(0.017)
Post × Treated × Low education	–	-0.089	–	0.062	–	-0.024	–	-0.018
		(0.181)		(0.130)		(0.030)		(0.022)
Post × Treated × Lower-Mid education	–	-0.204	–	0.062	–	-0.046	–	0.004
		(0.130)		(0.079)		(0.025)		(0.004)
Post × Treated × Upper-Mid education	–	-0.152	–	0.091	–	-0.023	–	0.023
		(0.093)		(0.096)		(0.020)		(0.017)
Average marginal effect (pp)	6.373	-	4.010	-				
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓
Immigrants	✓	✓	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓	✓	✓
N	177,541	177,541	177,541	177,541	177,541	177,541	177,541	177,541
Adj. R^2 / Pseudo-R^2	0.174	0.176	0.116	0.118	0.195	0.197	0.128	0.131

Notes: The table reports Difference-in-Differences estimates, where *Treated* is defined based on the pre-reform provincial excess of temporary employment, and the post-reform period corresponds to 2022–2023. Coefficients capture the differential effect of the labor reform between provinces with higher and lower prior exposure to temporary contracts. Columns (1)–(4) present logit estimates for the probability of accessing an open-ended contract as the first job, distinguishing between access to any open-ended contract and access to regular open-ended contracts, with coefficients reported in log-odds. Columns (5)–(8) show linear probability model (LPM) estimates, whose coefficients can be interpreted directly as changes in probability in percentage points. The row "Average marginal effect" shows the average marginal effects derived from the logit models, expressed in percentage points, allowing the log-odds coefficients to be translated into comparable probability changes. All models include individual controls, employment trends, and fixed effects for province and cohort year. Robust standard errors clustered at the provincial level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The reference category is higher education.

variables. The coefficients on the interaction term *Post × Treated* capture the difference between the treatment and control group in the change in the log odds ratio, i.e. the logarithm of the ratio between the probability of accessing a permanent contract and the complementary probability of accessing a temporary contract, before and after the reform. The positive coefficients imply a stronger improvement for entrants in the treatment group. The coefficient estimates are statistically significant at the 1 percent level and the effect is strongest when we include both ordinary and discontinuous open-ended contracts.

Moreover, we find no evidence of significant heterogeneous effects by educational level (Columns 2 and 4).

As the logit coefficients cannot be interpreted directly in terms of probabilities, we also report the marginal effects in Table 2. These indicate how much the predicted probability of a given outcome changes when an explanatory variable varies, holding other factors constant. In the case of interactions between binary variables such as *Post × Treated*, this procedure allows us to measure directly the percentage point difference in the improvement in the access to open-ended contracts for the treatment and control group.[17] According to our estimates, this difference amounts to 6.4 pp when we consider any type of open-ended contract and 4.0 pp when we limit attention to regular

[17] The calculation proceeds by first estimating average predicted probabilities for each group–period combination (before and after the reform) and then computing the corresponding difference-in-differences in probabilities, thereby translating the nonlinear logit scale into directly interpretable probability changes.

open-ended contracts.

For the sake of completeness, Columns (5)–(8) report the corresponding results when we use a linear probability model (LPM).[18] In this case the coefficients reflect the percentage-point differences between the treatment and control group that are equivalent to the marginal effects derived from the logit specifications in columns (1)–(4). The LPM results confirm the robustness of our findings. The coefficient of 0.068 in Column (5) implies that the differential effect on the access to open-ended contracts amounts to 6.8 pp, compared to 4.2 pp for regular open-ended contracts (column 7); both effects are statistically significant. Allowing the effects to differ by level of education (Columns 6 and 8), increases the baseline effects slightly, but the interactions with education remain statistically insignificant. This confirms that the reform has uniformly improved the access to open-ended contracts in entrants' first job.

The magnitude of the effects becomes clear when we compare the differential impact of the reform to the pre-existing gap in the access to open-ended jobs for the individuals in the treatment and control groups. As reported in Table A.1, before the reform 17.5% of the entrants in the control group started their working career with an open-ended contract, compared to 10.2% of the entrants in the treated provinces, resulting in a gap of 7.3 pp. The estimated average marginal effect of 6.4 pp implies that the reform closed approximately 87% of this pre-reform gap (6.4/7.3). In other words, the geographic differences in the access to open-ended jobs nearly disappeared following the implementation of the reform. For access to regular open-ended contracts, the pre-reform gap was 7.2 pp (16.1% in control versus 8.9% in treated provinces). With an estimated marginal effect of approximately 4 pp, the reform reduced this gap by 55.5% (4/7.2).

6.2 Labor market outcomes in the first job

Having established the positive effect of the reform on the access to permanent contracts, we now proceed to analyse the differential effect of the reform on entrants' labor market outcomes. The results shown in Table 3 consider the results obtained in the first job during a maximum period of 12 months.

In line with the results of the previous section, we obtain a positive and statistically significant differential effect for all three outcome variables. However, the effects are modest in size and account for a smaller percentage of the corresponding pre-reform gaps than the improvements in the access to open-ended contracts reported in the previous section. In particular, the differential effect on the duration of the first spell is 8.2 days in favor of the entrants in the treatment group, compared to a pre-reform gap of 20.6 days, as shown in Table A.1[19]. Hence, the estimated effect on the duration of the first spell represents approximately 40 percent of the pre-reform gap in this outcome variable (8.2/20.6). Once again we do not obtain evidence of any heterogeneous effect by level of education. Consistent with the findings for duration, we find that the differential effect on the probability a

[18]LPM is easier to interpret, but it less flexible than a logit specification and may generate estimated probabilities outside the unit interval.

[19]The mean pre-reform durations of the first spell for the treatment and control group were, respectively, 88.3 and 108.9 days

Table 3. Differential effect of the labor reform on the quality of the first job

	(1) Days of employment	(2) Days of employment	(3) Spell $\geq$ 6 months	(4) Spell $\geq$ 6 months	(5) Contribution base	(6) Contribution base
Post × Treated	8.192***	7.365**	0.113*	0.184***	0.142***	0.107**
	(2.996)	(2.965)	(0.059)	(0.060)	(0.038)	(0.046)
Post × Treated × Low education	–	5.104	–	0.095	–	0.083
		(6.410)		(0.159)		(0.062)
Post × Treated × Lower-Mid education	–	-1.926	–	-0.154	–	0.015
		(4.821)		(0.107)		(0.050)
Post × Treated × Upper-Mid education	–	0.499	–	-0.089	–	0.059
		(4.313)		(0.087)		(0.062)
Average marginal effect (pp)	–	–	1.952	–	–	–
Individual controls	✓	✓	✓	✓	✓	✓
Immigrants	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Cohort year FE	✓	✓	✓	✓	✓	✓
N	163,238	163,238	163,238	163,238	161,904	161,904
Adj. R^2 / Pseudo-R^2	0.007	0.007	0.089	0.089	0.037	0.038

Notes: The table reports Difference-in-Differences estimates, where *Treated* is defined based on the pre-reform provincial excess of temporary employment, and the post-reform period corresponds to 2022–2023. Coefficients capture the differential effect of the labor reform between provinces with higher and lower prior exposure to temporary contracts. Columns (1)–(2) present OLS estimates for the duration, in days, of the first employment spell after entering the labor market. Columns (3)–(4) report logit models for the probability that the first spell lasts at least six months, with coefficients in log-odds. Columns (5)–(6) use as dependent variable the logarithm of the accumulated contribution base during the first year under the initial contract. The row "Pre-reform mean" shows average values of each outcome variable in control and treated provinces before the reform. The row "Average marginal effect" presents the average marginal effect derived from the logit model, expressed in percentage points, translating log-odds coefficients into directly interpretable probability changes. All models include individual controls, employment trends, and fixed effects for province and cohort year. Robust standard errors clustered at the provincial level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The reference category is higher education.

duration above six months is about 2 pp. Given the pre-reform gap of 6.1 pp, this amounts to a reduction of approximately 32 percent of the gap that existed before the reform.

Finally, the longer duration also results in a positive and statistically significant effect on earnings in the first spell. The estimated coefficient on the *Post* × *Treated* interaction is approximately 0.14 log points, with no evidence of heterogeneous effects across educational levels. As a result, the reform reduced the gap in earnings by approximately 14%. Given mean pre-reform earnings of €4,169 and €3,053 for the entrants in the control and treatment group, this is equivalent to a reduction of the earnings gap by approximately €150. Finally, in the case of daily earnings, the effects of the reform are close to zero and statistically insignificant[20]. The absence of this effect when considering average daily earnings suggests that the improvement is not driven by higher wages but by a greater number of days worked

6.3 Labor market outcomes during the first twelve months

To complete the analysis, Table 4 reports the results when we extend the analysis to take into account the entrants' entire working history during their first twelve months in the labor market. Allowing for multiple spells makes the outcomes less exogenous, but this extension is a useful first

[20] The results are available upon request.

Table 4. Differential effects of the labor reform on labor market outcomes during the first year

	(1) Days of employment	(2) Days of employment	(3) Spell $\geq$ 6 months	(4) Spell $\geq$ 6 months	(5) Contribution base	(6) Contribution base
Post × Treated	7.116***	13.877**	0.095**	0.190***	0.140***	0.201***
	(2.315)	(4.621)	(0.048)	(0.060)	(0.026)	(0.039)
Post × Treated × Low education	–	-5.287	–	0.007	–	-0.077
		(8.012)		(0.134)		(0.059)
Post × Treated × Lower-Mid education	–	-11.051*	–	-0.157*	–	-0.101*
		(6.571)		(0.091)		(0.054)
Post × Treated × Upper-Mid education	–	-6.114	–	-0.127	–	-0.038
		(6.204)		(0.082)		(0.056)
Individual controls	✓	✓	✓	✓	✓	✓
Immigrants	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓
Average marginal effect (pp)	-	-	1.764	-	-	-
N	163,238	163,238	163,238	163,218	161,904	161,904
Adj. R^2 / Pseudo-R^2	0.014	0.014	0.086	0.086	0.058	0.055

Notes: The table reports Difference-in-Differences estimates, where *Treated* is defined based on the pre-reform provincial excess of temporary employment, and the post-reform period corresponds to 2022–2023. Coefficients capture the differential effect of the labor reform between provinces with higher and lower prior exposure to temporary contracts. Columns (1)–(2) present OLS estimates for the total number of employment days accumulated during the first year after entering the labor market. Columns (3)–(4) report logit models for the probability of experiencing at least one employment spell lasting six months or more within that first year, with coefficients in log-odds. Columns (5)–(6) use as the dependent variable the logarithm of the total contribution base accumulated during the first year, aggregating all jobs held in that period.The row "Average marginal effect" presents the average marginal effect derived from the logit model, expressed in percentage points, translating log-odds coefficients into directly interpretable probability changes. All models include individual controls, employment trends, and fixed effects for province and cohort year. Robust standard errors clustered at the provincial level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The reference category is higher education.

step towards analysing the impact of the reform on the school-to work transition of Spanish youth. As it turns out, this extension only results in minor changes. A comparison of Tables 3 and 4 shows that the differential effects are marginally smaller than before. For instance, the reform reduced the difference in the length of the first spell by 8.2 days, while the corresponding decrease in the total number of days worked during the first twelve months is equal to 7.1 days. Furthermore, over a twelve-month horizon, weak evidence emerges of a heterogeneous impact by level of education. The differential effect for university graduates is 13.9 days, whereas for entrants with lower-medium levels of education the differential effect is close to zero.

The effect on the total number of days worked corresponds to approximately 27% of the pre-reform gap in this outcome (7.1/(187-161). Similarly, following the same logic as before, we find that the reform reduced the gap in the probability of a spell longer than six months by 21.5%, while the reform reduced the gap in annual earnings[21] by approximately 14%. In all three cases, we obtain the smallest effects for entrants with lower-medium education.

[21]When the outcome is average daily earnings, the estimated coefficient remains positive but smaller in magnitude. This indicates that the improvement in annual earnings is mainly driven by a greater number of days worked rather than higher wages. The corresponding estimates are available from the authors upon request.

6.4 Robustness

This section presents a series of robustness exercises.

To begin with, we replicate the main findings using Coarsened Exact Matching (CEM). Exact matching is performed on predetermined individual characteristics, including gender, immigrant status, educational attainment, age at labor market entry, and month of entry. Age at entry is discretized into intervals, allowing for exact matches within homogeneous age ranges.

The results, reported in Table A.3, are very similar to those obtained in the baseline analysis, both in terms of the magnitude and the statistical significance of the coeficient estimates. This discards the concerns that our results may be driven by differences in the observable characteristics of entrants in the treatment and control group.

As a second robustness exercise, we estimate the effects separately on subsamples of entrants with the same educational attainment. This creates more homogeneous samples and allows for differences in the parameter estimates across education levels. The results, shown in Table A.4, are again very similar to those obtained for our baseline specification with triple interactions. For the reference group of high educated entrants we can directly compare the coefficients of the interaction term *Post* $\times$ *Treated* in Tables 2 and 3. For the remaining categories the total effect in our baseline corresponds to the sum of the *Post* $\times$ *Treated* coefficient and the relevant triple interaction term *Post* $\times$ *Treated* $\times$ *Education*.

Our third robustness check addresses concerns about measurement error in the education variable. The information on education in the MCVL is imported from municipal population registers. The social security administration updates this information using administrative data on official degrees, but even so measurement error may be considerable for individuals who have recently entered the labor market. To address this issue, we follow a common practice in the literature and re-estimate the results using the contribution group of an individual as a proxy for educational attainment. Table A.5 shows that the effects of the reform are of similar size but less significant than in our baseline. The lower statistical significance also shows up in the pattern of heterogeneity by contribution group. One reason why this may occur is the endogenous nature of our proxy. In particular, after the reform firms may prefer to assign some entrants to a lower contribution group than before in order to compensate for the loss of flexibility due to the transformation of temporary into permanent jobs. For this reason we prefer to use entrants' education and not their contribution group in our baseline.

In our next robustness test we allow for possible non-linear effects of the reform. In order to do so, we redefine the treatment and control groups. Rather than comparing entrants in provinces with positive and negative values for Excess_p, we now restrict the sample to entrants in the provinces that belong to the top and the bottom tercile of Excess_p. In this case, we obtain slightly larger effects than in our baseline as shown in Table A.6. This pattern is consistent with the idea that the reform generated stronger effects precisely in those provinces with the largest margins for improvement.

Finally, in our baseline, treatment status is assigned according to the province of the first employer. A potential weakness of this procedure is that entrants may self-select into different provinces after the reform. To address this problem we re-estimate our baseline specification on a restricted sample of individuals who enter the labor market in their province of birth. By construction, this definition excludes all non-natives from the sample. To disentangle these two issues, we estimate the baseline specification under two alternative sample restrictions. First, in Table A.7 we restrict the sample to native individuals, excluding all immigrants. Next, in Table A.8 we restrict the sample to the ones whose province of birth coincides with the province of their first employer.

Neither of these restrictions alter our results, but we do observe a slight decrease in the coefficient estimates when we exclude the entrants who migrated to another province. One possible explanation for this result is that the reform reduced the outward mobility from the provinces with the weakest access to permanent contracts before the reform. It is likely that these individuals have an above-average propensity to land on a permanent job as mobility is costly and may not pay off in case of temporary jobs.

7 Average duration

In previous sections, we have presented robust evidence showing that the labor market reform improved entrants' access to open-ended contracts but had less impact on their subsequent labor market outcomes. The key explanation for this discrepancy is an increase in separation rates. New ordinary open-ended contracts are less stable than those signed prior to the reform.

Figure A.12 clearly shows the shorter duration of entrants' contracts. The average duration of new regular open-ended contracts decreased by more than 100 days after the reform, while the average duration of discontinuous permanent contracts decreased by approximately 50 days. Furthermore, in line with the evidence presented thus far, these changes in contract duration are more significant in the provinces that belong to our treatment group. To illustrate this point, we separated the entrants by the type of their first contract and re-estimated our baseline specification using as outcome variables the duration of the first spell. These estimates do not have a causal interpretation, as they are based on self-selected subsamples, but the coefficients provide a more precise picture of changes in average contract duration following the reform. In particular, as shown in Table A.9, after the reform the average duration of standard open-ended contracts among entrants declined an additional 30 days in the provinces that are part of our treatment group. Thus, in relative terms, access to standard open-ended contracts improved in the treated provinces. However, at the same time the duration of these contracts decreased more than in the provinces in our control group.[22]

In order to better understand the mechanisms underlying the shortening of contract duration, it is useful to examine the changes in the timing of each type of separation. Figures A.13 and A.14 show the distribution of the causes of separation and the time elapsed until separation for exits observed in a worker's first recorded employment spell. For regular open-ended contracts, no substantial changes are observed in the relative weights of the different causes of separation. Voluntary quits remain the primary cause of separation, accounting for around 60 percent of exits, with dismissals and other causes trailing far behind. What does change is the timing of these separations. After the

[22]We do not find statistically significant differences by educational attainment, although the results suggest that the changes are more concentrated among higher-educated entrants.

reform, the average time to separation decreases by approximately 50 days for dismissals and by more than a month for voluntary quits.

For the remaining contract types, the observed changes are more pronounced due to the substitution of temporary contracts by discontinuous permanent contracts. As a result, the share of temporary contract expirations declines, while transitions into inactivity periods among workers with discontinuous permanent contracts increase. Nevertheless, these changes have a limited impact on effective employment stability, as the activity periods associated with the new discontinuous permanent contracts tend to be, on average, shorter than before the reform.

Figure A.15 illustrates the net effect of these changes on the empirical distribution of accumulated days of employment. This evidence complements the mean outcomes reported in Table A.1. For entrants' first employment spells, the changes are concentrated in the lower tail of the distribution. After the reform we observe a fall in in the frequency of very short spells that is stronger in the treated provinces. The right-hand panel further shows that these distributional changes become more pronounced over the first year, affecting both the lower and the upper tail of the distribution.

To further explore these changes we estimated discrete-time duration models, following the methodology of Jenkins (1995), using data on the first employment spells. When no separation is observed within the time window covered by the data, the spell is treated as right-censored. Moreover, to maintain comparability across spells and prevent exceptionally long employment relationships from dominating the estimation, the analysis is restricted to the first 36 months of each employment spell. Spells exceeding this duration are therefore also right-censored at that point. This approach allows for the simultaneous inclusion of spells ending within the observation window and employment relationships that remain ongoing at the end of the data or beyond the 36-month horizon, avoiding bias from excluding ongoing jobs and ensuring consistent estimation of exit probabilities at each point in time.

The estimates reported in Table 5 indicate that, in aggregate terms, the reform did not significantly modify exit rates from employment among entrants in the more exposed provinces. In other words, despite the changes introduced in contractual regulation, the overall intensity of labor market turnover in the early stages of professional careers remains essentially unchanged. However, this aggregate result conceals relevant changes in the way such turnover occurs. When the effect of the reform is allowed to vary by contract type, exit rates are found not to change significantly for temporary contracts—which constitute the reference category—nor for discontinuous permanent contracts, whereas they increase significantly for standard open-ended contracts in the more exposed provinces.

Taken together, these results suggest that the reform primarily altered the channel through which labor market adjustment takes place. The reduction in the share of temporary contracts did not eliminate turnover; rather, part of it appears to have shifted toward open-ended employment relationships. A simple way to illustrate the magnitude of changes in employment stability is by means of the survival functions presented in Figure A.17. These figures show the evolution over

Table 5. Differential impact of the labor reform on job hazard rates

	(1) Hazard rate	(2) Hazard rate
Post × Treated	0.010	0.040
	(0.028)	(0.031)
Post × Treated × Intermittent open-ended		0.092
		(0.099)
Post × Treated × Regular open-ended		0.249***
		(0.065)
Individual controls	✓	✓
Macroeconomic controls	✓	✓
Duration polynomial	✓	✓
Province (FE)	✓	✓
Cohort year (FE)	✓	✓
Observations	805,547	805,547
Pseudo-R^2	0.119	0.157

Notes: The table reports Difference-in-Differences estimates from discrete-time duration logit models, where the dependent variable indicates the probability of leaving employment in each period conditional on having remained employed until that point (i.e., the hazard rate). The *Treated* indicator is defined based on the pre-reform provincial excess of temporary employment, and the post-reform period corresponds to 2022–2023. Coefficients are reported in log-odds and capture the differential effect of the labor reform on job hazard rates between provinces with higher and lower prior exposure to temporary contracts. Column (1) shows the average effect on the hazard rate, while column (2) allows this effect to vary by initial contract type. All models include individual and macroeconomic controls, a polynomial in employment spell duration, and fixed effects for province and cohort year. Robust standard errors clustered at the provincial level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The reference category is Open-ended.

time in the survival probability of a representative individual in the treated provinces.[23] In the case of regular open-ended contracts we observe a discrete drop in the survival rates around the time of the reform. The size of this drop is increasing in duration and reaches the level of 15 pp for a duration of twelve months. In contrast, the survival rates of intermittent open-ended contracts are stable over time, while we observe a minor improvement in the survival probability of temporary contracts.

8 Conclusions

This paper examines the impact of the 2021 labor market reform on labor market outcomes for young workers in Spain. The empirical strategy exploits the existence of persistent within-sector differences across provinces in the prevalence of temporary employment prior to the reform.

Overall, the results indicate that the 2021 labor market reform clearly improved the quality of contracts for new entrants to the labor market, particularly in provinces where temporary employment was more prevalent before the reform. As a result, the pre-existing gaps among entrants in the access to open-ended jobs almost disappeared. However, these improvements in the quality of the contracts did not give rise to an equivalent improvement in the labor market outcomes of the

[23] The survival functions are constructed from the predicted values of the duration model for provinces with positive excess temporary employment, holding the remaining covariates at their mean values. The curves reflect the evolution of the predicted survival probabilities under treatment for each duration.

entrants. While the reform eliminated nearly 90% of the gap in the probability of an open-ended contract at the start of an entrant's first job, it only reduced the gap in the duration of these jobs and the associated earnings of the entrants by 40% and 14%, respectively. The main explanation for these weaker effects are offsetting changes in the duration of regular open-ended contracts. On average, the duration of these jobs decreased by 100 days with stronger declines in the provinces with the highest prevalence of temporary contracts before the reform.

From a policy perspective, the evidence suggests that, while legal reforms to curb temporary employment are necessary, they are not sufficient to guarantee a substantial improvement in the stability of employment. In particular, the findings point to the importance of designing mechanisms that require firms to internalize the social costs associated with high levels of rotation. One way to achieve this objective without raising redundancy pay or tightening the rules for fair dismissals would be to implement a bonus-malus system that links firms' social security contributions to indicators of excess churning at the firm level.

Appendix

Table A.1. Observable characteristics of labor market entrants by treatment status

		Pre-reform (2014–2021)				Post-reform (2022–2023)			
Variable	**Category**	**Control**	**Treated**	**Diff.**	**Cohen**	**Control**	**Treated**	**Diff.**	**Cohen**
Gender	Male	51.169	52.683	1.514	0.030	50.623	51.650	1.027	0.021
	Female	48.831	47.317	-1.514	-0.030	49.377	48.350	-1.027	-0.021
Immigrant	No	77.913	83.381	5.468	0.139	72.434	79.517	7.082	0.166
	Yes	22.087	16.619	-5.468	-0.139	27.566	20.483	-7.082	-0.166
Education	Low	6.967	13.684	6.717	0.224	6.660	12.405	5.745	0.198
	Lower-Mid	28.391	34.641	6.249	0.135	37.245	42.497	5.253	0.107
	Upper-Mid	30.571	26.982	-3.589	-0.079	39.933	32.369	-7.564	-0.158
	High	34.070	24.693	-9.377	-0.206	16.163	12.729	-3.434	-0.098
Entry age by educational level (years)	Low	21.405	21.316	-0.089	-0.024	21.295	21.725	0.430	0.108
	Lower-Mid	20.768	20.571	-0.197	-0.061	20.252	20.062	-0.190	-0.058
	Upper-Mid	21.082	21.226	0.144	0.046	20.936	21.176	0.240	0.082
	High	22.587	22.526	-0.061	-0.020	24.431	24.014	-0.417	-0.153
Contribution group	Low	44.280	58.910	14.630	0.294	42.248	54.635	12.387	0.249
	Lower-Mid	32.412	25.490	-6.922	-0.153	34.477	28.577	-5.900	-0.127
	Upper-Mid	17.056	10.559	-6.496	-0.190	17.828	12.518	-5.310	-0.149
	High	6.252	5.041	-1.211	-0.053	5.448	4.270	-1.177	-0.055
Labor market outcomes	Access to open-ended contract	0.175	0.102	-0.072	-0.211	0.542	0.533	-0.009	-0.017
	Access to regular open-ended contract	0.161	0.089	-0.072	-0.219	0.345	0.313	-0.032	-0.067
	Days employed, first spell	108.867	88.261	-20.605	-0.176	100.389	86.836	-13.553	-0.123
	Days employed, first year	186.923	161.071	-25.852	-0.195	194.457	174.691	-19.767	-0.152
	First spell $\geq$ 6 months	0.244	0.183	-0.061	-0.148	0.212	0.169	-0.043	-0.108
	Any spell $\geq$ 6 months (year)	0.365	0.283	-0.082	-0.176	0.361	0.297	-0.065	-0.138
	Contribution base, first spell (€)	4169.384	3053.091	-1116.293	-0.215	4163.622	3250.359	-913.263	-0.179
	Total contribution base, first year (€)	6276.380	4711.169	-1565.211	-0.262	6749.120	5427.677	-1321.442	-0.220

Notes: The table reports proportions (percentages) and means for cohorts entering the salaried labor market. The difference is defined as Treated minus Control. Effect sizes correspond to Cohen's h for categorical variables and Cohen's d for continuous variables. Treated and Control groups are defined based on pre-reform provincial excess of temporary employment.

Table A.2. Baseline choice

	Binary treatment (Above/Below)							Continuous treatment						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Post × Treated	0.556***	0.585***	0.568***	0.576***	0.596***	0.567***	0.520***	–	–	–	–	–	–	–
	(0.081)	(0.087)	(0.089)	(0.090)	(0.093)	(0.077)	(0.074)							
Post × Continuous treatment	–	–	–	–	–	–	–	0.056***	0.058***	0.058***	0.059***	0.060***	0.057***	0.052***
								(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Education level	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Immigrant status	✓	–	✓	✓	–	✓	✓	✓	–	✓	✓	–	✓	✓
Employment trends	–	–	✓	✓	✓	✓	✓	–	–	✓	✓	✓	✓	✓
Province (FE)	–	–	✓	✓	✓	✓	✓	–	–	✓	✓	✓	✓	✓
Cohort year (FE)	–	–	–	✓	✓	✓	✓	–	–	–	✓	✓	✓	✓
Contribution group	–	–	–	–	–	✓	✓	–	–	–	–	–	✓	✓
Sector (FE)	–	–	–	–	–	–	✓	–	–	–	–	–	–	✓

Notes: The table reports Difference-in-Differences estimates from logit models for the probability that the first employment spell corresponds to a regular open-ended contract. The post-reform period corresponds to 2022–2023. Columns (1)–(7) define treatment as a binary indicator, equal to one for provinces with positive pre-reform excess temporary employment and zero otherwise; columns (8)–(14) define treatment as a continuous variable measuring the intensity of pre-reform excess temporary employment. Coefficients correspond to the interaction between the post-reform period and the treatment variable, capturing the differential effect of the labor reform between provinces with higher and lower prior exposure to temporary employment, reported in log-odds. Columns present progressively richer specifications in terms of controls, including individual characteristics, education level, immigrant status, local employment trends, contribution group, and various sets of fixed effects (province, cohort year, sector). Robust standard errors clustered at the province level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.3. Differential effect of the labor reform under coarsened exact matching

	(1) Access to open-ended	(2) Access to open-ended	(3) Access to regular open-ended	(4) Access to regular open-ended	(5) Employment days	(6) Employment days	(7) Spell $\geq$ 6 months	(8) Spell $\geq$ 6 months	(9) Contribution base	(10) Contribution base
Post × Treated	0.613***	0.701***	0.563***	0.530***	7.957**	5.838**	0.110*	0.136**	0.165***	0.127**
	(0.085)	(0.127)	(0.093)	(0.074)	(3.201)	(3.058)	(0.063)	(0.062)	(0.045)	(0.053)
Post × Treated × Low education	–	-0.014	–	0.057	–	7.200	–	0.159	–	0.089
		(0.175)		(0.137)		(6.525)		(0.172)		(0.069)
Post × Treated × Lower-middle education	–	-0.112	–	0.088	–	0.009	–	-0.091	–	0.011
		(0.125)		(0.079)		(4.673)		(0.110)		(0.051)
Post × Treated × Upper-middle education	–	-0.055	–	0.115	–	3.093	–	-0.015	–	0.053
		(0.089)		(0.095)		(4.188)		(0.089)		(0.062)
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Immigrant status	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
CEM	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	177,500	177,500	177,500	177,500	163,190	163,190	163,190	163,190	161,857	161,857
Adj. R^2 / Pseudo-R^2	0.154	0.155	0.091	0.092	0.006	0.006	0.052	0.053	0.034	0.033

Notes: The table reports Difference-in-Differences estimates using a sample matched through *Coarsened Exact Matching* (CEM). The variable *Treated* is defined based on the provincial pre-reform excess temporary employment. The post-reform period corresponds to 2022–2023. Columns (1)–(4) and (7)–(8) report logit model estimates; columns (5)–(6) use OLS for employment days in the first spell; columns (9)–(10) use the log of total contributions accumulated during the first year under the initial contract. All models include individual controls, employment trends, and province and cohort year fixed effects. Robust standard errors clustered at the province level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.4. Differential effects of the labor reform by level of education

	Access to open-ended	Access to regular open-ended	Employment days 1st spell	Spell $\geq$ 6 months	Contribution base 1st year
Panel A. Higher education					
Post $\times$ Treated	0.746***	0.491***	9.061***	0.209***	0.116**
	(0.122)	(0.072)	(3.007)	(0.061)	(0.044)
N	46,334	46,334	43,480	43,480	42,996
Adj. R^2 / Pseudo-R^2	0.139	0.121	0.012	0.106	0.043
Panel B. Low education					
Post $\times$ Treated	0.671***	0.582***	12.123*	0.266*	0.160**
	(0.242)	(0.148)	(6.155)	(0.157)	(0.064)
N	17,460	17,460	16,146	16,146	16,016
Adj. R^2 / Pseudo-R^2	0.226	0.153	0.008	0.072	0.031
Panel C. Lower-middle education					
Post $\times$ Treated	0.545***	0.560***	5.268	0.028	0.096**
	(0.191)	(0.082)	(3.838)	(0.078)	(0.039)
N	59,082	59,082	53,992	53,992	53,643
Adj. R^2 / Pseudo-R^2	0.185	0.120	0.005	0.057	0.019
Panel D. Upper-middle education					
Post $\times$ Treated	0.574***	0.565***	7.389**	0.077	0.149***
	(0.165)	(0.115)	(3.356)	(0.063)	(0.048)
N	54,665	54,665	49,620	49,620	49,249
Adj. R^2 / Pseudo-R^2	0.165	0.103	0.007	0.073	0.024

Notes: Each panel reports Difference-in-Differences estimates obtained from separate models for each education level. The variable *Treated* is defined based on the provincial pre-reform excess temporary employment, and the post-reform period corresponds to 2022–2023. Reported coefficients capture the differential effect of the labor reform between provinces with higher versus lower prior exposure to temporary employment within each education group. This specification is equivalent to estimating a joint model including a triple interaction *Post* $\times$ *Treated* $\times$ *education level*, with the table reporting the total effect for each group. Columns on access to open-ended and regular open-ended contracts, as well as the probability of reaching a spell of at least six months, are estimated via logit models and reported in log-odds, while employment days of the first spell are estimated via OLS and the contribution base of the first year is measured in logarithms. All models include individual controls, employment trends, and province and cohort year fixed effects. Robust standard errors clustered at the province level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.5. Differential effects of the labor reform by contribution group

	(1) Access to open-ended	(2) Access to open-ended	(3) Access to regular open-ended	(4) Access to regular open-ended	(5) Employment days	(6) Employment days	(7) Spell $\geq$ 6 months	(8) Spell $\geq$ 6 months	(9) Contribution base	(10) Contribution base
Post × Treated	0.600***	0.519***	0.548***	0.327**	6.972**	9.861*	0.097	0.126	0.139***	0.123
	(0.162)	(0.129)	(0.077)	(0.130)	(2.894)	(5.576)	(0.059)	(0.097)	(0.038)	(0.104)
Post × Treated × Low contribution	–	0.159	–	0.281	–	-1.572	–	0.030*	–	0.025
		(0.234)		(0.161)		(5.794)		(0.089)		(0.119)
Post × Treated × Lower-middle contribution	–	-0.111	–	0.131	–	-7.308	–	-0.098	–	-0.021
		(0.167)		(0.151)		(7.634)		(0.125)		(0.143)
Post × Treated × Upper-middle contribution	–	-0.253	–	-0.052	–	-8.807	–	-0.224*	–	-0.060
		(0.172)		(0.141)		(6.675)		(0.130)		(0.105)
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Immigrants	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Contribution group	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	173,860	173,860	173,860	173,860	159,833	159,833	159,833	159,833	158,500	158,500
Adj. R^2 / Pseudo-R^2	0.173	0.173	0.109	0.112	0.008	0.008	0.086	0.087	0.039	0.039

Notes: The table reports Difference-in-Differences estimates where the contribution group is used as a *proxy* for education level, and the variable *Treated* is defined based on provincial pre-reform excess temporary employment. The post-reform period corresponds to 2022–2023. Reported coefficients capture the differential effect of the labor reform between provinces with higher versus lower prior exposure to temporary employment within each contribution group. Columns (1)–(4) and (7)–(8) report logit estimates in log-odds for access to open-ended employment, access to regular open-ended contracts, and the probability that the first employment spell lasts at least six months. Columns (5)–(6) report OLS estimates for the number of days worked during the first spell, and columns (9)–(10) use as dependent variable the logarithm of the total contribution base accumulated during the first year under the initial contract. All models include individual controls, employment trends, and province and cohort year fixed effects. Robust standard errors clustered at the province level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The reference category is the high contribution group.

Table A.6. Differential effects of the labor reform: upper vs lower tertile of $Excess_p$

	(1) Access to open-ended	(2) Access to open-ended	(3) Access to regular open-ended	(4) Access to regular open-ended	(5) Employment days	(6) Employment days	(7) Spell $\geq$ 6 months	(8) Spell $\geq$ 6 months	(9) Contribution base	(10) Contribution base
Post × Treated	0.721***	0.796***	0.620***	0.521***	9.128***	11.365***	0.112*	0.235***	0.164***	0.167**
	(0.146)	(0.111)	(0.103)	(0.074)	(3.433)	(3.091)	(0.069)	(0.063)	(0.044)	(0.042)
Post × Treated × Low education	–	-0.057	–	0.043	–	0.977	–	0.034	–	0.041
		(0.198)		(0.141)		(7.305)		(0.181)		(0.073)
Post × Treated × Lower-middle education	–	-0.152	–	0.091	–	-4.769	–	-0.202	–	-0.018
		(0.130)		(0.091)		(5.812)		(0.125)		(0.048)
Post × Treated × Upper-middle education	–	-0.138*	–	0.094	–	-4.707	–	-0.186**	–	-0.008
		(0.077)		(0.109)		(4.654)		(0.084)		(0.062)
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Immigrants	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	141,266	141,266	141,266	141,266	130,037	130,037	130,037	130,037	128,902	128,902
Adj. R^2 / Pseudo-R^2	0.168	0.169	0.117	0.118	0.008	0.008	0.076	0.076	0.038	0.038

Notes: The table reports Difference-in-Differences estimates in which *Treated* is defined using provinces in the top tercile of excess temporary employment in the pre-reform period, while the control group consists of provinces in the bottom tercile of this distribution. The post-reform period corresponds to 2022–2023. Reported coefficients capture the differential effect of the labor reform between provinces with higher and lower prior excess temporary employment within this restricted sample. Columns (1)–(4) present logit estimates for access to open-ended contracts in the first job, distinguishing between any open-ended contract and regular open-ended contracts. Columns (5)–(6) report OLS estimates for the number of employment days in the first employment spell. Columns (7)–(8) present logit estimates for the probability that the first employment spell lasts at least six months. Columns (9)–(10) use the logarithm of the contribution base accumulated during the first year under the initial contract as the dependent variable. Even-numbered columns allow the differential effect to vary by education through interaction terms, with higher education as the reference category. All models include individual controls, employment trends, and province and entry-year fixed effects. Robust standard errors clustered at the province level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.7. Differential effect of the labor reform on natives

	(1) Access to open-ended	(2) Access to open-ended	(3) Access to regular open-ended	(4) Access to regular open-ended	(5) Employment days	(6) Employment days	(7) Spell $\geq$ 6 months	(8) Spell $\geq$ 6 months	(9) Contribution base	(10) Contribution base
Post × Treated	0.582***	0.750***	0.583***	0.583***	6.911*	6.101	0.073	0.177**	0.152**	0.123*
	(0.161)	(0.131)	(0.080)	(0.080)	(2.972)	(3.238)	(0.055)	(0.087)	(0.046)	(0.055)
Post × Treated × Low education	–	-0.220	–	-0.055	–	-0.822	–	-0.088	–	0.049
		(0.139)		(0.155)		(6.433)		(0.214)		(0.085)
Post × Treated × Lower-middle education	–	-0.255*	–	-0.013	–	-0.735	–	-0.136	–	0.019
		(0.120)		(0.092)		(5.455)		(0.149)		(0.059)
Post × Treated × Upper-middle education	–	-0.180	–	0.020	–	1.399	–	-0.113	–	0.054
		(0.092)		(0.116)		(5.255)		(0.128)		(0.072)
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	140,675	140,675	140,675	140,675	129,299	129,299	129,299	129,299	128,182	128,182
Adj. R^2 / Pseudo-R^2	0.152	0.153	0.079	0.079	0.006	0.006	0.073	0.073	0.028	0.028

Notes: Difference-in-Differences estimates for individuals born in Spain. The variable *Treated* is defined based on the provincial pre-reform excess temporary employment. The post-reform period corresponds to 2022–2023. Columns (1)–(2) report logit estimates for access to any open-ended contract (*fijo_bin*). Columns (3)–(4) report logit estimates for the probability of accessing a regular open-ended contract in the first job (*y_bin*). Columns (5)–(6) present OLS estimates for the number of days worked under the first contract of the first employment spell. Columns (7)–(8) report logit estimates for the probability that the first employment spell lasts at least six months. Columns (9)–(10) use as dependent variable the logarithm of the contribution base during the first job. All models include individual controls, employment trends, and province and entry-year fixed effects. Robust standard errors clustered at the province level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The reference category is higher education.

Table A.8. Differential effect of the labor reform on stayers

	(1) Access to open-ended	(2) Access to open-ended	(3) Access to regular open-ended	(4) Access to regular open-ended	(5) Employment days	(6) Employment days	(7) Spell $\geq$ 6 months	(8) Spell $\geq$ 6 months	(9) Contribution base	(10) Contribution base
Post × Treated	0.480***	0.635***	0.541***	0.596***	5.094**	6.581*	0.013	0.212**	0.130***	0.093*
	(0.143)	(0.137)	(0.077)	(0.102)	(2.168)	(3.544)	(0.049)	(0.092)	(0.033)	(0.055)
Post × Treated × Low education	–	-0.166	–	-0.088	–	-5.375	–	-0.309*	–	-0.008
		(0.143)		(0.154)		(4.760)		(0.161)		(0.102)
Post × Treated × Lower-middle education	–	-0.236*	–	-0.126	–	-3.102	–	-0.242*	–	0.037
		(0.124)		(0.105)		(4.950)		(0.140)		(0.066)
Post × Treated × Upper-middle education	–	-0.144	–	-0.008	–	0.237	–	-0.171	–	0.082
		(0.102)		(0.107)		(5.370)		(0.132)		(0.068)
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Immigrant status	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
CEM	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	99,122	99,122	99,122	99,122	90,933	90,933	90,933	90,933	90,263	90,263
Adj. R^2 / Pseudo-R^2	0.079	0.167	0.091	0.080	0.005	0.005	0.076	0.076	0.025	0.025

Notes: The table reports Difference-in-Differences estimates for a sample restricted to workers whose province of birth coincides with the province of the Social Security contribution account of their first employer. This restriction focuses on individuals without interprovincial mobility at labor market entry and indirectly excludes workers born outside Spain. The variable *Treated* is defined based on the provincial pre-reform excess temporary employment. The post-reform period corresponds to 2022–2023. Columns (1)–(4) and (7)–(8) report logit model estimates; columns (5)–(6) use OLS for employment days in the first spell; columns (9)–(10) use the log of total contributions accumulated during the first year under the initial contract. All models include individual controls, employment trends, and province and entry year fixed effects. Robust standard errors clustered at the province level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.9. Differential effect of the labor reform on days worked in the first spell by contract type

	(1)	(2)	(3)	(4)	(5)	(6)
	Regular open-ended		Intermittent open-ended		Temporary	
Post × Treated	-34.655***	-30.924***	0.047	4.339	1.078	-4.853
	(5.266)	(9.103)	(10.642)	(11.640)	(2.055)	(3.194)
Post × Treated × Low education	–	22.999	–	2.514	–	-3.658
		(18.081)		(19.370)		(5.899)
Post × Treated × Lower-middle education	–	-7.911	–	-8.820	–	-4.301
		(9.483)		(16.025)		(7.703)
Post × Treated × Upper-middle education	–	-14.472	–	2.835	–	10.023
		(8.087)		(10.367)		(3.611)
Individual controls	✓	✓	✓	✓	✓	✓
Immigrants	✓	✓	✓	✓	✓	✓
Employment trends	✓	✓	✓	✓	✓	✓
Province (FE)	✓	✓	✓	✓	✓	✓
Cohort year (FE)	✓	✓	✓	✓	✓	✓
N	29,688	29,688	10,545	10,545	123,005	123,005
Adj. R^2	0.012	0.012	0.017	0.017	0.007	0.007

Notes: The table reports Difference-in-Differences estimates, where *Treated* is defined based on the pre-reform provincial excess of temporary employment, and the post-reform period corresponds to 2022–2023. The coefficients capture the differential effect of the labor reform between provinces with higher and lower prior exposure within this restricted sample. Each block of columns limits the sample to workers whose first contract corresponds to the indicated type: regular open-ended contracts in columns (1)–(2), intermittent open-ended contracts in columns (3)–(4), and temporary contracts in columns (5)–(6). The dependent variable is the number of days worked during the first employment spell. All models include individual controls, employment trends, and fixed effects for province and entry year. Robust standard errors clustered at the province level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The reference category is higher education.

The table reports Difference-in-Differences estimates, where *Treated* is defined based on the pre-reform provincial excess of temporary employment, and the post-reform period corresponds to 2022–2023.

Figure A.1. Evolution of excess temporary employment by province

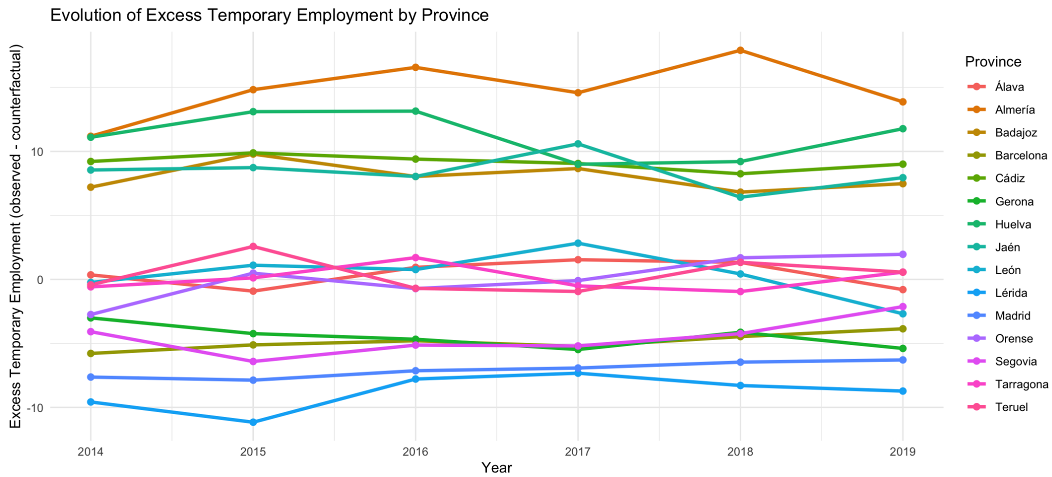
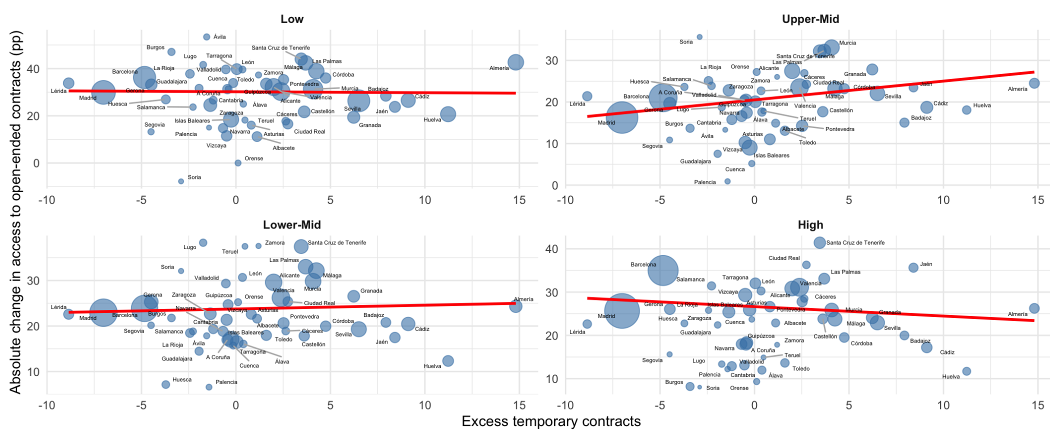


Figure A.2. Absolute change in access to regular open-ended contracts by education level



Notes: The figure shows for each province the relationship between pre-reform excess temporary employment and the absolute change in access to regular open-ended contracts after the labor reform, disaggregated by education level. The absolute change is calculated as the variation in percentage points between the pre-reform period (2014–2021) and the post-reform period (2022–2023). Each point represents a province, with size proportional to the province's relative weight in the pre-reform period, measured by the number of labor market entrants. The solid lines represent weighted linear fits, giving greater weight to provinces with larger sample sizes.

Figure A.3. Trends in access to open-ended contracts by education level

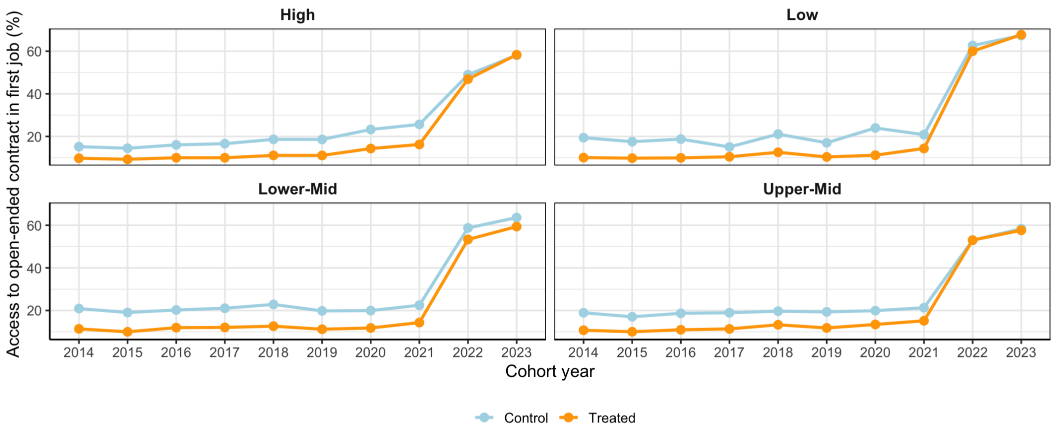


Figure A.4. Trends in access to regular open-ended contracts by education level

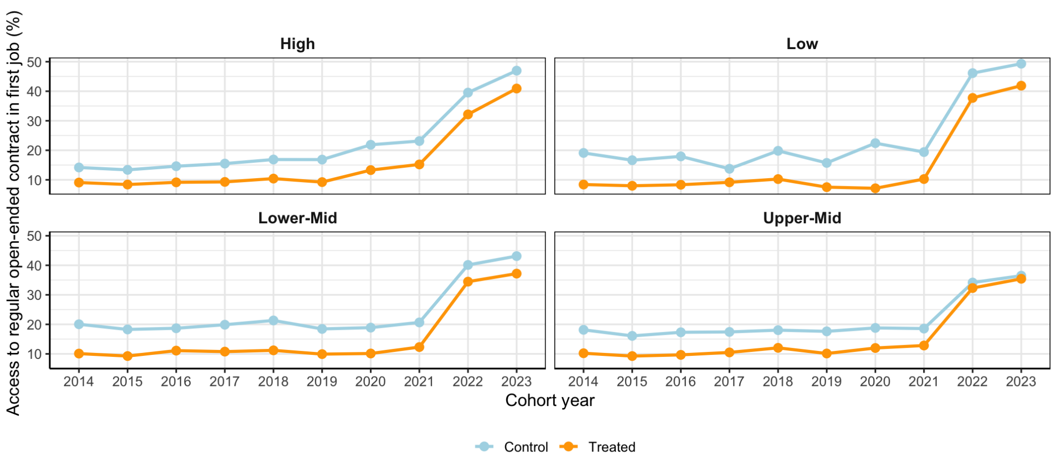


Figure A.5. Trends in total days worked in the first spell by education level

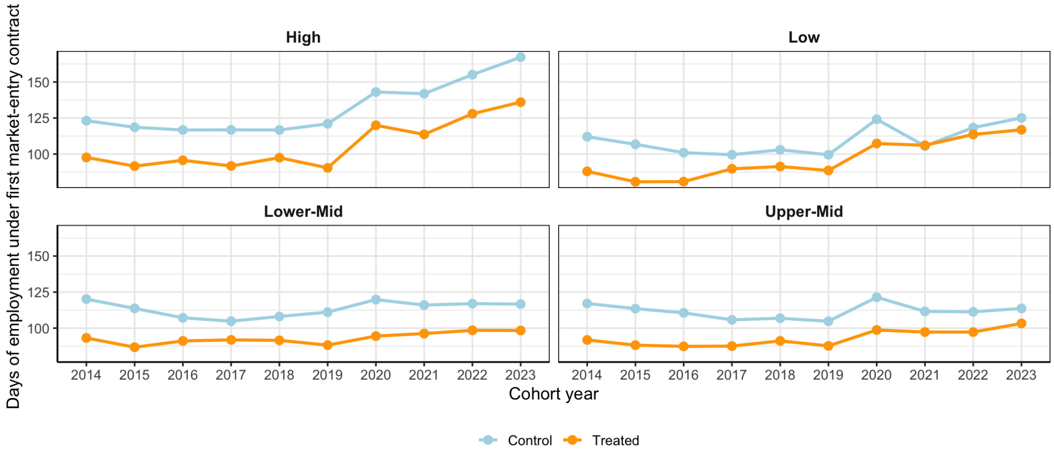


Figure A.6. Trends in total days worked in the first year by education level

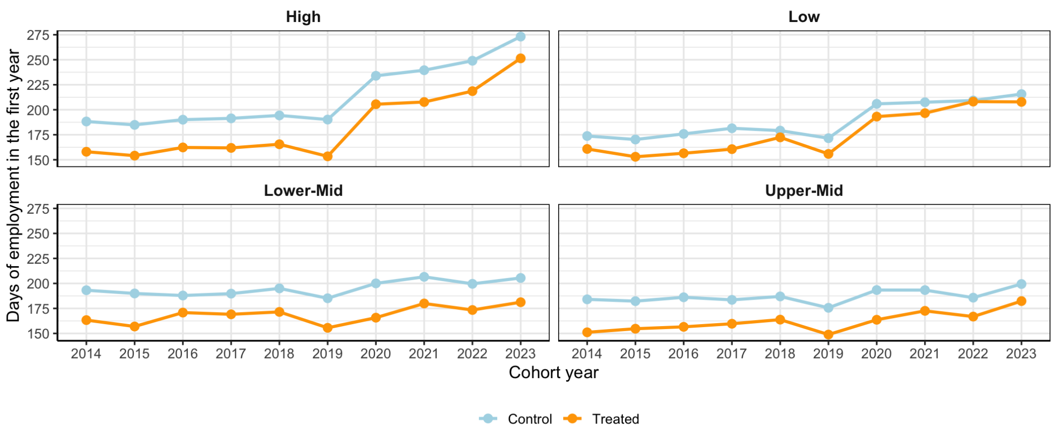


Figure A.7. Trends in the probability that the first spell lasts at least six months by education level

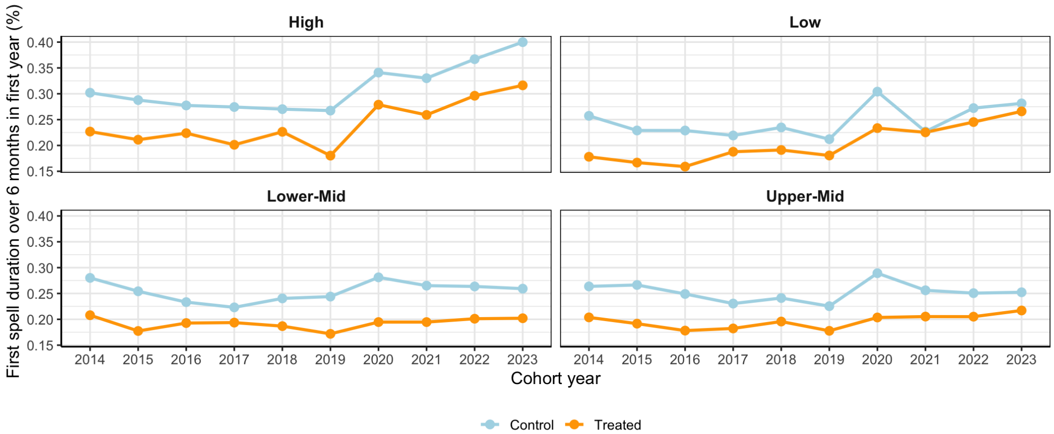


Figure A.8. Trends in the probability of a spell lasting at least six months by education level

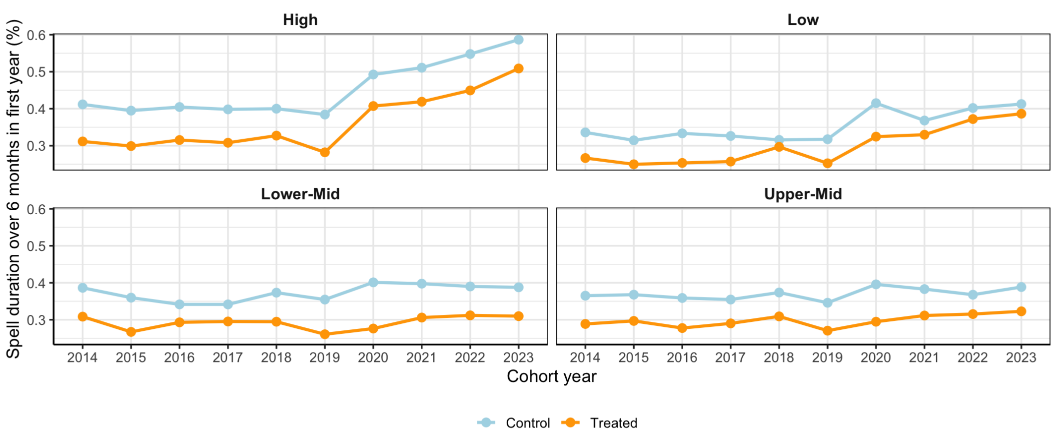


Figure A.9. Trends in the cumulative annual contribution base during the first spell by education level

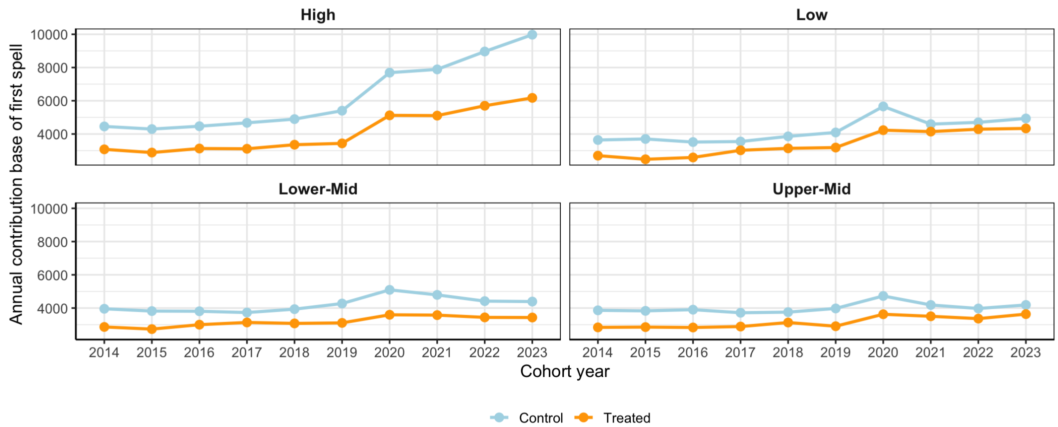


Figure A.10. Trends in the cumulative annual contribution base across all spells by education level

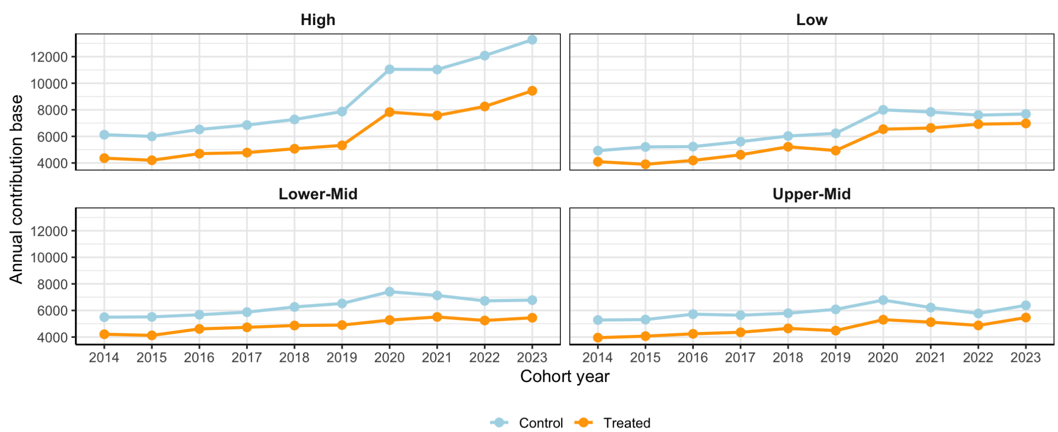
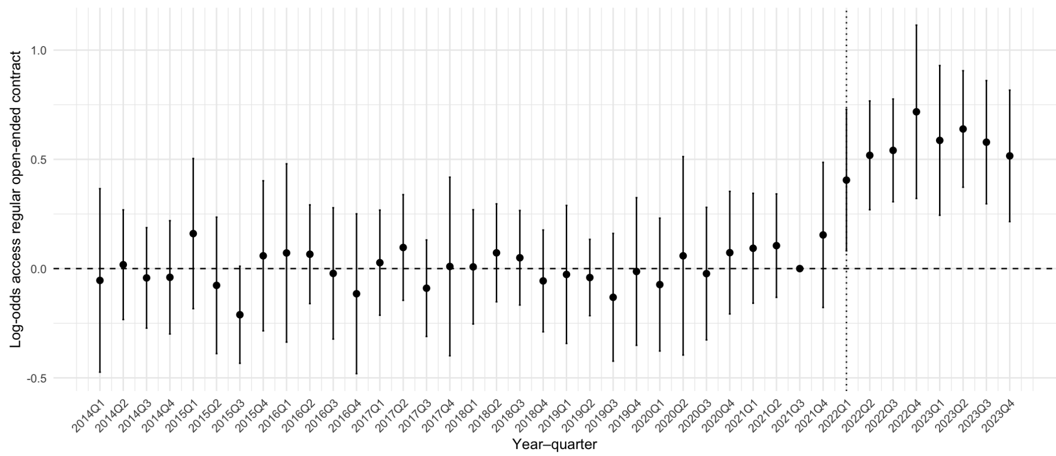


Figure A.11. Differential effect of the labor reform on access to regular open-ended contracts



Notes: The figure shows the estimated coefficients from the event study model, capturing the differential effect of the labor reform on the probability of accessing a regular open-ended contract as a first job. The coefficients are reported in log-odds estimated from a logit model. Since the treatment is interacted with year-quarter dummies, each coefficient represents the differential effect of the reform in a specific quarter relative to the reference pre-reform period (Q3 2021). The shaded areas represent 95% confidence intervals.

Figure A.12. Duration of initial employment spells in the first year by contract type

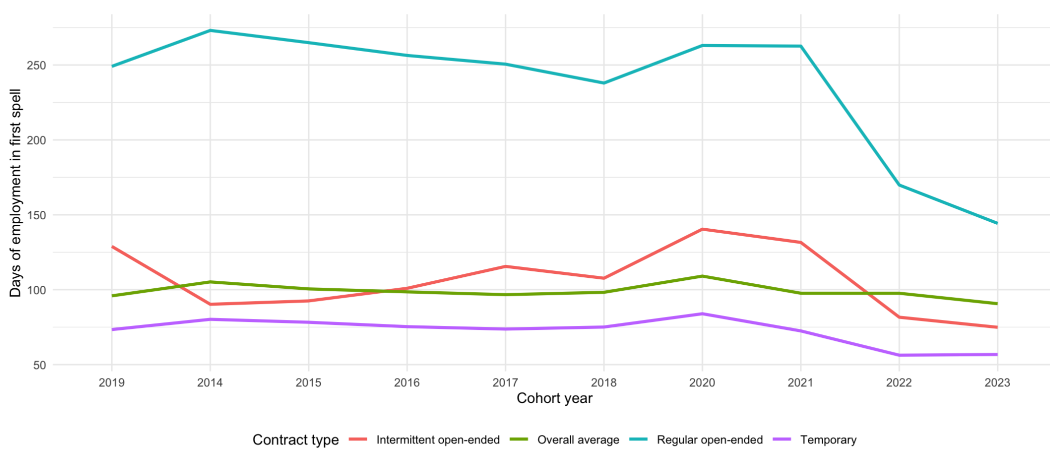
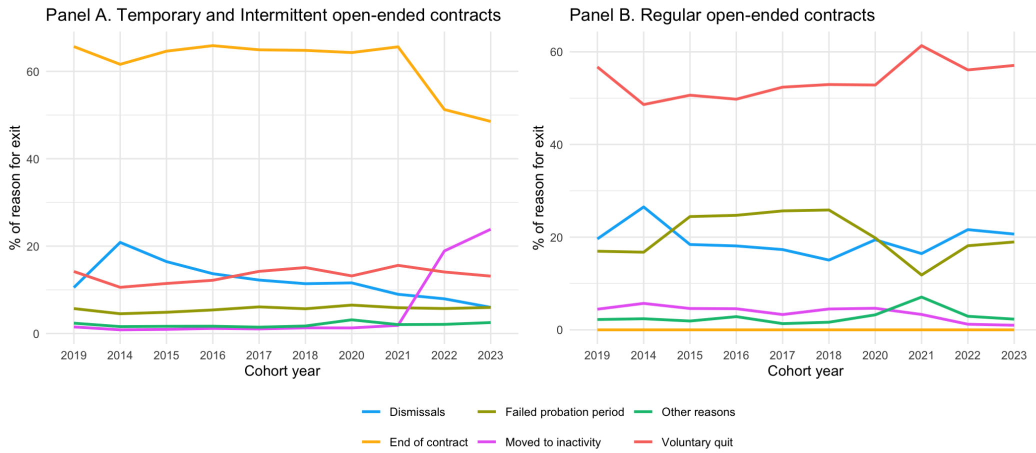
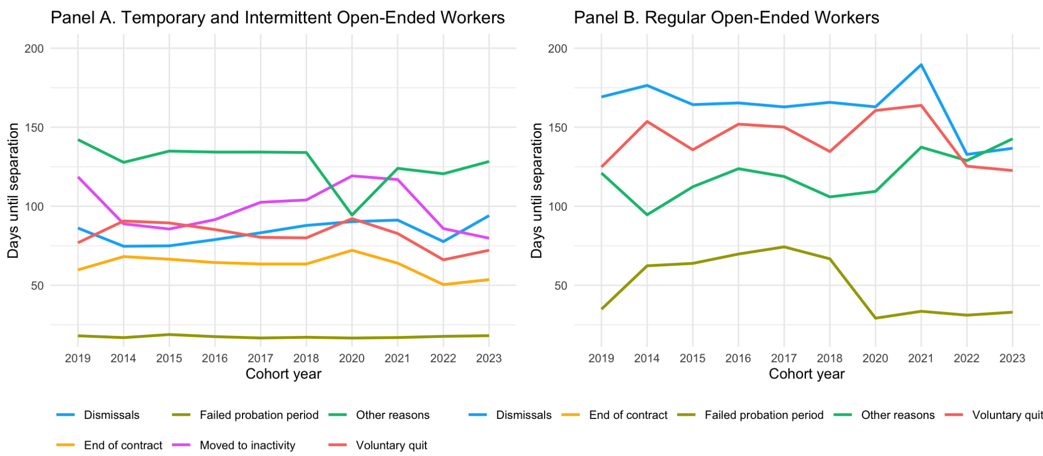


Figure A.13. Reasons for exits in the first spell during the first twelve months since labor market entry



Notes: The figure shows the evolution of reasons for exit associated with employment spells started in each entry cohort. The analysis is restricted to exits occurring within the twelve months following initial labor market entry. Exit reasons are grouped into the following categories: *Voluntary quit:* includes exits due to worker resignation (MCVL code 51). *Failure to pass probation:* includes exits associated with not completing the initial probation period of the contract (code 85). *Dismissals:* covers involuntary exits related to employer decisions or termination of the employment relationship, including involuntary exit (54), exit due to company merger or acquisition (55), temporary suspension due to collective dismissal (69), collective redundancy (77), objective reasons on the part of the employer (91), and objective reasons on the part of the worker (92). *Contract end:* includes exits due to the completion of temporary or fixed-term contracts (code 93). *Transition to inactivity:* includes cases of temporary or administrative exit from employment, such as moving to pension status (58), administrative exits or reviews (60), temporary incapacity and exhaustion of sick leave (65), leave for childcare (68) or family care (73), contract suspension (74), and transition to inactivity for intermittent permanent workers (94). Finally, *Other reasons* groups residual or unclassified motives. A detailed definition of each exit reason code is available in the Continuous Sample of Working Lives (MCVL) Guide.

Figure A.14. Days until job exit by contract type



Notes: The figure shows the evolution of days elapsed until job exit for the different causes of exit associated with employment spells started in each entry cohort. The analysis is limited to exits from the first spell occurring within twelve months after initial labor market entry.

Figure A.15. Density function of days of employment

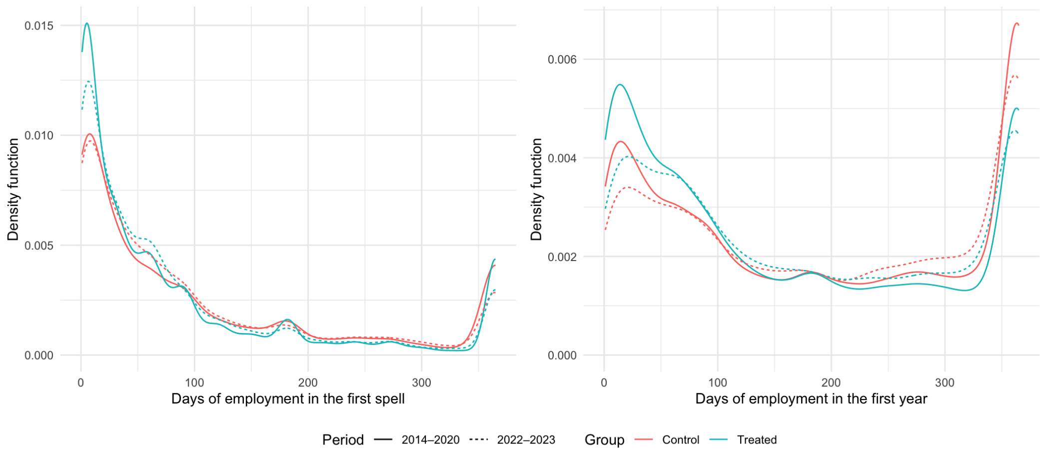


Figure A.16. Evolution of entry contract type

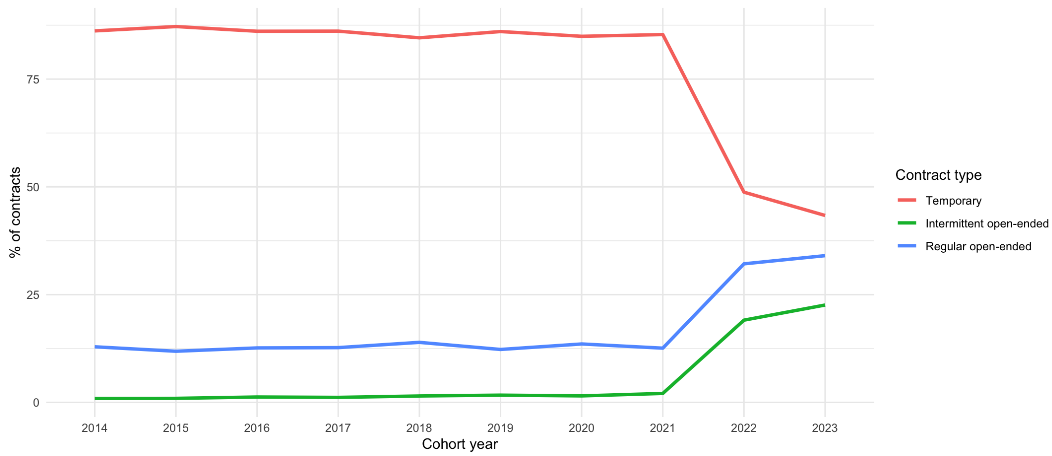
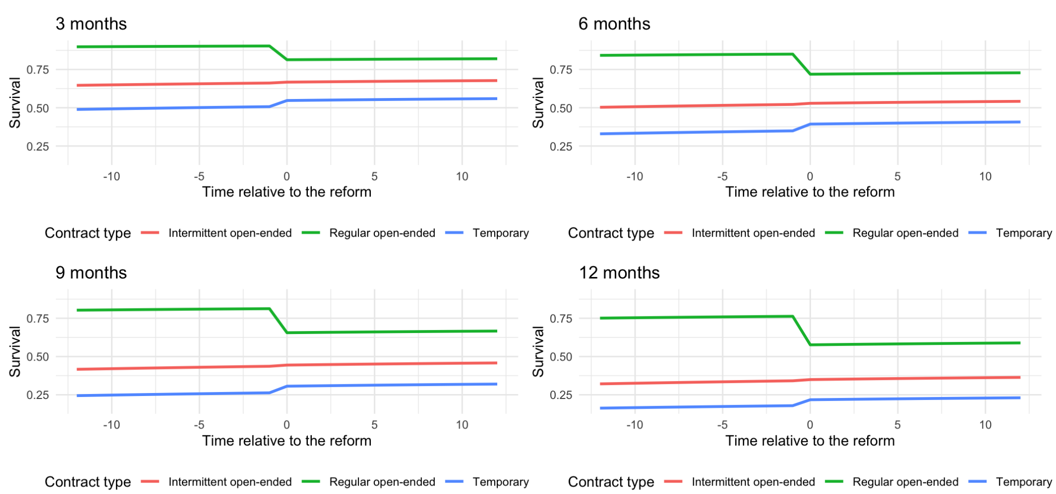


Figure A.17. Employment survival rates



Notes: The figure shows predicted probabilities of remaining employed (survival function) obtained from a discrete-time duration logit model. Each panel presents the cumulative probability of staying employed as a function of time relative to the labor reform (t = 0), for different potential contract durations (3, 6, 9, and 12 months). The curves are calculated from the estimated exit probabilities in each period, holding other covariates at their mean values or reference categories. Three contract types are distinguished: temporary, intermittent open-ended, and regular open-ended.

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