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# Does Limiting Temporary Work Improve Employment Stability? Evidence for Young Workers in Spain

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# Does Limiting Temporary Work Improve Employment Stability?

## Evidence for Young Workers in Spain\*

### Abstract

The labor market reform enacted in Spain in December 2021 introduced unprecedented restrictions on the use of temporary contracts with the aim of making open-ended employment the norm rather than the exception. This paper examines the reform's impact on the quality of contracts and the labor market outcomes of young labor market entrants using administrative records from the Continuous Sample of Working Lives (MCVL). We identify the causal effects of the reform through a difference-in-differences design that exploits persistent pre-reform differences across provinces in the prevalence of temporary employment. To this end, we construct a novel measure of temporary employment that adjusts for differences in the sectoral composition of employment across provinces. We show that the reform led to a near-complete convergence in the probability that entrants obtained an open-ended contract in their first job. However, this substantial improvement in contractual stability translated into only modest convergence in subsequent labor market outcomes. We demonstrate that this apparent discrepancy is the result of two developments. Firstly, part-time and intermittent contracts account for over 80% of the increase in access to open-ended contracts. Secondly, the average length of entrants' regular open-ended contracts decreased following the reform, particularly in provinces where temporary employment was most prevalent prior to the reform.

### JEL classification

J08, J21, J41, J63

### Keywords

dual labor markets, labor market reforms, 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 employment stability and earnings remains an open question.

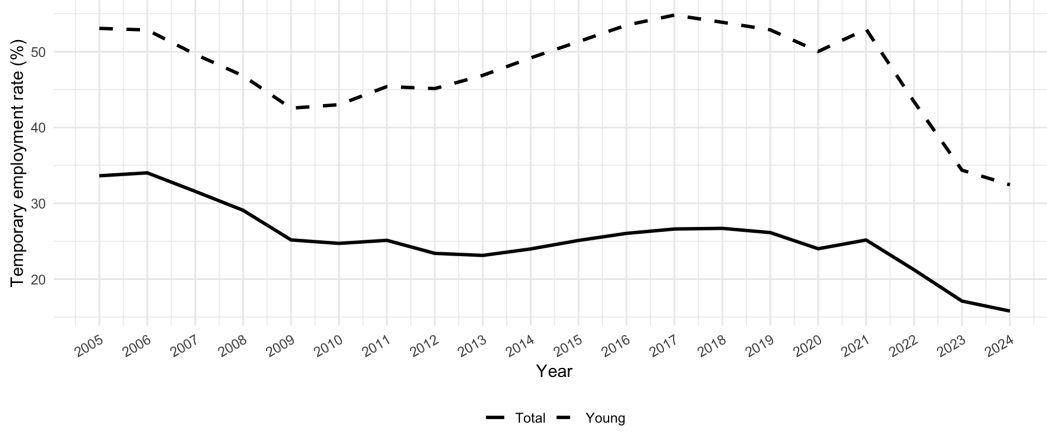
This paper studies the reform’s impact on young people entering the labor market. Using administrative records from the Continuous Sample of Working Lives (MCVL), we analyze how the 2021 reform affected the quality of their contracts and a broad set of labor market outcomes, including the length of their employment spells, and their earnings levels. There are two reasons why we have chosen to focus on young workers. Firstly, prior to the reform, young workers had by far the highest rate of temporary employment of all age groups (see Figure 1). While temporary contracts can play a useful role as a screening device or stepping stone towards permanent employment, the evidence for Spain indicates that they primarily served as a tool to reduce firms’ labor costs and to act as a buffer stock against demand fluctuations.<sup>1</sup> Consequently, young workers were disproportionately exposed to prolonged periods of insecure employment with low wages, both compared to adult workers and young workers elsewhere in Europe (Dolado et al., 2013). On the contrary, these days more than half of the labor market entrants manage to secure an open-ended contract in their first job (See Figure 2), and it is important to analyze how this affected their employment stability. Secondly, a large body of literature shows that the delays in the transition from education to stable employment in the pre-reform period hindered the accumulation of human capital, with negative consequences that may persist throughout the working life (see, for example, García-Pérez et al., 2019; Cabrales et al., 2017). Therefore, by shortening this transition period, the reform may help to improve productivity growth.

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

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<sup>1</sup>Since the seminal work of Alba-Ramírez (1998), numerous studies have demonstrated that temporary contracts seldom act as stepping stones to permanent jobs in Spain (e.g. Amuedo-Dorantes, 2000, Dolado et al., 2002, and Güell and Petrongolo, 2007). The findings of García-Pérez and Muñoz Bullón (2011) are particularly pertinent in the context of youth employment. Based on an analysis of social security records for young temporary workers between 1996 and 2003, the authors conclude that temporary contracts generally do not constitute stepping-stones for young workers. Finally, Casquel and Cunyat (2011) show that the likelihood of transiting from temporary to permanent employment increases with the level of education of temporary workers.

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



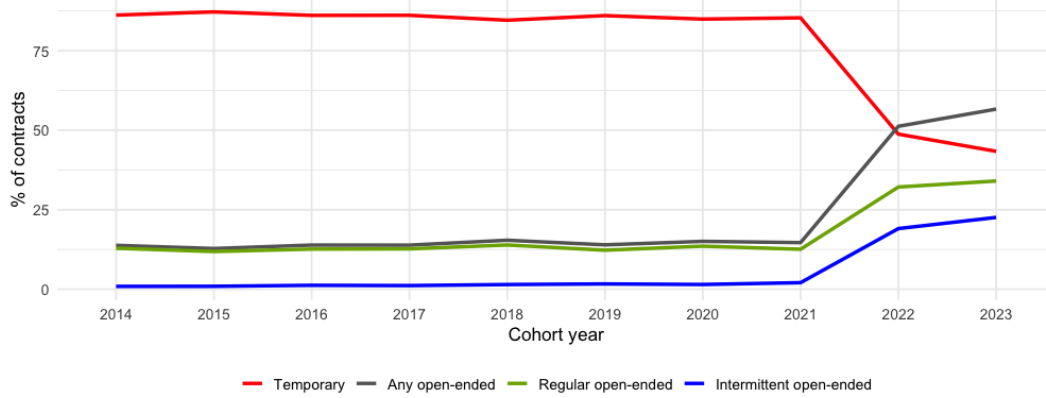
*Source:* Own calculations based on the Spanish Labor Force Survey (EPA).

*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.

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 the 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 excessive temporary job use displays substantial variation across provinces, remains stable over time, and is a powerful predictor of the proportion of entrants who obtained a permanent contract before the reform, as well as the subsequent improvement in this outcome in the post-reform period.

Our empirical strategy employs a difference-in-differences design. In the baseline specification, labor market entrants are divided into two groups according to the prevalence of temporary employment in their province of entry. Those entering the labor market in provinces with a positive value for our indicator — signaling excessive temporary contracts use 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 assess the reform’s effect on the contract type offered to entrants. Specifically, we estimate the reform’s impact on the relative probability that an entrant obtains an open-ended versus a temporary contract. In our baseline specification, we group all open-ended contracts together. However, we also provide separate estimates for the subset of regular open-ended

**Figure 2.** The distribution of contract types in entry jobs



positions, distinguishing between full-time and part-time contracts. In the next step, we use our baseline specification to estimate the effects of the reform on a range of labor market outcomes. The construction of a credible counterfactual is particularly important in this case because the reform was adopted in the aftermath of the pandemic, at a time of strong employment growth, which could confound our estimates. Our outcome variables include 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 earnings. We analyze these outcomes both during the first employment spell and during the twelve months following labor market entry.

Our empirical approach differs 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. By contrast, our strategy is based on the residual variation in the incidence of temporary employment across provinces after netting out differences in sectoral employment composition. A thorough analysis of the causes of this residual variation in the prevalence of temporary jobs is beyond our scope. However, in Figure A.1 we show that the temporary employment shares are positively correlated with the average unemployment rates at the province-level. Moreover, this correlation is much stronger than the correlation between the unemployment rates and our counterfactual temporary employment shares. This resembles one of the findings in Auciello-Estévez et al. (2023).<sup>2</sup> It suggests that the proportion of temporary jobs is higher in areas where workers have fewer outside options, granting firms more leeway to offer worse employment conditions. This interpretation is consistent with recent evidence showing that monopsony power is associated with lower wages and reduced access to permanent jobs (Bassanini et al., 2025).

Our results show that, while the reform led to a substantial overhaul of hiring patterns, it produced only modest improvements in labor market outcomes for new entrants. Our estimates indicate that

<sup>2</sup>The other channel they propose is a within-sector task specialization according to the relative abundance of college-educated workers in each of the provinces.

the increase in access to permanent contracts was 6.4 percentage points larger in the treatment group, eliminating 87% of the pre-reform gap. When we restrict attention to regular open-ended contracts, however, the differential effect falls to 4 percentage points, corresponding to 55.5% of the pre-reform gap, and only about one-quarter of this effect is attributable to the creation of full-time open-ended positions. Thus, the convergence in access to open-ended employment was driven primarily by the stronger growth of intermittent and part-time contracts in treated provinces. Because these contracts yield smaller improvements in subsequent labor market outcomes than regular full-time open-ended contracts, the pronounced shift in hiring patterns translated into more limited gains in employment stability and earnings. Specifically, we find that the reform reduced the pre-existing gaps in employment duration, the probability of a spell lasting more than six months, and total earnings during entrants' first employment spell by 40%, 32%, and 14%, respectively. These improvements remain statistically significant over a twelve-month horizon, although their magnitude declines: the proportional reduction in the gap falls to 27% for total employment duration and to 21.5% for the probability of experiencing a spell lasting more than six months. Finally, the effect on daily earnings is close to zero and statistically insignificant during the first spell. Over the first twelve months, by contrast, the effect on average daily earnings is positive and statistically significant, with a differential increase of 4.2%. This effect appears to reflect differences in working time rather than in hourly wages.

The second reason why the convergence in the access to open-ended jobs did not produce a corresponding convergence in labor market outcomes is due to a significant drop in the duration of open-ended contracts 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. 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 or after the probationary period. Indeed, among entrants, voluntary quits are the most frequent cause of separation, accounting for roughly 60 percent of separations 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 regular 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.

#### *Related literature*

We are not the first to study the impact of the 2021 reform on labor turnover, but the existing evidence is largely descriptive. Conde-Ruiz et al. (2025) analyze administrative data containing 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, they find little evidence of a significant change in labor turnover following the reform. Although informative, their analysis does not support a causal interpretation. Along similar lines, Calvo Gallego et al.

(2024) document an increase in the quit propensity of workers with permanent contracts after the reform. By contrast, focusing on new labor market entrants, we find no evidence that the reform increased the overall quit rate. Instead, our results suggest that it primarily shifted the timing of voluntary separations, with quits occurring earlier in the employment relationship rather than becoming more frequent overall.

Closest to our work is the early analysis of the IMF of the causal effects of the reform (International Monetary Fund (2024)). Using flow data from the Spanish Labor 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 differences in turnover rates, which are difficult to modify. In contrast, our identification strategy allows us to assess whether the reform reduced the scope for the excessive use of temporary contracts by firms operating in less competitive local labor markets.<sup>3</sup>

Finally, there is some evidence regarding the implications of previous EPL reforms for young people in Spain. García-Pérez et al. (2019) offer compelling evidence of the long-lasting negative effects of the 1984 reform, which is widely regarded as the root of labor market duality in Spain. The reform liberalized the use of temporary contracts in an attempt to stimulate job creation. Using a cohort regression discontinuity design, they find that the reform reduced the number of days worked and earnings of labor market entrants in their first ten years in the market by 4.9% and 9.8%, respectively. Moreover, even after 27 years on the market, yearly earnings are still 7.3% lower. Later reforms mostly intended to discourage the use of temporary contracts through marginal reductions in the gap in EPL between temporary and permanent contracts and/or hiring subsidies for permanent contracts. In some cases, this produced a temporary improvement in the outcomes of young workers. For instance, 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. 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. However, neither this reform nor any of the other counter-reforms undertaken before 2021 achieved a durable reduction in the incidence and persistence of temporary employment among Spanish youth.

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<sup>3</sup>Also using MCVL data, García-Pérez (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.

The last major EPL reform took place in 2012. The reform loosened the conditions for fair dismissals, lowered redundancy pay for unfair dismissal, suppressed the need to pay procedural wages and expanded the scope for internal adjustment mechanisms such as 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). It did, however, lay the groundwork for the 2021 reform which consolidated the redundancy regulation and the internal flexibility measures introduced in the 2012 reform. Given these margins of flexibility, the employers organizations agreed to a reform that imposed strict limits on the use of temporary contracts.

The remainder of the paper is organized as follows. Section 2 describes the changes introduced by the 2021 reform in the regulation of temporary work. Section 3 describes our data and the construction of the main outcome variables. Section 4 presents the methodology, followed by a discussion in Section 5 of the key identification challenges. Section 6 summarizes the main results. This is followed by an in-depth analysis of the drop in the duration of regular open-ended jobs in Section 7. Section 8 concludes.

## 2 Institutional details

The principal objective of the 2021 labor market reform was to make open-ended employment the default contractual arrangement. The most far-reaching measure is the abolition of the *contrato de obra y servicio*. This contract allowed firms to hire workers for the duration of a specific project or service with autonomy within the firm, for a maximum period of up to three years. In practice, however, it had become a broad channel for long-lasting temporary employment relationships, often in activities that were recurrent rather than genuinely temporary.

The second relevant measure is the creation of a new more restrictive temporary contract for production-related circumstances that replaced the existing ones. The contract may be used to cover unforeseeable increases in activity or mismatches between available staff and production needs. In this case, the contract may last up to six months within an eighteen-month period, extendable to twelve months by collective agreement. Alternatively, in the case of foreseeable activities, its use is subject to a limit of 90 days per calendar year which cannot be used uninterruptedly. These rules were designed to reinforce the principle of causality, restricting temporary hiring to clearly identified and genuinely temporary personnel needs.

The remaining measures tightened the regulation of training contracts — excluded from our analysis — and the rules on the chaining of temporary contracts.<sup>4</sup> In addition, the reform introduced a

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<sup>4</sup>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: since 2022, a worker becomes permanent after accumulating more than 18 months of employment within a 24-month period.

surcharge on social security contributions for very short contracts.<sup>5</sup> Both measures aim to limit firms’ ability to rely on repeated short-term hiring.

Unlike the single open-ended contract proposal put forward by experts like Samuel Bentolila in 2009, the 2021 reform did not reduce dismissal costs or otherwise make regular open-ended contracts more attractive to firms. Instead, it expanded the scope of the use of intermittent open-ended contracts (*contratos fijos-discontinuos*). Before the reform, this type of contract was restricted to seasonal activities. The reform broadened its use to cover intermittent activities, recurrent but discontinuous tasks and activities performed through subcontractors or temporary work agencies. Its purpose was to offer firms a flexible open-ended alternative for activities previously covered by the *contrato de obra y servicio*, as well as for recurrent intermittent activities lasting more than 90 days per year. Although the intermittent open-ended contract is formally an open-ended contract, it offers workers less employment security than a regular open-ended contract. Employers may suspend these contracts at no cost during periods in which there is no demand for the worker’s services, with the expectation that the employment relationship will resume when activity picks up. During these periods of inactivity, workers are entitled to unemployment benefits under the same conditions as other unemployed job seekers. Throughout the analysis, we assess the extent to which these institutional differences between regular and intermittent open-ended contracts influence our estimates.

### 3 Data

Our empirical analysis uses administrative records on labor market entrants extracted from the Continuous Sample of Working Lives (MCVL). The MCVL contains the employment records and social security contribution base wages for a random sample of 4 percent of all affiliates. For each individual’s employment spell, we observe the exact start and end dates, the contract type, the monthly social security base wage — our proxy for earnings — and detailed job characteristics, including the part-time coefficient, the firm’s location and sector of activity<sup>6</sup> and the contribution group. Although the contribution base is top-coded, it provides very accurate information on the earnings of labor market entrants. Indeed, only 0.85% of entrants reach the maximum contribution base during their first year in the labor market.

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 spells due to turnover, interruptions, or contractual changes. In order to obtain a clean and coherent definition of labor 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

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<sup>5</sup>Temporary contracts lasting less than 30 days are subject to a social security surcharge equivalent to roughly three times the daily contribution for common contingencies.

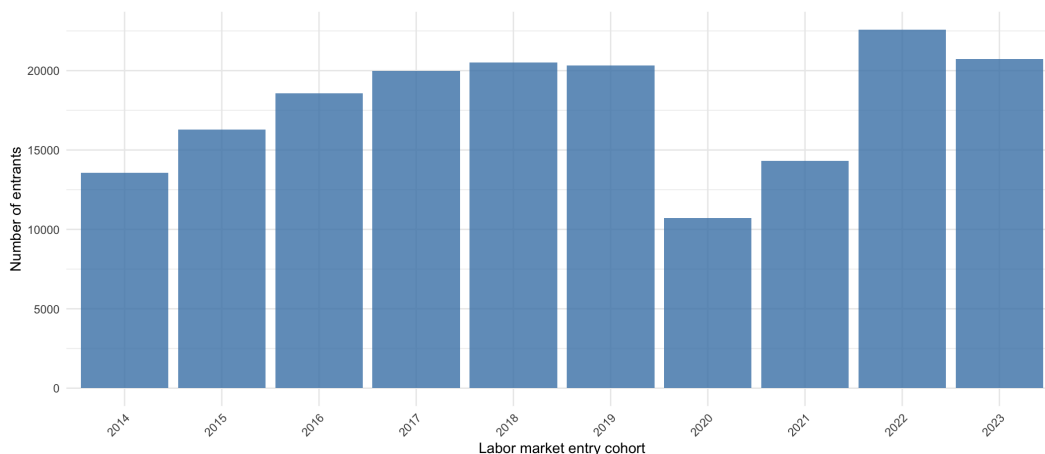
<sup>6</sup>Sector of activity is aggregated into 10 broad categories from detailed CNAE-2009 codes; see the Classification of economic sectors section in the Appendix for details.

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.

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 3, with a transitory decline in 2020 coinciding with the COVID-19 pandemic <sup>7</sup>, followed by a rebound in the years thereafter.

**Figure 3.** Annual entry cohort sizes



As we compare the outcomes of pre- and post-reform entry cohorts, it is important to ensure a high degree of similarity.

Table A.1 compares the characteristics of the labor market entrants in our sample when the pre- and post-reform cohorts are pooled. The reported Cohen statistics in column 4 show that the differences for most characteristics are small when we account for sample size.<sup>8</sup> The only notable exception concerns the educational attainments of entrants<sup>9</sup>, where we observe a decline in the

<sup>7</sup>The relative decline in cohort size during the pandemic years is similar across treatment and control provinces. Thus, the pre-reform prevalence of temporary jobs does not seem to have generated differential effects on the volume of new entrants.

<sup>8</sup>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.

<sup>9</sup>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

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 1 shows similar shifts in the provinces in the treatment and control groups, suggesting the presence of a common trend rather than a differential change in educational attainments that could be associated with the reform. Second, all our empirical specifications include controls for the level of educational attainment and in the robustness analysis we explicitly account for potential heterogeneity of the effects by level of education. Finally, given the concerns about 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.

Besides the shift in educational attainments, we also observe an increase in the relative share of entrants with a foreign nationality after the reform.<sup>10</sup> 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 entrants with Spanish nationality.

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 intermittent open-ended contracts, while we only observe minor changes in the mean days of work and earnings.

## 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.<sup>11</sup> 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

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advanced vocational training), and higher education to codes 44–98 (university and postgraduate degrees).

<sup>10</sup>We construct a binary indicator, *Nationality*, coded 1 if the entrant’s nationality is Spanish and 0 otherwise (foreign nationality).

<sup>11</sup>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.

each of its sectors were equal to the corresponding average share in all other provinces.<sup>12</sup> 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  $\tau_s^{-p}$  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:

$$\hat{\tau}_p = \sum_s \omega_{p,s} \tau_s^{-p} \quad (1)$$

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

$$\text{Excess}_p = \tau_p - \hat{\tau}_p. \quad (2)$$

**Figure 4.** Excess prevalence of temporary employment by province

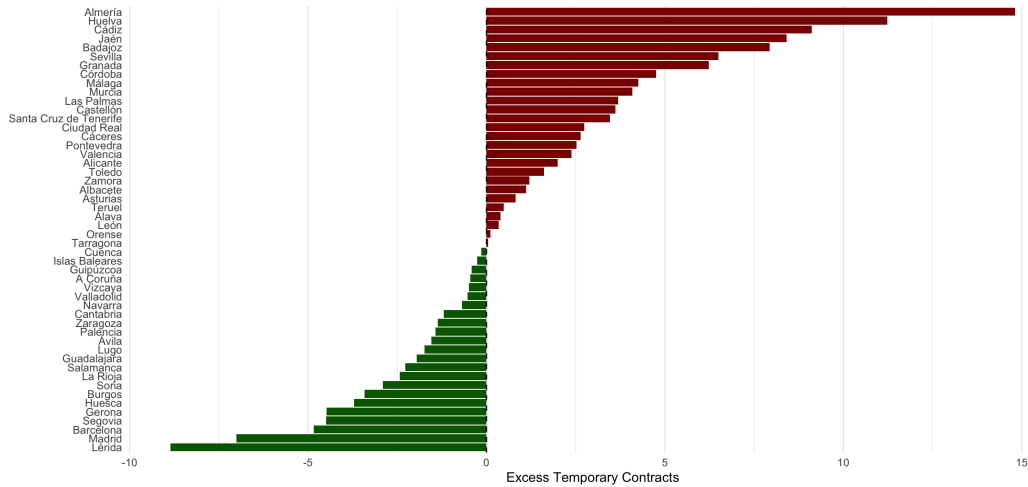
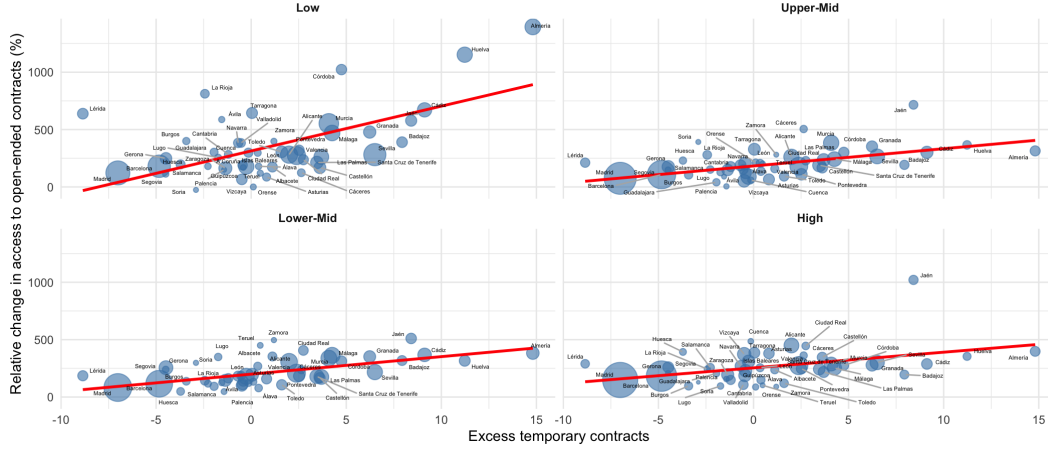


Figure 4 shows the resulting ranking of provinces. The red bars correspond to provinces with an excess prevalence of temporary employment in the pre-reform period. Young entrants in these provinces constitute our baseline treatment group. By contrast, entrants in provinces with a negative value of  $\text{Excess}_p$  constitute our baseline control group. Figure A.2 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  $\text{Excess}_p$  captures structural pre-reform

<sup>12</sup>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.

differences across provinces.

**Figure 5.** 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 solid weighted linear fits based on this provincial weight, giving greater relevance to provinces with larger sample sizes.

Additionally, the evidence reported in Figure 5 reveals a positive correlation between 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.<sup>13</sup> 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  $Excess_p$ .<sup>14</sup> The estimated equation is as follows:

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

<sup>13</sup>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.3.

<sup>14</sup>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:

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

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 an open-ended contract. The duration of the spell is measured in days and we also use a binary indicator equal to 1 for spells lasting six months or more. Finally, earnings are measured by the logarithm of the individual's Social Security contribution bases.<sup>15</sup> Our baseline outcomes refer to entrants' first employment spell, but we also report the results referring to the first twelve months after entry.<sup>16</sup>

The coefficient of interest is  $\beta_1$ . 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, nationality, educational attainment, gender, and month of entry, as well as macroeconomic controls such as annual provincial employment growth. The term  $\alpha_p$  denotes province fixed effects, and  $\lambda_t$  cohort-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 is measured in levels, the coefficient has a direct interpretation in units of time.

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<sup>15</sup>For spells that last longer than a year we consider the earnings during the first twelve months

<sup>16</sup>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 earnings 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.

Finally, for earnings accumulated during the first year, the outcome is expressed in logarithms. In this case, the coefficient on  $post_t \times treated_i$  can be interpreted as a semi-elasticity: to a first approximation, a value  $\beta_1$  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.<sup>17</sup>

## 5 Identification

In a DiD design with repeated cross-sections, the identification assumptions are more demanding than in panel settings (Heckman et al., 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 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 covariates except entrants’ nationality and 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. Besides true changes in educational attainments, such as the fall in dropout rates and the growth in vocational education and training, these shifts may also reflect delays in the updating of the data on educational attainments.<sup>18</sup> In any case, these common trends do not pose a threat to our identification strategy. Nevertheless, as mentioned earlier, we also provide estimates using the contribution group of entrants’ first job as a proxy for educational attainment. 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. Similarly, to deal with the lack of balance for the share of non-nationals, we also offer results for the subsample of Spanish nationals.

### Exogeneity

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<sup>17</sup>The exact percentage effect is given by  $100 \times (e^{\beta_1} - 1)$ .

<sup>18</sup>The educational attainment data is updated periodically. As a result, this variable tends to be more accurate for pre-reform cohorts, who completed their studies earlier, than for post-reform cohorts. Furthermore, delays in the updating of the educational attainments may also explain why the data for university graduates is more noisy. Due to the longer time span between the legal working age and their graduation age a relatively large proportion of university graduates may have a first contact with the social security administration before the completion of their university degree. Supporting this view, we observe the same type of shifts in educational attainments when the pre-reform cohort is divided into an early and a late sub-cohort.

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

| Variable                               | Category                         | Pre-reform (2014–2021) |          |           |        | Post-reform (2022–2023) |          |           |        |
|----------------------------------------|----------------------------------|------------------------|----------|-----------|--------|-------------------------|----------|-----------|--------|
|                                        |                                  | 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 |
| Nationality                            | Spanish                          | 77.913                 | 83.381   | 5.468     | 0.139  | 72.434                  | 79.517   | 7.082     | 0.166  |
|                                        | Foreign                          | 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 temporary contract     | 0.825                  | 0.898    | 0.072     | 0.211  | 0.458                   | 0.467    | 0.009     | 0.017  |
|                                        | Access to open-ended contract    | 0.175                  | 0.102    | -0.072    | -0.211 | 0.542                   | 0.533    | -0.009    | -0.017 |
|                                        | Intermittent open-ended          | 0.014                  | 0.013    | -0.001    | -0.005 | 0.197                   | 0.220    | 0.023     | 0.056  |
|                                        | Regular open-ended               | 0.161                  | 0.089    | -0.072    | -0.219 | 0.345                   | 0.313    | -0.032    | -0.067 |
|                                        | Full-time                        | 0.082                  | 0.043    | -0.039    | -0.163 | 0.155                   | 0.125    | -0.030    | -0.086 |
|                                        | Part-time                        | 0.079                  | 0.046    | -0.033    | -0.137 | 0.190                   | 0.188    | -0.002    | -0.005 |
|                                        | 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 |
|                                        | Earnings, first spell (€)        | 4169.384               | 3053.091 | -1116.293 | -0.215 | 4163.622                | 3250.359 | -913.263  | -0.179 |
|                                        | Total Earnings, first year (€)   | 6276.380               | 4711.169 | -1565.211 | -0.262 | 6749.120                | 5427.677 | -1321.442 | -0.220 |

*Notes:* The table reports proportions (%) and means for cohorts entering the 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. Regular open-ended, full-time and part-time are subcategories of “Access to regular open-ended contract,” split by weekly schedule.

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 covariates that could potentially be affected by the reform in ways that differ across provinces are the age and province of entry. Reassuringly, the descriptive evidence presented in Table 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. Furthermore, using our baseline empirical specification and the age of entry as outcome variable, we find no evidence of a differential effect of the reform on this margin (see Table A.3).<sup>19</sup> Changes in the mobility pattern of entrants would pose a separate concern if the reform altered the mobility of entrants between the provinces in the treatment and control group, since entrants' home provinces are based on the location of their first job. In addition, the degree of geographic mobility may itself be an outcome of interest. To address both issues, we also present results for the case in which treatment assignment is based on the province of birth, as well as for a subsample of stayers 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. However, the reform did not come into full force until April 2022. During the transition period between January and March 2022 firms were granted temporary exemptions to allow them to adapt to the new rules for temporary jobs. To minimize 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. Finally, in one of our robustness tests we excluded the individuals who signed their first contract during the transition period between January and March 2022.

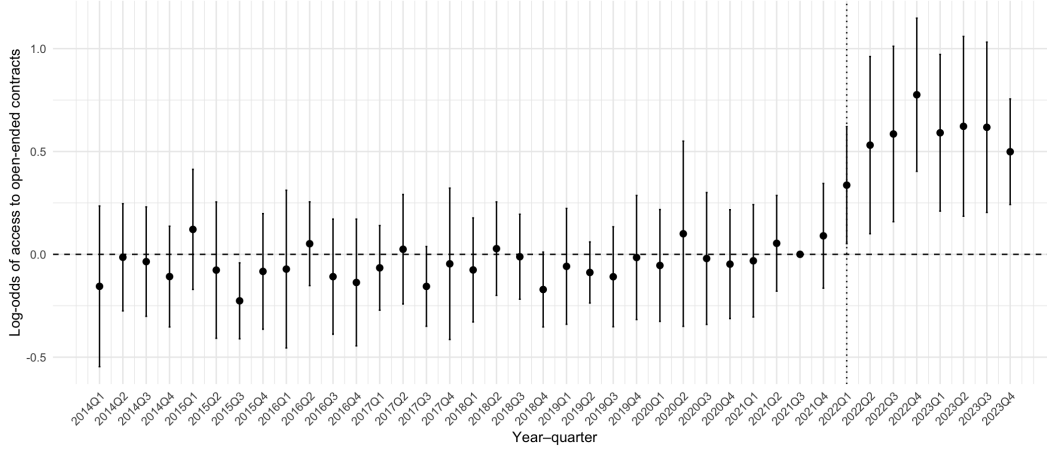
### **Parallel trends**

To rule out a violation of 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. 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.

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<sup>19</sup>This result does not rule out the possibility that entrants in both the treatment and control groups found it harder to access employment after the reform. We leave this issue for future work.

**Figure 6.** 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). Vertical bars represent 95% confidence intervals.

Figure 6 presents the results when we consider access to any open-ended contract (see Figure A.4 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. This holds even during the pandemic period, which further supports our decision to retain 2020 in the estimation sample.<sup>20</sup> 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 it confirms our premise that the reform had a stronger bite in the treated provinces.

We complement this evidence with event studies at an annual frequency for labor market outcomes in the first employment spell (Figures A.5, A.6, and A.7). As before, the estimated coefficients for the pre-reform period are close to zero and statistically insignificant in the five years prior to the reform, confirming parallel pre-trends between treated and control provinces and ruling out anticipation effects.

<sup>20</sup>Notice that our results do not rule out negative effects of the pandemic that were common to entrants in both the treatment and control groups. Such common shocks are absorbed by the cohort-year fixed effects. The absence of differential effects may seem surprising given the heterogeneous impact of the pandemic across provinces, sectors and contract types. However, our analysis focuses on entrants with no prior work experience. Due to the high degree of uncertainty during the pandemic it was highly unlikely for entrants to obtain an open-ended contract in their first employment spell.

## 6 Results

This section presents our main results.

### 6.1 Access to open-ended contracts

We begin with an in-depth analysis of the differential effects of the reform on the access to open-ended contracts. Our event study design considered access to all types of open-ended contract. The aim of this section is to assess the contribution of each specific type of open-ended contract to the overall effects reported above. To this end, we estimated our baseline specification for different subsets of open-ended contracts. In addition, the DiD specification provides a more precisely estimated average effect for the post-reform period than the quarterly event-study coefficients, whose primary role is to validate the parallel-trends assumption underlying our empirical strategy. Table 2 shows the results.

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

|                              | Any                 | Regular             |                     |                     | Intermittent     |
|------------------------------|---------------------|---------------------|---------------------|---------------------|------------------|
|                              | (1)                 | All<br>(2)          | Full-time<br>(3)    | Part-time<br>(4)    | (5)              |
| Post × Treated               | 0.636***<br>(0.172) | 0.576***<br>(0.090) | 0.467***<br>(0.098) | 0.601***<br>(0.090) | 0.160<br>(0.241) |
| Average marginal effect (pp) | 6.373               | 4.010               | 0.923               | 3.093               | 2.357            |
| Individual controls          | ✓                   | ✓                   | ✓                   | ✓                   | ✓                |
| Nationality                  | ✓                   | ✓                   | ✓                   | ✓                   | ✓                |
| Employment trends            | ✓                   | ✓                   | ✓                   | ✓                   | ✓                |
| Province (FE)                | ✓                   | ✓                   | ✓                   | ✓                   | ✓                |
| Cohort year (FE)             | ✓                   | ✓                   | ✓                   | ✓                   | ✓                |
| N                            | 177,541             | 177,541             | 177,541             | 177,541             | 177,541          |
| Adj. Pseudo- $R^2$           | 0.174               | 0.116               | 0.150               | 0.075               | 0.274            |

*Notes:* The table reports Difference-in-Differences logit estimates. The treated group is defined according to the pre-reform provincial excess of temporary employment, while the post-reform period corresponds to 2022–2023. Column (1) reports access to any open-ended contract, column (2) access to a regular open-ended contract, column (3) access to a regular open-ended contract at full-time, column (4) access to a regular open-ended contract at part-time, and column (5) access to an intermittent open-ended contract. The row “Average marginal effect” reports the average marginal effect, expressed in percentage points. All specifications include individual controls, nationality controls, employment trends, province fixed effects, and cohort-year fixed effects. Robust standard errors clustered at the provincial level are reported in parentheses. Heterogeneous effects by level of education are reported separately in Table A.8. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

Column (1) reports the results when access to any open-ended contract – including both regular and intermittent open-ended contracts – is used as the outcome variable. The coefficient on the interaction term *Post  $\times$  Treated* captures the difference in the change in the log odds ratio between the treatment and control groups, i.e. the logarithm of the ratio between the probability of

accessing an open-ended contract and the complementary probability of not accessing it, before and after the reform. The positive coefficient implies a stronger improvement for entrants in the treatment group, and it is statistically significant at the 1 percent level; the effect is strongest in this specification, which pools together regular and intermittent open-ended contracts. As the logit coefficient cannot be interpreted directly in terms of probabilities, we also report the average marginal effect (AME) in Table 2. This indicates how much the predicted probability of accessing an open-ended contract changes when the interaction  $Post \times Treated$  varies, holding other factors constant, thereby measuring directly the percentage point difference in the improvement in access for the treatment and control group.<sup>21</sup> According to our estimates, this difference amounts to 6.4 pp. The magnitude of this effect becomes clear when we compare it to the pre-existing gap in access for the individuals in the treatment and control groups. As reported in Table 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).

Columns (2)–(4) restrict the outcome variable to regular open-ended contracts, excluding intermittent open-ended contracts. Column (2) considers regular open-ended contracts as a whole; the coefficient is again positive and significant at the 1 percent level, with an estimated AME of 4.0 pp. This is smaller than the pre-reform gap of 7.2 pp between control and treated provinces (16.1% versus 8.9%, respectively), so that the reform closed approximately 55.5% of this gap (4/7.2). Columns (3) and (4) further break down regular open-ended contracts into full-time and part-time positions, defined as contracts with a part-time coefficient strictly below one.<sup>22</sup> The results indicate that the differential effect on regular open-ended contracts is primarily due to part-time contracts: the estimated marginal effect is 3.1 pp for part-time contracts, compared to 0.9 pp for full-time contracts, so that the growth in full-time regular open-ended contracts accounts for less than one-sixth of the differential effect reported in column (1). This asymmetry also shows up when comparing these estimates to the corresponding pre-reform gaps: the reform closes approximately 24% of the pre-existing gap of 3.9 pp in full-time contracts, whereas for part-time contracts it produces a near complete convergence (93%) given a pre-reform gap of 3.3 pp.<sup>23</sup> Moreover, it is worthwhile to mention that we obtain virtually the same results for columns (1)–(4) when we use a linear probability model instead of our baseline logit specification.<sup>24</sup>

Finally, column (5) reports the estimate when access to an intermittent open-ended contract is used as the outcome variable. Neither this coefficient nor the difference between the point estimates for

<sup>21</sup>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.

<sup>22</sup>We are grateful to one of the anonymous referees for suggesting this exercise.

<sup>23</sup>In Table 2, part-time positions are defined as contracts with a part-time coefficient below one. As a robustness check, we also estimated the results using a stricter definition that defines part-time jobs as jobs with a part-time coefficient strictly below 75 percent. Under this stricter definition, the differential effect is even larger: the AME rises to 3.5 pp, compared with 3.1 pp in column (4).

<sup>24</sup>See Table A.4 in the Appendix.

access to any open-ended contract (column 1) and access to a regular open-ended contract (column 2) is statistically significant. Hence, the results for the access to intermittent open-ended contracts should be interpreted with caution. Nonetheless, the AME of 2.4 pp suggests that intermittent open-ended contracts could account for roughly one-third of the differential effect of the reform on access to open-ended jobs overall.

Taken together, these results highlight the importance to take into account changes in the mix of open-ended contracts. Part-time and intermittent contracts generate smaller improvements in labor market outcomes than full-time contracts. Furthermore, employment on these contracts may be involuntary in the sense that the individuals would prefer a regular full-time position.

## 6.2 Labor market outcomes in the first spell

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

|                              | (1)<br>Days of employment | (2)<br>Spell $\geq 6$ months | (3)<br>Earnings     |
|------------------------------|---------------------------|------------------------------|---------------------|
| Post $\times$ Treated        | 8.192***<br>(2.996)       | 0.113*<br>(0.059)            | 0.142***<br>(0.038) |
| Average marginal effect (pp) | –                         | 1.952                        | –                   |
| Individual controls          | ✓                         | ✓                            | ✓                   |
| Nationality                  | ✓                         | ✓                            | ✓                   |
| Employment trends            | ✓                         | ✓                            | ✓                   |
| Province FE                  | ✓                         | ✓                            | ✓                   |
| Cohort year FE               | ✓                         | ✓                            | ✓                   |
| N                            | 163,238                   | 163,238                      | 161,904             |
| Adj. $R^2$ / Pseudo- $R^2$   | 0.007                     | 0.089                        | 0.037               |

*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. Column (1) presents OLS estimates for the duration, in days, of the first employment spell after entering the labor market. Column (2) reports a logit model for the probability that the first spell lasts at least six months, with coefficients in log-odds. Column (3) uses as dependent variable the logarithm of earnings accumulated during the first year under the initial contract. 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, nationality controls, employment trends, and fixed effects for province and cohort year. Robust standard errors clustered at the provincial level are shown in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ . Heterogeneous effects by level of education are reported separately in Table A.8.

We now proceed to analyze the differential effects of the reform on entrants’ labor market outcomes in their first spell. Table 3 reports the results for the duration of the first spell, the probability of a spell longer than six months and total earnings. The maximum period considered for duration and earnings in the first spell is twelve months.

All three differential effects are positive and statistically significant, but rather modest in size. 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, while the pre-reform gap in this outcome variable amounted to 20.6 days, as shown in Table 1<sup>25</sup>. Hence, the reform reduced the gap in duration by around 40% (8.2/20.6). Similarly, the 2 pp differential effect on the probability a duration above six months is equivalent to a 32% reduction in the pre-reform gap of 6.1 pp in this outcome variable. Finally, 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, the effects on daily and hourly earnings are close to zero and statistically insignificant, and we find no evidence of any heterogeneous effect by level of education.<sup>26</sup> The null effect on hourly and daily earnings suggests that the improvement in total earnings is driven by the increase in duration.

### 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 step towards analyzing 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.

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 by approximately 14%. Finally, unlike the null effect found for the first spell, over the first twelve months we obtain a significant differential effect on daily earnings of 4.2% (See Table A.5). The results for earnings are very similar if we exclude employment spells that last less than 30 days.

### 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, nationality, 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.6, are very similar to our baseline results,

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<sup>25</sup>The mean pre-reform durations of the first spell for the treatment and control group were, respectively, 88.3 and 108.9 days

<sup>26</sup>See Table A.5, columns (1)–(2), for details.

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

|                              | (1)<br>Days of employment | (2)<br>Spell $\geq$ 6 months | (3)<br>Earnings     |
|------------------------------|---------------------------|------------------------------|---------------------|
| Post $\times$ Treated        | 7.116***<br>(2.315)       | 0.095**<br>(0.048)           | 0.140***<br>(0.026) |
| Average marginal effect (pp) | -                         | 1.764                        | -                   |
| Individual controls          | ✓                         | ✓                            | ✓                   |
| Nationality                  | ✓                         | ✓                            | ✓                   |
| Employment trends            | ✓                         | ✓                            | ✓                   |
| Province (FE)                | ✓                         | ✓                            | ✓                   |
| Cohort year (FE)             | ✓                         | ✓                            | ✓                   |
| N                            | 163,238                   | 163,238                      | 161,904             |
| Adj. $R^2$ / Pseudo- $R^2$   | 0.014                     | 0.086                        | 0.058               |

*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. Column (1) presents OLS estimates for the total number of employment days accumulated during the first year after entering the labor market. Column (2) reports a logit model for the probability of experiencing at least one employment spell lasting six months or more within that first year, with coefficients in log-odds. Column (3) uses as the dependent variable the logarithm of earnings 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, nationality controls, employment trends, and fixed effects for province and cohort year. Robust standard errors clustered at the provincial level are shown in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

both in terms of the magnitude and the statistical significance of the coefficient 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, reported in Table A.7. This creates more homogeneous samples and allows for differences in the parameter estimates across education levels. The resulting estimates are very similar to those implied by our baseline pooled specification with triple interactions, reported in Table A.8. For the reference group of high-educated entrants, the *Post  $\times$  Treated* coefficient in Table A.8 can be compared directly with the corresponding coefficient in Table A.7. For the remaining education categories, the implied baseline effect is given by the sum of this coefficient and the corresponding triple-interaction term, both reported in Table A.8, which can then be compared with the subsample estimate in Table A.7.

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.9 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. 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  $\text{Excess}_p$ , we now restrict the sample to entrants in the provinces that belong to the top and the bottom tercile of  $\text{Excess}_p$ . In this case, we obtain slightly larger effects than in our baseline as shown in Table A.10. This pattern is consistent with the idea that the reform generated stronger effects precisely in those provinces with the largest margins for improvement. In our baseline specification, an individual's treatment status is determined by the location of their first employer. In our next robustness test, we analyze how our results change when entrants are assigned to their province of birth. While the province of birth is completely exogenous, it disregards the possibility that individuals moved to another province prior to the labor market entry. Furthermore, the variable province of birth is only available for Spanish nationals<sup>27</sup>. Once again, we obtain similar results as in our baseline, but a comparison of Table A.12 with Tables 2 and 3, shows that the effects are somewhat smaller and less significant. Since the labor market reform may have changed entrants' propensity for geographic mobility, we have also estimated our baseline specification on the subset of stayers who entered the labor market in their province of birth. For this self-selected sample, the results are essentially the same as in our baseline, except for the smaller difference between the estimates for the access to any open-ended and regular open-ended contracts (see Table A.13). Finally, for the sake of completeness, we have also estimated the differential effect of the reform on the probability of being a stayer. According to our estimates, this probability dropped by 1.9 pp after the reform for the individuals in the treatment group relative to the individuals in the control group. Hence, the reform seems to have produced a small increase in the geographic mobility of young workers.

The reform was approved in December 2021 but did not become fully effective until April 2022. During the intervening transition period, only some of the new restrictions on temporary contracts were legally binding. Table A.14 shows that our estimates are virtually unchanged when we exclude individuals who entered employment for the first time between January and March 2022. This finding suggests that firms had already adjusted their hiring practices during the transition period to the full set of restrictions that took effect in April 2022.

Finally, our last robustness test addresses the concern that our definition of labor market entry

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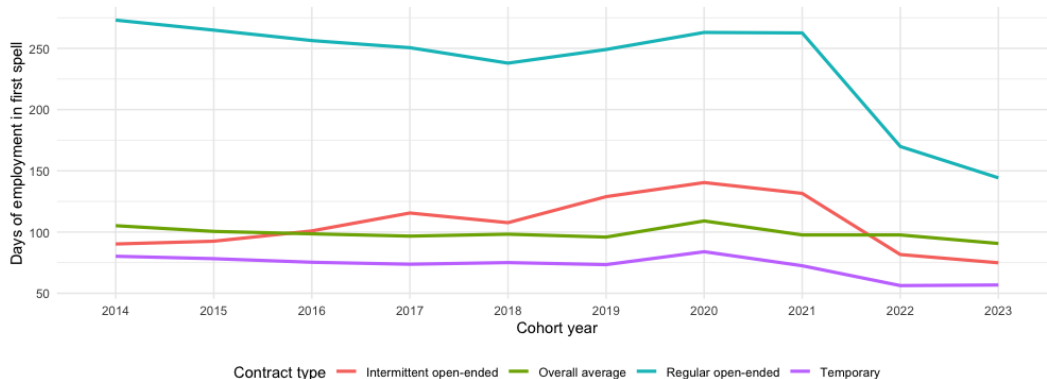
<sup>27</sup>We have also estimated our baseline specification on the subsample of entrants with Spanish nationality, retaining our original treatment definition based on the province of the first employer; see Table A.11.

may capture temporary jobs of individuals who are still enrolled in education. Our baseline already excludes training contracts. We further consider two additional sample restrictions. Table A.15 reports the results when we also exclude individuals whose first job started in June, July, or August, when seasonal student employment is most common, while Table A.16 reports the results when we restrict the sample to entrants whose first employment spell lasted more than 30 days. Neither of these restrictions leads to any noteworthy changes in our results.

## 7 Duration analysis

The evidence presented so far shows that the convergence in the access to open-ended contracts was much stronger than the convergence in labor market outcomes. As shown in Section 6.1, one part of the explanation is that the convergence in the access to open-ended contracts was mainly achieved through the creation of part-time and intermittent open-ended contracts. These contracts lead to smaller improvements in labor market outcomes than full-time regular open-ended contracts. Another relevant factor is the significant post-reform reduction in the duration of permanent jobs, shown in the figure below.

**Figure 7.** Duration of initial employment spells in the first year by contract type



Following the reform, the mean duration of new regular open-ended contracts for our sample of entrants decreased by over 40% (-100 days), while the spell length of intermittent contracts was halved (-60 days) compared to the pre-reform period when these contracts could only be used for seasonal activities. Furthermore, the drop in duration of regular open-ended contracts is more pronounced in the treatment group. To quantify this difference, we grouped entrants according to their initial contract type and re-estimated our baseline specification, using the duration of the first spell as the outcome variable. The results show that the difference in the drop in mean regular open-ended contract duration between the treatment and control groups amounts to 34 days (see Table A.17). This additional reduction partially offsets the improvement in the access to open-ended contracts due to the reform. Indeed, the mean duration of entrants' first employment spell, represented by the green line in Figure 7, is virtually unchanged after the reform.

Figures A.8 and A.9 offer additional descriptive evidence for the causes and timing of separations. The only relevant change in the causes of separations is the substitution of ‘end of contract’ by ‘transition to inactivity’, due to the transformation of temporary contracts into intermittent open-ended contracts. In the case of regular open-ended contracts, voluntary quits are the main source of separation, accounting for about 60% of exits, before and after the reform. Furthermore, we find no evidence of a significant increase in the share of entrants who do not pass their probation period. What does change is the timing of the separations. In the case of regular open-ended contracts both quits and dismissals take considerably less time than before the reform, and the workers with intermittent open-ended contracts transition to inactivity more quickly than before the reform (see Figure A.9).

Figure A.10 illustrates the net effect of the above changes on the empirical distribution of the length of entrants’ first spell and the cumulative days of work during the first year. This evidence complements the data on mean outcomes reported in Figure 7 and in Table 1. For entrants’ first employment spells, the changes are concentrated in the lower tail of the distribution. On the contrary, in the case of the cumulative days of work during the first twelve months we observe small differences in both tails of the distributions.

To formalize the analysis, we estimated a discrete-time duration model à la Jenkins (1995), using data on first employment spells. Our analysis is formally equivalent to a regression discontinuity design, as we allow the hazard rates to change at the time of the reform. 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 the first column of Table 5 indicate that, in aggregate terms, the reform did not significantly modify exit rates from employment of the entrants in the treatment group compared to those in the control group. However, this aggregate result conceals relevant changes in the hazard rates by contract type. In particular, in line with before, we find that the exit rate from employment of entrants with a regular open-ended contract experienced a stronger increase in the treated provinces. Figure A.11 illustrates the corresponding survival probabilities in employment for an individual in the treated provinces.<sup>28</sup> 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

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<sup>28</sup>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.

**Table 5.** Differential impact of the reform on the exit rate from an individual’s first job

|                                                        | (1)              | (2)                 |
|--------------------------------------------------------|------------------|---------------------|
| Post $\times$ Treated                                  | 0.010<br>(0.028) | 0.040<br>(0.031)    |
| Post $\times$ Treated $\times$ Intermittent open-ended |                  | 0.092<br>(0.099)    |
| Post $\times$ Treated $\times$ Regular open-ended      |                  | 0.249***<br>(0.065) |
| Individual controls                                    | ✓                | ✓                   |
| Nationality                                            | ✓                | ✓                   |
| 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 Temporary.

the survival probability of temporary contracts.

Our findings indicate that part of the pre-reform turnover associated with temporary contracts has shifted to the permanent segment of the labor market. This increase in turnover should, however, not necessarily be interpreted as entirely inefficient. Many of the jobs that are converted into open-ended positions after the reform may be inherently less stable than the permanent jobs that existed before the reform. In addition, average match quality among permanent employment relationships may have declined, as firms can no longer be as selective when open-ended contracts have become the default form of hiring. Finally, some workers who expect to remain with their employer only temporarily may even have preferred a fixed-term contract, since this could preserve their entitlement to unemployment benefits upon separation. Determining the degree to which the increased turnover on permanent jobs reflects privately efficient decisions rather than inertia in the rotation decisions of firms is an important task for future research.

## 8 Conclusions

This paper examines the impact of the 2021 labor market reform on the employment outcomes of young workers in Spain. Our empirical strategy exploits persistent within-sector differences across provinces in the pre-reform prevalence of temporary employment, which was strongly correlated with unemployment. One possible explanation for this empirical regularity is that greater labor

market slack weakens the bargaining position of job seekers, allowing firms to offer less attractive contracts. Regardless of the precise mechanism underlying this correlation, the reform’s restrictions on temporary hiring should have been more binding for firms in provinces with excess temporary employment. We exploit this variation to assess whether, and to what extent, the reform improved employment stability among labor market entrants.

Our results show that the reform substantially improved access to open-ended contracts among labor market entrants, particularly in provinces where temporary employment had been more prevalent before the reform. Consequently, the pre-existing differences across provinces in access to open-ended jobs almost disappeared. However, this convergence did not translate into commensurate improvements in entrants’ subsequent labor market outcomes. While the reform eliminated nearly 90% of the gap in the probability of starting the first job on an open-ended contract, it reduced the corresponding gaps in job duration and earnings by only 40% and 14%, respectively. Our analysis offers two explanations for this apparent discrepancy. First, regular full-time contracts accounted for only a small share of the convergence in access to open-ended jobs. Most of the convergence instead resulted from the greater use of part-time and intermittent contracts by firms in treated provinces with a high pre-reform prevalence of temporary employment. Second, the mean duration of regular open-ended contracts fell after the reform. Their average duration declined by 100 days, with larger reductions in provinces where temporary employment had been more prevalent before the reform. Overall, the evidence suggests that legal reforms aimed at curbing temporary employment, while necessary, may not be sufficient to deliver substantial improvements in employment stability. On the one hand, our results appear to reflect firms’ efforts to preserve flexibility after the reform by relying on part-time and intermittent contracts rather than regular full-time contracts. On the other hand, because mandatory redundancy payments are low at the beginning of an employment relationship, some firms may be reluctant to reduce worker turnover. These conclusions should nevertheless be interpreted with two caveats in mind. First, our estimates capture only the short-run differential impact of the reform on entrants in the treatment and control groups. They therefore do not capture general-equilibrium effects that affect entrants in both groups, including possible changes in school leavers’ job-finding rates. Second, the effects may become larger over time if improved access to open-ended contracts at the start of entrants’ careers leads to more stable employment trajectories later in their working lives. We leave the analysis of these longer-term and general-equilibrium effects to future research.

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# Appendix

## Classification of economic sectors

The MCVL records the firm’s sector of activity at the group level (3-digit codes) of the 2009 revision of the Spanish Classification of Economic Activities (CNAE-2009). This revision has been used for administrative purposes, including Social Security registrations, since 2009, and therefore applies uniformly to our entire sample of entrants (2014–2023). We first group these detailed 3-digit codes into a set of intermediate categories, broadly reflecting each sector’s technological and knowledge intensity as well as its institutional characteristics (e.g. separating temporary work agencies and public administration from the rest of market services). We then combine several of these intermediate categories into the 10 broad sectors: in particular, extractive activities and energy, high- and medium-technology manufacturing, and low-technology manufacturing are grouped into a single “Industry” category; low-skill services and real estate activities are grouped together; and knowledge-intensive services are grouped together with finance. The resulting categories are used as sector fixed effects specifications.

Classification of economic sectors

| Sector                                           | N       | %      |
|--------------------------------------------------|---------|--------|
| Accommodation and food service activities        | 42,446  | 23.91  |
| Knowledge-intensive services and finance         | 37,103  | 20.90  |
| Wholesale and retail trade                       | 27,904  | 15.72  |
| Low-skill services and real estate activities    | 16,953  | 9.55   |
| Temporary work agencies                          | 13,570  | 7.64   |
| Education and health                             | 13,383  | 7.54   |
| Agriculture, livestock, forestry, and fishing    | 11,604  | 6.54   |
| Extractive activities, energy, and manufacturing | 9,018   | 5.08   |
| Construction                                     | 5,471   | 3.08   |
| Public administration                            | 89      | 0.05   |
| Total                                            | 177,541 | 100.00 |

*Notes:* The table reports the 10 broad sector categories used as sector fixed effects throughout the paper, together with the number and share of entrants in our sample belonging to each category. These categories are constructed by aggregating detailed CNAE-2009 group-level (3-digit) activity codes into intermediate categories based on technological and knowledge intensity, several of which are in turn combined into the final categories listed above.

## Tables

**Table A.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  |
| Nationality    | Spanish                          | 80.402   | 75.617   | -4.785   | 0.000   | -0.116 |
|                | Foreign                          | 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  |
|                | Regular 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      | 0.216    | 0.193    | -0.023   | 0.000   | -0.057 |
|                | Any spell $\geq 6$ months (year) | 0.330    | 0.332    | 0.003    | 0.326   | 0.005  |
|                | Earnings, first spell            | 3660.380 | 3753.344 | 92.964   | 0.001   | 0.018  |
|                | Total earnings, 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).

**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 $\times$ Treated              | 0.556***<br>(0.081)            | 0.585***<br>(0.087) | 0.568***<br>(0.089) | 0.576***<br>(0.090) | 0.596***<br>(0.093) | 0.567***<br>(0.077) | 0.520***<br>(0.074) | –                    | –                   | –                   | –                   | –                   | –                   | –                   |
| Post $\times$ Continuous treatment | –                              | –                   | –                   | –                   | –                   | –                   | –                   | 0.056***<br>(0.005)  | 0.058***<br>(0.005) | 0.058***<br>(0.005) | 0.059***<br>(0.005) | 0.060***<br>(0.005) | 0.057***<br>(0.004) | 0.052***<br>(0.005) |
| Individual controls                | ✓                              | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   | ✓                    | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   |
| Education level                    | ✓                              | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   | ✓                    | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   | ✓                   |
| Nationality                        | ✓                              | –                   | ✓                   | ✓                   | –                   | ✓                   | ✓                   | ✓                    | –                   | ✓                   | ✓                   | –                   | ✓                   | ✓                   |
| 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, nationality, 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 on the age of entry

| (1)                   |                  |
|-----------------------|------------------|
| Age at entry (years)  |                  |
| Post $\times$ Treated | 0.007<br>(0.048) |
| N                     | 177,541          |
| Adj. $R^2$            | 0.057            |

*Notes:* The table reports the Difference-in-Differences (DiD) coefficient from our baseline specification, using the age at entry (in years) as the dependent variable. *Treated* is defined based on the pre-reform provincial excess of temporary employment, and the post-reform period corresponds to 2022–2023. Standard errors, clustered at the provincial level, are shown in parentheses.

**Table A.4.** Differential effect of the labor reform on access to open-ended contracts in the first job: linear probability model (LPM)

|                       | <b>Any</b>         | <b>Regular</b>      |                  |                     | <b>Intermittent</b> |
|-----------------------|--------------------|---------------------|------------------|---------------------|---------------------|
|                       |                    | <b>All</b>          | <b>Full-time</b> | <b>Part-time</b>    |                     |
|                       | (1)                | (2)                 | (3)              | (4)                 | (5)                 |
| Post $\times$ Treated | 0.068**<br>(0.029) | 0.042***<br>(0.013) | 0.010<br>(0.007) | 0.032***<br>(0.010) | 0.026<br>(0.032)    |
| Individual controls   | ✓                  | ✓                   | ✓                | ✓                   | ✓                   |
| Nationality           | ✓                  | ✓                   | ✓                | ✓                   | ✓                   |
| Employment trends     | ✓                  | ✓                   | ✓                | ✓                   | ✓                   |
| Province (FE)         | ✓                  | ✓                   | ✓                | ✓                   | ✓                   |
| Cohort year (FE)      | ✓                  | ✓                   | ✓                | ✓                   | ✓                   |
| N                     | 177,541            | 177,541             | 177,541          | 177,541             | 177,541             |
| Adj. $R^2$            | 0.195              | 0.128               | 0.098            | 0.049               | 0.147               |

*Notes:* The table reports linear probability model (LPM) estimates corresponding to the logit specifications in Table 2. Coefficients reflect percentage-point differences between the treatment and control group and are directly comparable to the average marginal effects derived from the logit specifications. Column (1) reports access to any open-ended contract, column (2) access to a regular open-ended contract, column (3) access to a regular open-ended contract at full-time, column (4) access to a regular open-ended contract at part-time, and column (5) access to an intermittent open-ended contract. All specifications include individual controls, nationality controls, employment trends, province fixed effects, and cohort-year fixed effects. Robust standard errors clustered at the provincial level are reported in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

**Table A.5.** Differential effect of the labor reform on wages

|                            | First Spell         |                      | First year          |                      | First Spell (>30 days) |                      | First year (>30 days) |                      |
|----------------------------|---------------------|----------------------|---------------------|----------------------|------------------------|----------------------|-----------------------|----------------------|
|                            | Earnings/day<br>(1) | Earnings/hour<br>(2) | Earnings/day<br>(3) | Earnings/hour<br>(4) | Earnings/day<br>(5)    | Earnings/hour<br>(6) | Earnings/day<br>(7)   | Earnings/hour<br>(8) |
| Post $\times$ Treated      | 0.024<br>(0.022)    | -0.011<br>(0.024)    | 0.042***<br>(0.007) | 0.007<br>(0.010)     | -0.020<br>(0.019)      | 0.012<br>(0.012)     | 0.044***<br>(0.008)   | 0.013<br>(0.012)     |
| Individual controls        | ✓                   | ✓                    | ✓                   | ✓                    | ✓                      | ✓                    | ✓                     | ✓                    |
| Nationality                | ✓                   | ✓                    | ✓                   | ✓                    | ✓                      | ✓                    | ✓                     | ✓                    |
| Employment trends          | ✓                   | ✓                    | ✓                   | ✓                    | ✓                      | ✓                    | ✓                     | ✓                    |
| Province (FE)              | ✓                   | ✓                    | ✓                   | ✓                    | ✓                      | ✓                    | ✓                     | ✓                    |
| Cohort year (FE)           | ✓                   | ✓                    | ✓                   | ✓                    | ✓                      | ✓                    | ✓                     | ✓                    |
| N                          | 161,904             | 161,904              | 161,904             | 161,904              | 99,749                 | 99,749               | 99,749                | 99,749               |
| Adj. $R^2$ / Pseudo- $R^2$ | 0.040               | 0.015                | 0.150               | 0.089                | 0.041                  | 0.140                | 0.219                 | 0.107                |

*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. Column (1) uses as the dependent variable the logarithm of the daily wage in the first spell. Column (2) uses the logarithm of the hourly wage in the first spell. Column (3) uses the logarithm of the daily wage accumulated during the first twelve months. Column (4) uses the logarithm of the hourly wage during the first twelve months. Columns (5)–(6) replicate columns (1)–(2), and columns (7)–(8) replicate columns (3)–(4), on the sample restricted to first spells lasting more than 30 days. All models include individual controls, employment trends, and fixed effects for province and cohort year. Robust standard errors clustered at the provincial level are reported in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

**Table A.6.** Differential effect of the labor reform under coarsened exact matching

|                            | (1)<br>Access to open-ended | (2)<br>Access to regular open-ended | (3)<br>Employment days | (4)<br>Spell $\geq 6$ months | (5)<br>Earnings     |
|----------------------------|-----------------------------|-------------------------------------|------------------------|------------------------------|---------------------|
| Post $\times$ Treated      | 0.613***<br>(0.085)         | 0.563***<br>(0.093)                 | 7.957**<br>(3.201)     | 0.110*<br>(0.063)            | 0.165***<br>(0.045) |
| Individual controls        | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Nationality                | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Employment trends          | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Province (FE)              | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Cohort year (FE)           | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| CEM                        | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| N                          | 177,500                     | 177,500                             | 163,190                | 163,190                      | 161,857             |
| Adj. $R^2$ / Pseudo- $R^2$ | 0.154                       | 0.091                               | 0.006                  | 0.052                        | 0.034               |

*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), (2), and (4) report logit model estimates; column (3) uses OLS for employment days in the first spell; column (5) uses the log of total contributions accumulated during the first year under the initial contract. All models include individual controls, nationality 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.7.** Differential effects of the labor reform by level of education in the first job

|                                        | Access to open-ended | Access to regular<br>open-ended | Employment days     | Spell<br>≥ 6 months | Earnings            |
|----------------------------------------|----------------------|---------------------------------|---------------------|---------------------|---------------------|
| <b>Panel A. Higher education</b>       |                      |                                 |                     |                     |                     |
| Post × Treated                         | 0.746***<br>(0.122)  | 0.491***<br>(0.072)             | 9.061***<br>(3.007) | 0.209***<br>(0.061) | 0.116**<br>(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               |
| <b>Panel B. Low education</b>          |                      |                                 |                     |                     |                     |
| Post × Treated                         | 0.671***<br>(0.242)  | 0.582***<br>(0.148)             | 12.123*<br>(6.155)  | 0.266*<br>(0.157)   | 0.160**<br>(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               |
| <b>Panel C. Lower-middle education</b> |                      |                                 |                     |                     |                     |
| Post × Treated                         | 0.545***<br>(0.191)  | 0.560***<br>(0.082)             | 5.268<br>(3.838)    | 0.028<br>(0.078)    | 0.096**<br>(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               |
| <b>Panel D. Upper-middle education</b> |                      |                                 |                     |                     |                     |
| Post × Treated                         | 0.574***<br>(0.165)  | 0.565***<br>(0.115)             | 7.389**<br>(3.356)  | 0.077<br>(0.063)    | 0.149***<br>(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 earnings of the first year are measured in logarithms. All models include individual controls, nationality 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.8.** Heterogeneous effects of the labor reform by level of education in the first job (pooled sample)

|                                                    | (1)<br>Any open-ended | (2)<br>Regular open-ended | (3)<br>Days of employment | (4)<br>Spell $\geq 6$ months | (5)<br>Earnings    |
|----------------------------------------------------|-----------------------|---------------------------|---------------------------|------------------------------|--------------------|
| Post $\times$ Treated                              | 0.739***<br>(0.125)   | 0.489***<br>(0.073)       | 7.365**<br>(2.965)        | 0.184***<br>(0.060)          | 0.107**<br>(0.046) |
| Post $\times$ Treated $\times$ Low education       | -0.089<br>(0.181)     | 0.062<br>(0.130)          | 5.104<br>(6.410)          | 0.095<br>(0.159)             | 0.083<br>(0.062)   |
| Post $\times$ Treated $\times$ Lower-Mid education | -0.204<br>(0.130)     | 0.062<br>(0.079)          | -1.926<br>(4.821)         | -0.154<br>(0.107)            | 0.015<br>(0.050)   |
| Post $\times$ Treated $\times$ Upper-Mid education | -0.152<br>(0.093)     | 0.091<br>(0.096)          | 0.499<br>(4.313)          | -0.089<br>(0.087)            | 0.059<br>(0.062)   |
| Individual controls                                | ✓                     | ✓                         | ✓                         | ✓                            | ✓                  |
| Nationality                                        | ✓                     | ✓                         | ✓                         | ✓                            | ✓                  |
| Employment trends                                  | ✓                     | ✓                         | ✓                         | ✓                            | ✓                  |
| Province (FE)                                      | ✓                     | ✓                         | ✓                         | ✓                            | ✓                  |
| Cohort year (FE)                                   | ✓                     | ✓                         | ✓                         | ✓                            | ✓                  |
| N                                                  | 177,541               | 177,541                   | 163,238                   | 163,238                      | 161,904            |
| Adj. $R^2$ / Pseudo- $R^2$                         | 0.176                 | 0.118                     | 0.007                     | 0.089                        | 0.038              |

*Notes:* The table reports Difference-in-Differences estimates on the pooled sample of entrants, with heterogeneous effects by educational attainment captured through triple interactions and higher education as the reference category. Column (1) reports access to any open-ended contract, column (2) access to a regular (ordinary) open-ended contract, column (3) the duration, in days, of the first employment spell, column (4) the probability that the first spell lasts at least six months, and column (5) the logarithm of earnings accumulated during the first spell. Columns (1), (2), and (4) report logit estimates, with coefficients in log-odds; columns (3) and (5) report OLS estimates. All specifications include individual controls, nationality controls, employment trends, province fixed effects, and cohort-year fixed effects. Robust standard errors clustered at the provincial level are reported in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

**Table A.9.** Differential effects of the labor reform by contribution group

|                            | (1)<br>Access to open-ended | (2)<br>Access to regular open-ended | (3)<br>Employment days | (4)<br>Spell $\geq 6$ months | (5)<br>Earnings     |
|----------------------------|-----------------------------|-------------------------------------|------------------------|------------------------------|---------------------|
| Post $\times$ Treated      | 0.600***<br>(0.162)         | 0.548***<br>(0.077)                 | 6.972**<br>(2.894)     | 0.097<br>(0.059)             | 0.139***<br>(0.038) |
| Individual controls        | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Nationality                | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Employment trends          | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Province (FE)              | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Cohort year (FE)           | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Contribution group         | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| N                          | 173,860                     | 173,860                             | 159,833                | 159,833                      | 158,500             |
| Adj. $R^2$ / Pseudo- $R^2$ | 0.173                       | 0.109                               | 0.008                  | 0.086                        | 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), (2), and (4) 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. Column (3) reports OLS estimates for the number of days worked during the first spell, and column (5) uses as dependent variable the logarithm of earnings accumulated during the first year under the initial contract. All models include individual controls, nationality 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.10.** Differential effects of the labor reform: upper vs lower tertile of  $Excess_p$ 

|                            | (1)<br>Access to open-ended | (2)<br>Access to regular open-ended | (3)<br>Employment days | (4)<br>Spell $\geq 6$ months | (5)<br>Earnings     |
|----------------------------|-----------------------------|-------------------------------------|------------------------|------------------------------|---------------------|
| Post $\times$ Treated      | 0.721***<br>(0.146)         | 0.620***<br>(0.103)                 | 9.128***<br>(3.433)    | 0.112*<br>(0.069)            | 0.164***<br>(0.044) |
| Individual controls        | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Nationality                | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Employment trends          | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Province (FE)              | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Cohort year (FE)           | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| N                          | 141,266                     | 141,266                             | 130,037                | 130,037                      | 128,902             |
| Adj. $R^2$ / Pseudo- $R^2$ | 0.168                       | 0.117                               | 0.008                  | 0.076                        | 0.038               |

*Notes:* The table reports Difference-in-Differences estimates in which *Treated* is defined using provinces in the top tertile of excess temporary employment in the pre-reform period, while the control group consists of provinces in the bottom tertile 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. Column (1) presents logit estimates for access to any open-ended contract in the first job, and column (2) for access to a regular open-ended contract. Column (3) reports OLS estimates for the number of employment days in the first employment spell. Column (4) presents logit estimates for the probability that the first employment spell lasts at least six months. Column (5) uses the logarithm of earnings accumulated during the first year under the initial contract as the dependent variable. All models include individual controls, nationality controls, employment trends, and province and entry-year fixed effects. Robust standard errors clustered at the provincial level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.11.** Differential effect of the labor reform among Spanish nationals

|                            | (1)<br>Access to open-ended | (2)<br>Access to regular open-ended | (3)<br>Employment days | (4)<br>Spell $\geq 6$ months | (5)<br>Earnings     |
|----------------------------|-----------------------------|-------------------------------------|------------------------|------------------------------|---------------------|
| Post $\times$ Treated      | 0.582***<br>(0.161)         | 0.583***<br>(0.080)                 | 6.911**<br>(2.972)     | 0.073<br>(0.055)             | 0.152***<br>(0.046) |
| Individual controls        | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Employment trends          | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Province (FE)              | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| Cohort year (FE)           | ✓                           | ✓                                   | ✓                      | ✓                            | ✓                   |
| N                          | 140,675                     | 140,675                             | 129,299                | 129,299                      | 128,182             |
| Adj. $R^2$ / Pseudo- $R^2$ | 0.152                       | 0.079                               | 0.006                  | 0.073                        | 0.028               |

*Notes:* Difference-in-Differences estimates for individuals with Spanish nationality. The variable *Treated* is defined based on the provincial pre-reform excess temporary employment. The post-reform period corresponds to 2022–2023. Column (1) reports logit estimates for access to any open-ended contract. Column (2) reports logit estimates for the probability of accessing a regular open-ended contract in the first job. Column (3) presents OLS estimates for the number of days worked under the first contract of the first employment spell. Column (4) reports logit estimates for the probability that the first employment spell lasts at least six months. Column (5) uses 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.12.** Differential effect of the labor reform on labor market outcomes: treatment assigned by province of birth

|                              | (1)                  | (2)                             | (3)                | (4)                   | (5)                 |
|------------------------------|----------------------|---------------------------------|--------------------|-----------------------|---------------------|
|                              | Access to open-ended | Access to regular<br>open-ended | Employment days    | Spell $\geq 6$ months | Earnings            |
| Post $\times$ Treated        | 0.381***<br>(0.110)  | 0.429***<br>(0.070)             | 3.773**<br>(1.560) | 0.007<br>(0.040)      | 0.095***<br>(0.025) |
| Average marginal effect (pp) | 2.64                 | 3.45                            | —                  | 0.35                  | —                   |
| Individual controls          | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| Employment trends            | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| Province (FE)                | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| Cohort year (FE)             | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| N                            | 124,046              | 124,046                         | 113,900            | 113,900               | 112,657             |
| Adj. $R^2$ / Pseudo- $R^2$   | 0.161                | 0.075                           | 0.006              | 0.076                 | 0.029               |

*Notes:* Difference-in-Differences estimates, where treatment and control status are assigned according to entrants' province of birth rather than the province of their first employer. The variable *Treated* is defined based on the provincial pre-reform excess temporary employment in the province of birth. The post-reform period corresponds to 2022–2023. Columns (1), (2), and (4) report logit model estimates for access to any open-ended, access to a regular open-ended contract, and the probability of a first spell lasting at least six months, respectively. Column (3) reports OLS estimates for the number of days worked in the first employment spell. Column (5) reports OLS estimates using the logarithm of earnings in the first employment spell as the dependent variable. Average marginal effects are reported for logit specifications only; coefficients from linear models (columns (3) and (5)) are already expressed in the units of the dependent variable. 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.13.** Differential effect of the labor reform on labor market outcomes: stayers

|                              | (1)                  | (2)                             | (3)                | (4)                   | (5)                 |
|------------------------------|----------------------|---------------------------------|--------------------|-----------------------|---------------------|
|                              | Access to open-ended | Access to regular<br>open-ended | Employment days    | Spell $\geq 6$ months | Earnings            |
| Post $\times$ Treated        | 0.480***<br>(0.143)  | 0.541***<br>(0.077)             | 5.095**<br>(2.168) | 0.008<br>(0.007)      | 0.130***<br>(0.033) |
| Average marginal effect (pp) | 3.40                 | 4.32                            | —                  | 0.38                  | —                   |
| Individual controls          | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| Employment trends            | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| Province (FE)                | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| Cohort year (FE)             | ✓                    | ✓                               | ✓                  | ✓                     | ✓                   |
| N                            | 99,122               | 99,122                          | 90,933             | 90,933                | 90,263              |
| Adj. $R^2$ / Pseudo- $R^2$   | 0.167                | 0.079                           | 0.006              | 0.083                 | 0.025               |

*Notes:* Difference-in-Differences estimates for the stayer subsample (entrants whose province of birth coincides with the province of their first employer). 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), and (4) report logit model estimates for access to any open-ended contract, access to a regular open-ended contract, and the probability of a first spell lasting at least six months, respectively. Column (3) reports OLS estimates for the number of days worked in the first employment spell. Column (5) reports OLS estimates using the logarithm of earnings in the first employment spell as the dependent variable. Average marginal effects are reported for logit specifications only; coefficients from linear models (columns (3) and (5)) are already expressed in the units of the dependent variable. 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.14.** Differential effect of the labor reform excluding entrants from January–March 2022

|                              | (1)                 | (2)                 | (3)                 | (4)                   | (5)                 |
|------------------------------|---------------------|---------------------|---------------------|-----------------------|---------------------|
|                              | Any open-ended      | Regular open-ended  | Employment days     | Spell $\geq 6$ months | Earnings            |
| Post $\times$ Treated        | 0.652***<br>(0.173) | 0.587***<br>(0.088) | 8.440***<br>(2.988) | 0.109*<br>(0.059)     | 0.139***<br>(0.039) |
| Average marginal effect (pp) | 6.843               | 4.322               | —                   | 1.982                 | —                   |
| Individual controls          | ✓                   | ✓                   | ✓                   | ✓                     | ✓                   |
| Nationality                  | ✓                   | ✓                   | ✓                   | ✓                     | ✓                   |
| Employment trends            | ✓                   | ✓                   | ✓                   | ✓                     | ✓                   |
| Province (FE)                | ✓                   | ✓                   | ✓                   | ✓                     | ✓                   |
| Cohort year (FE)             | ✓                   | ✓                   | ✓                   | ✓                     | ✓                   |
| N                            | 174,229             | 174,229             | 159,926             | 159,926               | 158,598             |
| Adj. $R^2$ / Pseudo- $R^2$   | 0.180               | 0.118               | 0.007               | 0.078                 | 0.037               |

*Notes:* The table reports Difference-in-Differences estimates for a sample that excludes entrants whose first job began during the transition period between January and March 2022. *Treated* is defined based on the provincial pre-reform excess of temporary employment, and the post-reform period corresponds to 2022–2023. Column (1) reports logit estimates for access to any open-ended contract. Column (2) reports logit estimates for access to a regular open-ended contract. Column (3) presents OLS estimates for the number of employment days in the first spell. Column (4) reports logit estimates for the probability that the first spell lasts at least six months. Column (5) uses the logarithm of earnings accumulated during the first spell as the dependent variable. All specifications include individual controls, employment trends, province fixed effects, and cohort-year fixed effects. Robust standard errors clustered at the provincial level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.15.** Differential effect of the labor reform excluding summer entrants (June–August)

|                              | Any open-ended      | Regular open-ended contract |                     |                     | First Spell            |                              |                     | First twelve months    |                              |                     |
|------------------------------|---------------------|-----------------------------|---------------------|---------------------|------------------------|------------------------------|---------------------|------------------------|------------------------------|---------------------|
|                              | (1)                 | All<br>(2)                  | Full-time<br>(3)    | Part-time<br>(4)    | Employment days<br>(5) | Spell $\geq$ 6 months<br>(6) | Earnings<br>(7)     | Employment days<br>(8) | Spell $\geq$ 6 months<br>(9) | Earnings<br>(10)    |
| Post $\times$ Treated        | 0.694***<br>(0.140) | 0.597***<br>(0.094)         | 0.491***<br>(0.102) | 0.608***<br>(0.095) | 10.005***<br>(3.411)   | 0.133**<br>(0.063)           | 0.171***<br>(0.041) | 8.029***<br>(2.510)    | 0.098*<br>(0.051)            | 0.163***<br>(0.027) |
| Average marginal effect (pp) | 7.913               | 4.892                       | 1.167               | 3.724               | –                      | 2.833                        | –                   | –                      | 2.256                        | –                   |
| Individual controls          | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Nationality                  | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Employment trends            | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Province (FE)                | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Cohort year (FE)             | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| N                            | 103,799             | 103,799                     | 103,799             | 103,799             | 97,580                 | 97,580                       | 96,594              | 97,580                 | 97,580                       | 96,594              |
| Adj. $R^2$ / Pseudo- $R^2$   | 0.157               | 0.106                       | 0.145               | 0.066               | 0.006                  | 0.049                        | 0.042               | 0.011                  | 0.061                        | 0.058               |

*Notes:* The table reports Difference-in-Differences estimates on a sample that excludes entrants whose first job began in June, July, or August, as a robustness check against seasonal hiring patterns. The variable *Treated* is defined based on the provincial pre-reform excess of temporary employment, and the post-reform period corresponds to 2022–2023. Column (1) reports logit estimates for access to any open-ended contract. Columns (2)–(4) report logit estimates for access to a regular open-ended contract, overall and split by weekly schedule (full-time and part-time). Column (5) presents OLS estimates for the number of employment days in the first spell. Column (6) reports logit estimates for the probability that the first spell lasts at least six months. Column (7) uses the logarithm of earnings accumulated during the first spell as the dependent variable. Column (8) presents OLS estimates for the total number of employment days during the first twelve months. Column (9) reports logit estimates for the probability of experiencing at least one employment spell lasting six months or more within the first twelve months. Column (10) uses the logarithm of earnings accumulated during the first twelve months as the dependent variable. The row “Average marginal effect” reports the average marginal effect from the logit specifications, expressed in percentage points; it is not applicable to the OLS specifications (columns 5, 7, and 10), denoted with “–”. All specifications include individual controls, employment trends, province fixed effects, and cohort-year fixed effects. Robust standard errors clustered at the provincial level are reported in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

**Table A.16.** Differential effect of the labor reform: sample restricted to first spells longer than 30 days

|                              | Any open-ended      | Regular open-ended contract |                     |                     | First Spell            |                              |                     | First twelve months    |                              |                     |
|------------------------------|---------------------|-----------------------------|---------------------|---------------------|------------------------|------------------------------|---------------------|------------------------|------------------------------|---------------------|
|                              | (1)                 | All<br>(2)                  | Full-time<br>(3)    | Part-time<br>(4)    | Employment days<br>(5) | Spell $\geq$ 6 months<br>(6) | Earnings<br>(7)     | Employment days<br>(8) | Spell $\geq$ 6 months<br>(9) | Earnings<br>(10)    |
| Post $\times$ Treated        | 0.608***<br>(0.184) | 0.525***<br>(0.088)         | 0.425***<br>(0.101) | 0.513***<br>(0.075) | 8.318***<br>(2.939)    | 0.095*<br>(0.055)            | 0.068***<br>(0.016) | 3.328<br>(2.046)       | 0.012<br>(0.011)             | 0.064***<br>(0.015) |
| Average marginal effect (pp) | 8.136               | 5.620                       | 1.619               | 4.001               | –                      | 1.802                        | –                   | –                      | 0.703                        | –                   |
| Individual controls          | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Nationality                  | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Employment trends            | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Province (FE)                | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| Cohort year (FE)             | ✓                   | ✓                           | ✓                   | ✓                   | ✓                      | ✓                            | ✓                   | ✓                      | ✓                            | ✓                   |
| N                            | 109,079             | 109,079                     | 109,079             | 109,079             | 99,936                 | 99,936                       | 99,749              | 99,936                 | 99,936                       | 99,749              |
| Adj. $R^2$ / Pseudo- $R^2$   | 0.157               | 0.108                       | 0.142               | 0.075               | 0.010                  | 0.079                        | 0.103               | 0.016                  | 0.088                        | 0.130               |

*Notes:* The table reports Difference-in-Differences estimates on a sample restricted to entrants whose first employment spell lasted longer than 30 days, as a robustness check against very short-lived spells that may reflect measurement error or trial periods. The variable *Treated* is defined based on the provincial pre-reform excess of temporary employment, and the post-reform period corresponds to 2022–2023. Column (1) reports logit estimates for access to any open-ended contract. Columns (2)–(4) report logit estimates for access to a regular open-ended contract, overall and split by weekly schedule (full-time and part-time). Column (5) presents OLS estimates for the number of employment days in the first spell. Column (6) reports logit estimates for the probability that the first spell lasts at least six months. Column (7) uses the logarithm of earnings accumulated during the first spell as the dependent variable. Column (8) presents OLS estimates for the total number of employment days during the first twelve months. Column (9) reports logit estimates for the probability of experiencing at least one employment spell lasting six months or more within the first twelve months. Column (10) uses the logarithm of earnings accumulated during the first twelve months as the dependent variable. The row “Average marginal effect” reports the average marginal effect from the logit specifications, expressed in percentage points; it is not applicable to the OLS specifications (columns 5, 7, and 10), denoted with “–”. All specifications include individual controls, employment trends, province fixed effects, and cohort-year fixed effects. Robust standard errors clustered at the provincial level are reported in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

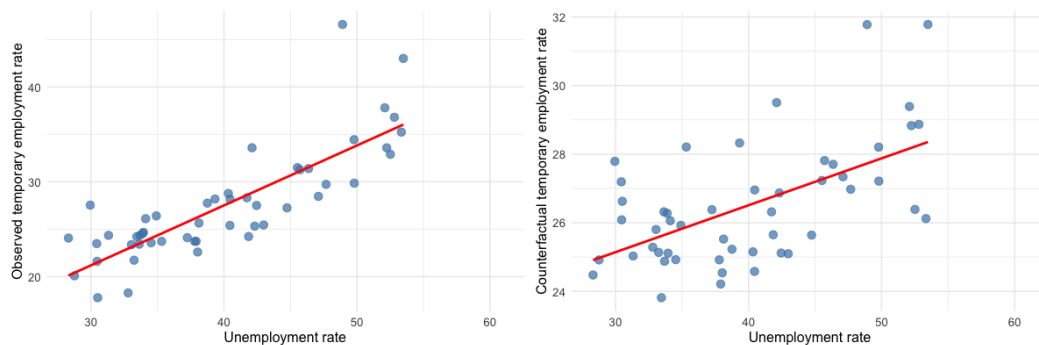
**Table A.17.** Differential effect of the labor reform on days worked in the first spell by contract type

|                       | (1)                   | (2)                     | (3)              |
|-----------------------|-----------------------|-------------------------|------------------|
|                       | Regular open-ended    | Intermittent open-ended | Temporary        |
| Post $\times$ Treated | -34.655***<br>(5.266) | 0.047<br>(10.642)       | 1.078<br>(2.055) |
| Individual controls   | ✓                     | ✓                       | ✓                |
| Nationality           | ✓                     | ✓                       | ✓                |
| Employment trends     | ✓                     | ✓                       | ✓                |
| Province (FE)         | ✓                     | ✓                       | ✓                |
| Cohort year (FE)      | ✓                     | ✓                       | ✓                |
| N                     | 29,688                | 10,545                  | 123,005          |
| Adj. $R^2$            | 0.012                 | 0.017                   | 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 column limits the sample to workers whose first contract corresponds to the indicated type: regular open-ended contracts in column (1), intermittent open-ended contracts in column (2), and temporary contracts in column (3). The dependent variable is the number of days worked during the first employment spell. All models include individual controls, nationality controls, employment trends, and fixed effects for province and entry year. Robust standard errors clustered at the province level are shown in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

## Figures

**Figure A.1.** Temporary employment and unemployment across provinces, 2014–2019



*Notes:* The left panel plots, for each province, the average unemployment rate over 2014–2019 against the observed average temporary employment rate over the same period. The right panel repeats the exercise using the counterfactual, sector-adjusted temporary employment rate described in Section 4, computed leaving out each province's own contribution to the national sectoral average (leave-one-out). Each point represents a province; the solid red line shows the corresponding unweighted linear fit.

**Figure A.2.** Evolution of excess temporary employment by province

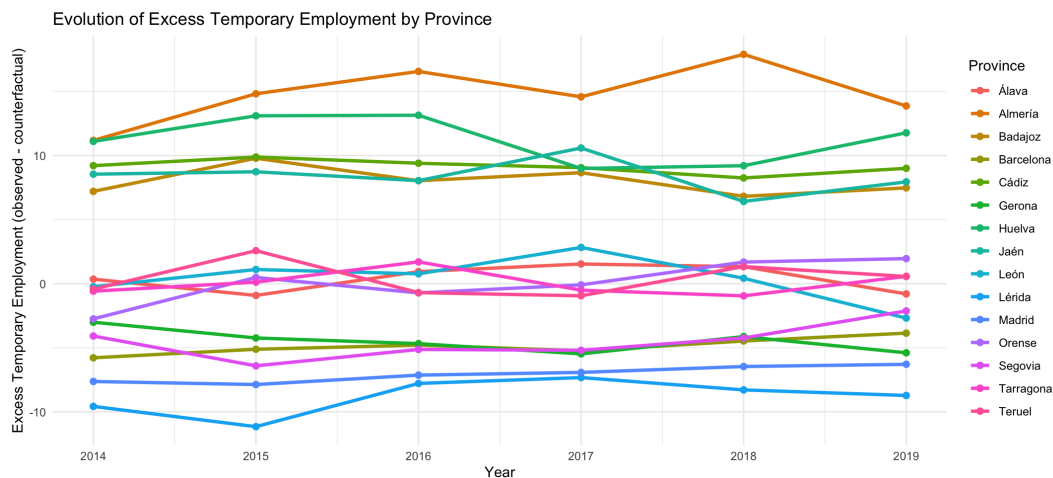
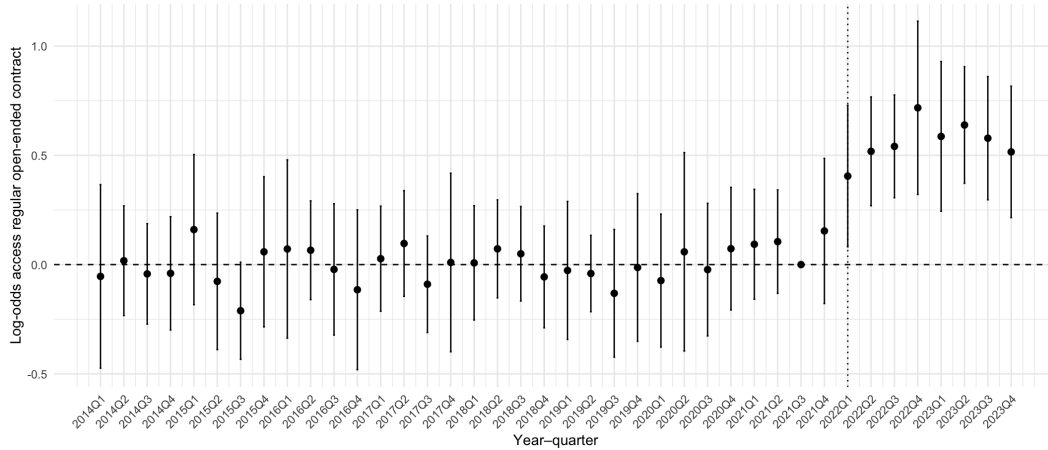


**Figure A.3.** Absolute change in access to regular open-ended contracts by education level



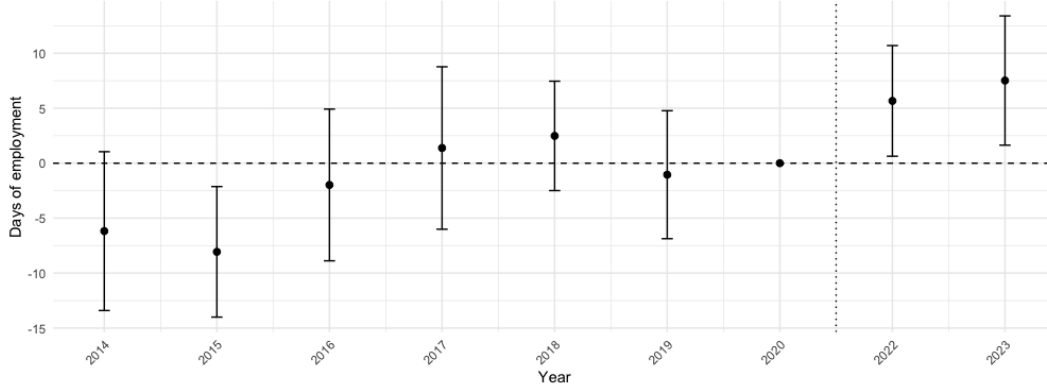
*Notes:* The figure shows for each province the relationship between pre-reform excess temporary employment and the absolute change in access to regular open-ended contracts after the labor reform, disaggregated by education level. The absolute change is calculated as the variation in percentage points 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.4.** Differential effect of the labor reform on access to regular open-ended contracts



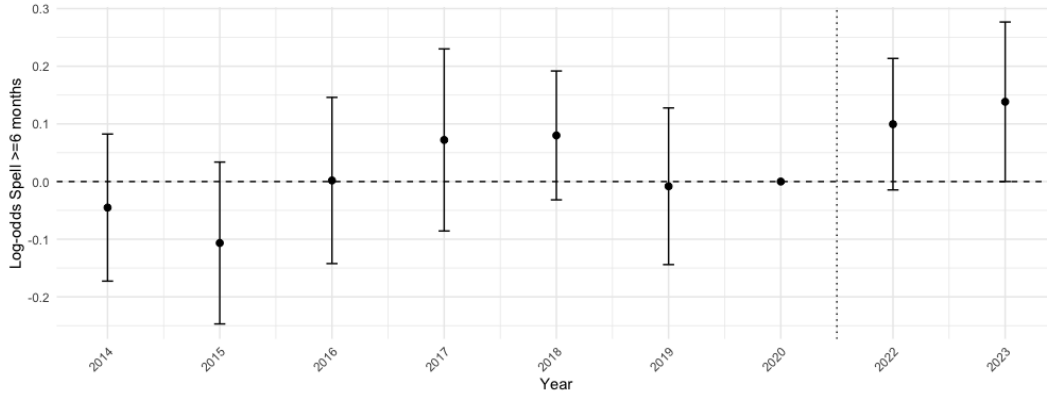
*Notes:* The figure shows the estimated coefficients from the event study model, capturing the differential effect of the labor reform on the probability of accessing a regular open-ended contract as a first job. The coefficients are reported in log-odds estimated from a logit model. Since the treatment 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). Vertical bars represent 95% confidence intervals.

**Figure A.5.** Differential effect of the labor reform on employment days



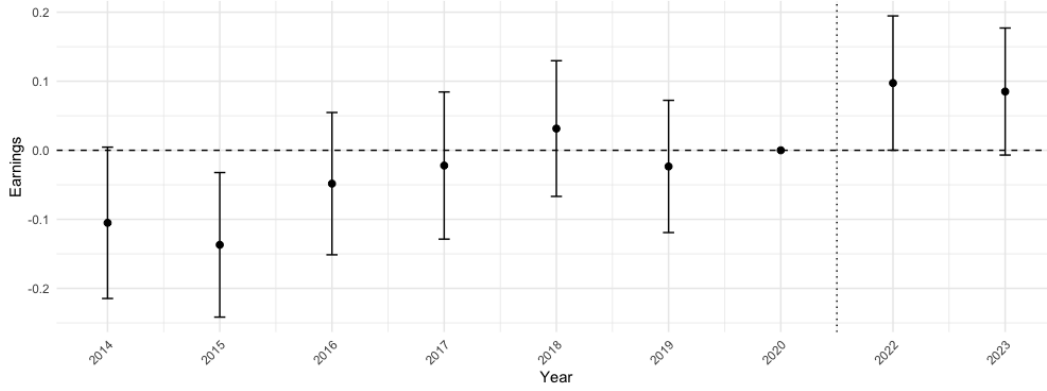
*Notes:* The figure shows the estimated coefficients from the event study model, estimated at annual frequency, capturing the differential effect of the labor reform on employment days in the first job. Since the treatment is interacted with year-of-entry dummies, each coefficient represents the differential effect of the reform in a specific cohort year relative to the reference pre-reform year (2020). The 2021 cohort is excluded from the estimation to avoid contamination from anticipation effects and from the overlap between the pre- and post-reform periods, as discussed in Section 5. Vertical bars represent 95% confidence intervals.

**Figure A.6.** Differential effect of the labor reform on a spell lasting at least 6 months



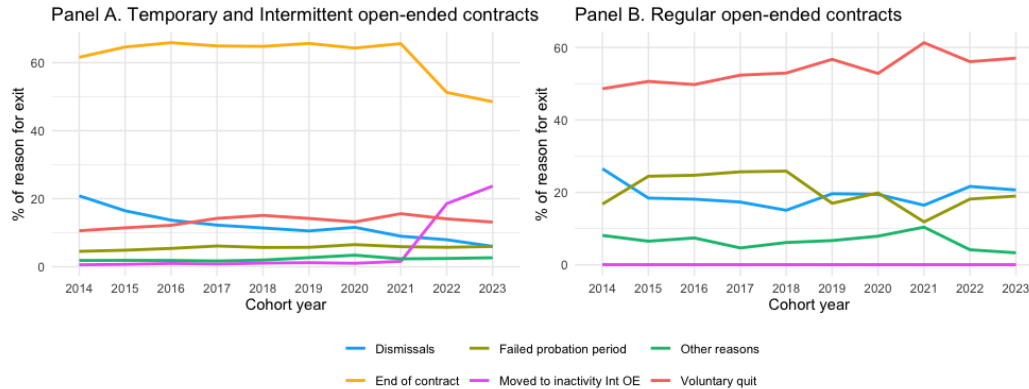
*Notes:* The figure shows the estimated coefficients from the event study model, estimated at annual frequency, capturing the differential effect of the labor reform on the probability of the entry spell lasting at least 6 months. Since the treatment is interacted with year-of-entry dummies, each coefficient represents the differential effect of the reform in a specific cohort year relative to the reference pre-reform year (2020). The 2021 cohort is excluded from the estimation to avoid contamination from anticipation effects and from the overlap between the pre- and post-reform periods, as discussed in Section 5. Vertical bars represent 95% confidence intervals.

**Figure A.7.** Differential effect of the labor reform on earnings



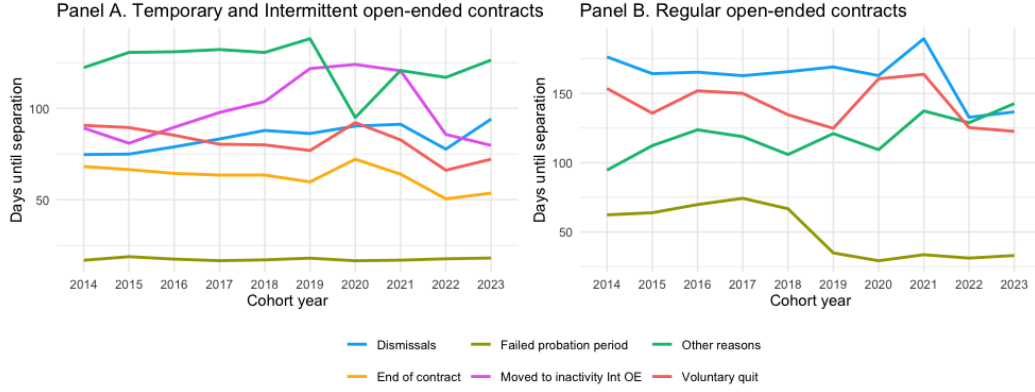
*Notes:* The figure shows the estimated coefficients from the event study model, estimated at annual frequency, capturing the differential effect of the labor reform on earnings. The dependent variable is the logarithm of earnings, so each coefficient can be interpreted as the approximate percentage change in earnings relative to the reference period. Since the treatment is interacted with year-of-entry dummies, each coefficient represents the differential effect of the reform in a specific cohort year relative to the reference pre-reform year (2020). The 2021 cohort is excluded from the estimation to avoid contamination from anticipation effects and from the overlap between the pre- and post-reform periods, as discussed in Section 5. Vertical bars represent 95% confidence intervals.

**Figure A.8.** 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 (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 (93). *Moved to inactivity Int OE*: includes transitions to inactivity for intermittent open-ended workers (94). Finally, *Other reasons*: includes retirement (58), administrative deregistration (60), expiration of sickness leave entitlement (65), childcare leave (68), family-care leave (73), suspension of the employment contract (74), and all remaining exit codes (99). A detailed definition of each exit reason code is available in the MCV Guide.

**Figure A.9.** Days until separation 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. 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 (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 (93). *Moved to inactivity Int OE*: includes transitions to inactivity for intermittent open-ended workers (94). Finally, *Other reasons*: includes retirement (58), administrative deregistration (60), expiration of sickness leave entitlement (65), childcare leave (68), family-care leave (73), suspension of the employment contract (74), and all remaining exit codes (99).

**Figure A.10.** Density function of days of employment

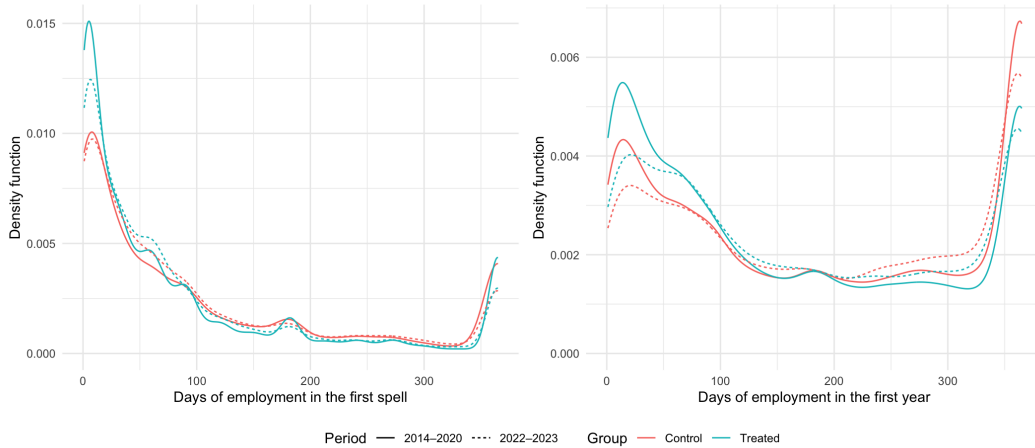
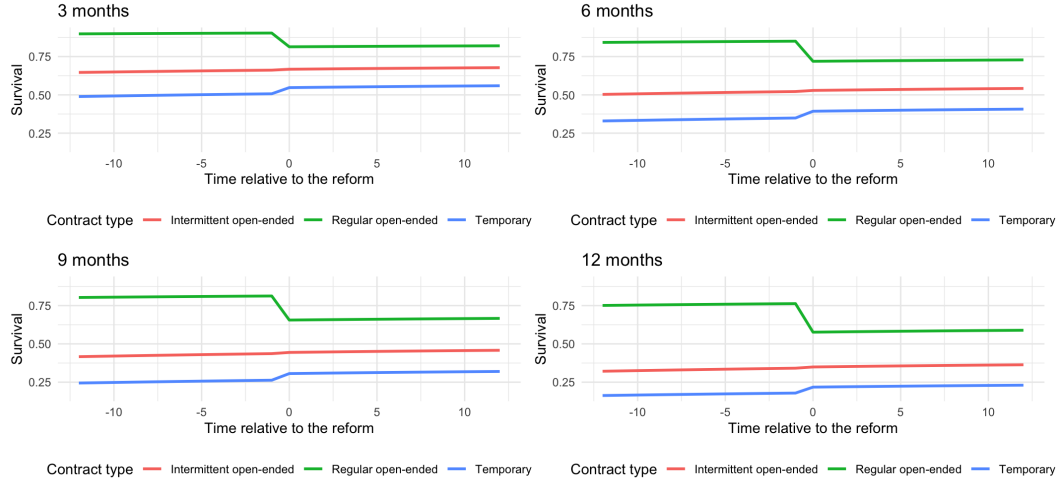


**Figure A.11.** Employment survival rates



*Notes:* The figure shows predicted probabilities of remaining employed (survival function) for a representative individual, 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 constructed from the predicted exit probabilities of the duration model for provinces with positive excess temporary employment (i.e., treated provinces), holding the remaining covariates at their mean values or reference categories so as to represent an individual with average sample characteristics. Three contract types are distinguished: temporary, intermittent open-ended, and regular open-ended.