Quasi-Random Matches: Evidence from Dual Labor Markets*

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María Alexandra Castellanos† Henry Redondo ‡ Jan Stuhler §

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Abstract

A fast-growing literature studies how sorting into particular jobs, firms, or locations affects workers. The key challenge when studying such questions is the non-random sorting of workers. We propose a novel identification strategy that exploits the *timing* of worker-firm matching, by interacting high-frequency information on the duration of contracts on the labor supply and transitory fluctuations in job creation on the labor demand side. We apply this method to address a central question in *dual labor markets*: how do different contract types – fixed-term or permanent contracts – affect workers' careers? We find that transitory variation in the opening of permanent contracts is highly predictive of individual promotion probabilities. Reaching a permanent position translates into higher employment and earnings growth in the short-run. However, the stability derived from these positions does not seem to lead to long-lasting earnings growth differentials.

JEL Codes: J29, J31, J41, J60

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†Universidad Carlos III de Madrid. Email: marcaste@eco.uc3m.es.

‡Universidad de Alicante. Email: henry.redondo@ua.es.

§Universidad Carlos III de Madrid. Email: jstuhler@eco.uc3m.es.

1 Introduction

A fast-growing literature studies how sorting into particular jobs, firms, or locations affects workers. Following Abowd et al. (1999), there has been much interest in the observation that pay premia vary across firms, the mechanisms that generate such variation (Manning 2021, Card et al. 2018), and its implications (Card et al. 2013). A natural question then is whether jobs also differ in their *dynamic* implications – if workers learn more and enjoy faster earnings growth in some jobs while being "stuck" in others. Indeed, recent studies suggest that earnings growth varies systematically across firms (Arellano-Bover and Saltiel 2021, Pesola 2011), regions (Roca and Puga 2017), and jobs (Kambourov and Manovskii 2009; Gathmann and Schönberg 2010; Garcia-Louzao et al. 2023).

The key challenge when studying such questions is the non-random sorting of workers into jobs. For example, firms paying higher wages might attract better applicants, and workers in urban labor markets might differ from those in rural areas. To address this selection problem, the literature often adopts a fixed effect strategy: by tracking workers across firms, researchers can decompose wages into time-constant differences between individuals (individual fixed effects) and match-specific components (such as firm fixed effects, as in Abowd et al. 1999). While this strategy is ubiquitous, there is an obvious tension: if workers or firms differ in their *level* of pay, they might also differ in wage *growth*, which the fixed effects would not capture.

In this paper, we propose an alternative strategy that exploits the *timing* of worker-firm matching. Specifically, we isolate quasi-random variation in matches by interacting high-frequency information on (i) the duration of contracts on the supply side of the labor market and (ii) transitory fluctuations in job creation on the demand side. We apply this method to address a central question in "dual" labor markets: how do different contract types – fixed-term (FT) or open-ended contracts (OEC) – affect workers' careers? A common concern is that fixed-term contracts may discourage firms from providing training or other investments to their workers (Cabrales et al. 2017; Albert et al. 2005). While we focus on the consequences for workers, this problem has important aggregate implications, and the prevalence of fixed-term contracts is one suspected reason for low labor productivity in countries characterized by dual labor markets (Cahuc et al. 2016).[1]

Our application focuses on Spain. With the highest rate of temporary employment in Europe of nearly 25% (See Figure 1) and as much as 90% of new contracts being fixed-term (until a major reform in 2022), the country provides an interesting context. Moreover, we can exploit rich, matched employer-employee data from Social Security records that track

[1] In addition, other relevant outcomes may be affected by labor market duality, such as fertility (Auer and Danzer 2016; Lopes 2020; Nieto 2022) and migration (Llull and Miller, 2018).

workers over time and contain detailed information on the type and length of individual employment contracts.

We first provide evidence using a standard fixed effects approach, estimating an earnings equation that allows for time-constant differences between individuals and different rates of worker experience gained in fixed-term or open-ended contracts. Consistent with recent evidence by Garcia-Louzao et al. (2023), we find that earnings growth is higher for workers with more experience in open-ended contracts: while earnings grow by 2.7 percent for each year of experience in FTs, they grow by 3.6 percent per year in OECs. These patterns are highly non-linear, and the gap is much greater for experienced than young, inexperienced workers. An intuitive interpretation of these findings is that fixed-term contracts slow skill acquisition and wage growth (i.e., differences in returns to experience). However, they could also be due to workers who secured an OEC early in their career experiencing higher wage growth *irrespectively* of current contract type (i.e., selection).

A key piece of evidence to distinguish between these competing interpretations is an event study graph studying wage growth around contract switches. For example, Card et al. (2013) show that workers who switch from low- to higher-paying firms tend to experience similar wage growth as those that make the reverse switch ("parallel pre-trends"), suggesting that worker-firm matching is sufficiently random in a dynamic sense. However, we show that the parallel trends assumption does not hold in dual labor markets: workers who switch into an open-ended contract as opposed to another fixed-term contract experienced higher wage growth even *before* they entered their new contract. The difference is sizable: while the earnings of workers switching to an open-ended contract grow, on average, by 5% in the year before the switch, earnings growth is negligible for workers who switch to another fixed-term contract instead. This gap remains large when controlling for a detailed set of worker characteristics. This observation suggests that the matching of workers to contract types is not random in a dynamic sense: the differences in wage growth between fixed-term and open-ended contracts primarily reflect heterogeneity between workers rather than differences in returns between contract types.

The selection of workers into contracts is, therefore, a more difficult problem than the selection into firms (Card et al., 2013) or regions (Card et al., 2021). We discuss several reasons why this might be the case. One factor is that the switch to open-ended contracts occurs more often within firms and is therefore based on more information than in the case of workers switching to other firms. Moreover, switching into an OEC within a firm can be a form of promotion; and promotions depend, of course, on the recent performance of the worker. Finally, higher-ability workers are more likely to be matched to better fixed-term contracts, i.e., they might be able to find actual stepping-stones. They would, therefore, display differential pre-trends even before switching to a permanent position.

Our paper, therefore, adds to two distinct strands of literature. On the methodological side, we relate to recent papers extending the standard two-way fixed effects specification to account for more complicated forms of selection. For example, Roca and Puga (2017) evaluate returns to experience heterogeneity based on city size. Their approach explores both static and dynamic advantages, allowing for heterogeneity of city gains across workers by interacting individual fixed-effects (a measure of unobserved innate ability) with city-size-specific experience. Similarly, Arellano-Bover and Saltiel (2021) show that returns to experience vary across firm types. Applying a clustering methodology, they are able to classify firms into *skill-learning* classes, which they show are not predicted by firms' observable characteristics.

Compared to these papers, we follow a different strategy: rather than enriching the fixed effects specification to account for specific forms of heterogeneity and dynamic selection, we isolate quasi-random variation in matching workers and firms using an instrumental variable strategy. That is, rather than trying to control for dynamic selection by modeling it explicitly, we aim to circumvent it. Specifically, we interact individual variation in the expiration date of fixed-term contracts with transitory fluctuations in the opening of new open-ended jobs over time to isolate exogenous variation in contract type.

Conceptually, our strategy is similar to studies that analyze the effects of labor market conditions at the entry on worker careers – "graduating in a recession" – (Oreopoulos et al. 2012; Kahn 2010), in particular, recent work by Arellano-Bover (2024) on the selection of workers into different firm types. However, rather than exploiting yearly variation in the labor market entry of recent graduates, we exploit high-frequency information on the duration of contracts. Specifically, exploiting the precision of administrative employment records, we are able to match the precise month when the individual's contract is about to end with transitory variation in job openings at the regional level. Our approach faces the usual challenges in establishing instrument relevance and validity. The upside, however, is that we do not have to specify the functional form of individual heterogeneity and dynamic selection.

We first establish the instrument's relevance, showing that the (leave-one-out) sum of new open-ended contracts is highly predictive for a worker to switch from a fixed-term into an open-ended contract. We then provide evidence to support the instrument independence assumption and exclusion restriction. Instrument independence would imply that facing more open-ended job openings (relative to trend) in the month a contract ends is as-good-as random for the worker. To support this assumption, we show that our instrument is indeed broadly uncorrelated with worker characteristics. However, the exclusion restriction is unlikely to hold without further adjustments. The number of new open-ended contracts (our instrument) does, of course, correlate with general business cycle conditions, so it is not obvious whether a worker enjoys higher wage growth because she started in an open-ended contract or because the economic conditions in this period were generally favorable, affecting wage growth conditional on the contract type. The objective, therefore, becomes to control for general economic trends while exploiting the exact timing of when an individual switched jobs, i.e., we exploit high-frequency variation in the types of contracts available while controlling for low(er)-frequency business cycle variation.

To the best of our knowledge, we are the first to exploit this source of exogenous variation to deal with the endogenous sorting of workers into jobs. We argue that it is applicable in many settings. While administrative panel data are not without problems, they offer highly precise (typically, daily) information on the duration of contracts, as this information is directly relevant to the calculation of taxes and social security contributions. Our approach, therefore, exploits a comparative advantage of administrative data (their high frequency), similarly as the fixed effects approach exploits another (their scale).

Apart from this methodological contribution, we add to the active literature on dual labor markets (Bentolila et al. 2020). The two-tier segmentation that characterizes many European labor markets results from a series of reforms that started in the 1980s and intended to tackle high structural unemployment. Fueled by regulations that aimed to introduce more hiring flexibility, fixed-term contracts became widespread. While these low-firing-cost contracts may, in theory, help workers avoid long periods of unemployment, they may also come at the expense of lower human capital accumulation and poor progression toward better jobs. Indeed, previous studies have shown that workers in temporary positions receive less firm-provided training (Cabrales et al. 2017; Bratti et al. 2021). With asymmetric on-the-job learning opportunities and uncertain conversion to permanent positions, long histories of recurrent fixed-term spells can perpetuate workers in low-wage-growth trajectories (Gagliarducci, 2005). While fixed-term contracts may serve as stepping-stones to more stable jobs, the favorable evidence mostly corresponds to countries with low firing costs for fixed and open-ended positions alike (Bentolila et al., 2020). For countries such as Spain and Italy, where not only the share of temporary jobs is higher but also the gaps in employment protection by type contract are large, these contracts more often result in "dead ends" (Güell and Petrongolo 2007; García-Pérez and Muñoz-Bullón 2011; Garcia-Louzao et al. 2023).

The paper is organized as follows: Section 2 provides a background of the institutional framework, Section 3 introduces the main data source, Section 4 provides a characterization of dualism in Spain and preliminary results of a mincerian approach, Sections 5 and 6 discuss the main sources of endogeneity and our identification strategy, respectively and Section 7.1 analyses the effect of upgrade promotion in workers' career trajectory by evaluating a series of labor market outcomes.

2 Institutional framework

After the democratic transition, Spain's institutions underwent major changes, including reforming its labor market legislation. Before 1976, labor laws in Spain were liberal (Toharia, 2002), as most labor contracts required only the acceptance of both employers and employees. The first step toward modernization was Law 16/1976.[2] Under this law, however, all contracts were considered full-time permanent, except where special hiring flexibility was required.

Initiating the dualism of the Spanish labor market, Law 32/1984 established the coexistence of permanent and temporary contracts; the latter was used to promote job creation. With this reform, even firms with no seasonal activities could sign temporary contracts. Therefore, firms had the choice between opening permanent vacancies with a high severance payment or temporary vacancies with a smaller severance payment. The reform did not alter any of the conditions for permanent contracts, and made temporary contracts more appealing for firms (García-Pérez et al. 2019, Aguirregabiria and Alonso-Borrego 2014).

As a response, a new reform in 1994 restricted temporary contracts to seasonal activities and relaxed dismissal conditions for permanent employees. In practice, however, employers continued hiring temporary workers, not just for seasonal jobs (García-Pérez et al., 2019). This perceived ineffectiveness of the 1994 reform led to additional reforms in 1997 and 2001. The changes created a new permanent contract with a smaller severance payment of 33 days per year worked compared to the 45 in the previous reforms—this new contract was aimed at the young, workers older than 45, and those with disabilities.[3]

It was not until 2012 that severance payments for permanent employees were significantly reduced. The compensation at the termination of the temporary contract was increased, reducing the gap between the dismissal costs of workers with permanent and temporary contracts. In addition, the reform eliminated interim wages in judicial processes. A new open-ended contract was introduced for firms below 50 employees, entailing no severance pay during an extended probationary period of one year. But fixed-term contracts still accounted for more than 20% of all employees.

Various reforms have been implemented in the last 30 years to decrease labor market

[2] Ley 16/1976 de 8 de Abril de Relaciones Laborales.

[3] The reform of 2001 also included women hired in sectors where they are underrepresented and long-term unemployed.

dualism while preserving hiring flexibility. The proportion of workers in a temporary contract has also decreased during that time. Still, many workers begin their working career on a temporary contract and experience a long sequence of unstable jobs. One major concern is that this lack of job stability has adverse consequences for the accumulation of human capital, fertility, and wages.

3 Data

Our main data source combines the 2006-2021 waves of the Continuous Sample of Working Lives (*Muestra Continua de Vidas Laborales* or MCVL). The microdata from the MCVL constitutes a 4% non-stratified random sample of Spain's Social Security administrative records. The sample allows tracking the full working history of individuals back to 1967 and the monthly earnings since 1980. Once an individual with an ongoing relationship with Social Security is included in the sample, it remains in all future waves.[4] Furthermore, every year, those individuals who are no longer affiliated with Social Security are replaced with new workers (along with their whole past labor history). This updating exercise ensures that the sample remains representative.

A key advantage of register-based sources such as the MCVL is their high-frequency records, reporting each contract's exact start and end dates. This enables us to measure workers' labor market conditions at a very detailed level, and enables the identification strategy proposed in this paper. Since we have information on each spell's entry and exit date, we are also able to compute the exact days an employee worked. Whenever there is an overlap of spells, we preserve the job characteristics of the main job, i.e., the largest spell of the month. We are then able to build a reliable measure of tenure and work experience with a clear distinction between the experience accumulated in fixed-term and open-ended contracts.

Furthermore, the Social Security records are matched with annual information from the municipal population registry (*Padrón Continuo Municipal*) and income tax records from 2006 onward. The former allows us to expand on workers' demographic characteristics, and the latter on additional worker and firm characteristics. We observe the date of birth, gender, educational attainment, and country of birth of each worker. While we do not observe occupation directly, we sort workers into five occupational-skill groups that we define based on ten occupational contribution categories that employers must report to the Social Security Administration. In principle, these refer to the skills required for a particular job and not necessarily those acquired by the worker. Still, they are closely

[4]Employees, self-employed individuals, pensioners, and people receiving unemployment benefits are included in this category.

related to the required formal education to execute a particular job.

At the firm level, we observe the province where the firm is located and its size since 2006. Strictly speaking, while a firm can have more than one establishment in different provinces, we treat each establishment as a separate firm. Additionally, for each job, we observe the sector of the economic activity at the two-digit level, the type of contract (permanent or fixed term, full-time or part-time), and whether the worker is self-employed, or a private or public sector employee.

The MCVL contains information on earnings from two distinct sources: social security and tax records. Given that the social security taxable base is bottom and top coded,[5] we compute monthly real earnings from tax records whenever available,[6] which are not subject to censorship. Combining data from several waves allows us to reconstruct the history of tax records, which, unlike social security records, do not contain the workers' retrospective history. In earlier years, we used information from social security. Likewise, given that the Autonomous Communities of Navarre and Basque Country collect income taxes independently from the National Government, we only observe social security records for workers of those regions. As we have accurate information on the length of each spell, we can also compute daily wages.

3.1 Sample restrictions

Our study evaluates the 1998-2020 period. Although we can trace each worker's earnings trajectory back to the 1980s, information on the type of contract is reliable only from 1998 onwards. To mitigate the potential impact of the COVID-19 pandemic on job creation, we limit observations up to February 2020. We focus on native workers aged 18-49. Lastly, we narrow our analysis to workers registered in the general social security regime or the special regime for agrarian, seamen, and mining workers. This excludes autonomous workers, as they do not hold open-ended contracts and thus fall outside the scope of our study.

In our main specification, we only consider private sector workers, as the contract duration of public sector employees is highly regulated and centralized, as well as the promotion to permanent positions relies on a special process.[7] However, whenever this is the case, our measure of experience does take into account the time that a private employee previously worked in the public sector, either in a fixed or a permanent contract.

[5]The upper and lower bounds are specified by sector and updated every year.

[6]Nominal wages are deflated using the 2009 Consumer Price Index.

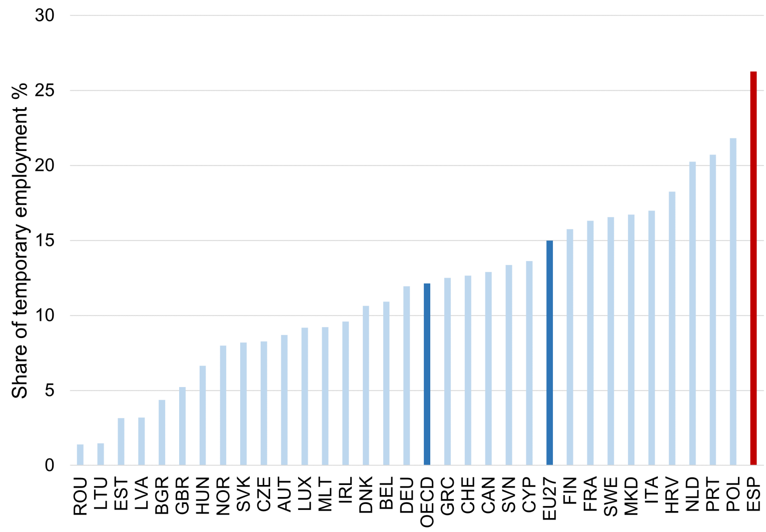
[7]Workers in the public sector are usually required to approve specific exams and fulfill special requirements to get a permanent position. This process is quite different from the promotion path of private sector workers.

Regionally, we exclude information from Ceuta and Melilla, for which the sample of workers is very small. Thus, we work with data from 50 provinces.

4 Descriptive evidence

Over the past decade, about one-third of all Spanish workers employed annually were on a fixed-term basis. Despite a decline in the share of temporary workers in the aftermath of the Great Recession (Appendix Figure A.1.1), until recently, the share of temporary employment in Spain was the highest among most European and OECD countries (Figure 1).[8]

Figure 1: Proportion of workers in temporary contracts by country, 2019



Notes: Share of dependent employees in temporary employment for OECD countries.
Source: OECD, Employment and Labour Market Statistics.

As previously discussed, the high dualism in the Spanish labor market suggests that instead of serving as stepping-stones, a significant portion of fixed-term contracts lead to "dead-ends" (Bentolila et al., 2019). While this problem is more severe for low-skilled occupations, it cannot be neglected at the top of the distribution. As shown in Table 1, the share of high-skilled occupations among temporary contracts has steadily increased. These contracts are equally spread among women and men regarding other workers and job characteristics. While most of these contracts correspond to full-time positions, the proportion of part-time jobs under this modality has increased substantially, representing almost one-third of these jobs by 2016.

[8]The recent 2022 Spanish labor reform, formally approved on the 31st of December 2021, brought some relevant changes to the historical values analyzed in this paper, as illustrated in Figure ??.

Table 1: Characteristics of workers in fixed-term contracts

	2004	2008	2012	2016
Age group				
<24	0.207	0.174	0.116	0.112
24-35	0.487	0.458	0.433	0.388
36-50	0.262	0.316	0.373	0.400
>50	0.044	0.052	0.079	0.099
Foreign	0.137	0.234	0.205	0.176
Female	0.429	0.457	0.500	0.489
Part-time	0.192	0.198	0.308	0.317
Occupations				
Very high skilled occupations	0.050	0.059	0.083	0.080
High-skilled occupations	0.070	0.081	0.100	0.095
Medium-high skilled occupations	0.117	0.126	0.142	0.134
Medium low skilled occupations	0.475	0.479	0.431	0.419
Low-skilled occupations	0.288	0.255	0.244	0.272

Notes: Characteristics of workers employed under fixed-term contracts.

For comparability with previous studies on returns to experience (Roca and Puga, 2017; Garcia-Louzao et al., 2023; Arellano-Bover and Saltiel, 2021), we first estimate the contribution of contract-specific experience to earnings growth using Mincer-type regressions that flexibly account for combinations of experience accumulated in fixed-term and open-ended contracts. We estimate the following equation by OLS:

$$\ln w_{irt} = f(exp_{it}^{FT}, exp_{it}^{OEC}, exp_{it})\boldsymbol{\beta} + X_{it}'\boldsymbol{\Omega} + \sigma_r + \psi_t + \varepsilon_{irt}, \quad (1)$$

where exp_{it}^{FT}, exp_{it}^{OEC} and exp_{it} denote the experience that worker i accumulated until period t in fixed-term, open-ended or any contracts, respectively, X_{it} is a vector of time-varying individual and job characteristics, σ_r and ψ_t are province and year-month fixed-effects, and ε_{ict} is the error term.

The first years of experience in open-ended or fixed-term contracts yield similar wage returns, but the growth rate for those in fixed-term contracts is lower in subsequent years (see Appendix C for details). For a worker with ten years of experience, an additional year on a fixed-term contract translates into a 3.0% increase in earnings. In contrast, an additional year in an open-ended contract is associated with a 4.5% surge. Although this specification acknowledges that the value of accumulated experience in each type of contract might differ, it ignores the potential sorting of workers into each type of contract. Previous work has addressed this concern by including worker-fixed effects. This slightly attenuates the gap between fixed-term and open-ended contract returns, but the overall pattern remains unchanged. For a worker with ten years of experience, an additional year in a fixed-term position is associated with a wage growth of 4.6% as compared to 5.6% if this experience was accumulated in a permanent contract (see Appendix Figure C.1.1 for illustration).

This finding of lower wage growth in fixed-term contracts is consistent with the work of (Garcia-Louzao et al., 2023), who also show that this discrepancy cannot be attributed to unobserved firm heterogeneity or match quality.[9] However, we show next that our descriptive estimates from a Mincerian specification with individual fixed effects have no causal interpretation; instead, they reflect that more able workers are (i) more likely to enter an open-ended contract and (ii) enjoy faster earnings growth irrespective of contract type, a form of selection that is not captured by the fixed-effects approach.

5 Selection into permanent positions

The results from the fixed effect model provide suggestive evidence about the differential value of experience that each of these contracts produce: with fewer on-the-job-training opportunities, a temporary contract in a country with high dualism might result in less skill accumulation (Cabrales et al., 2017) and slower wage growth. However, a worker fixed-effects specification only captures part of the endogeneity problem arising from contract sorting.

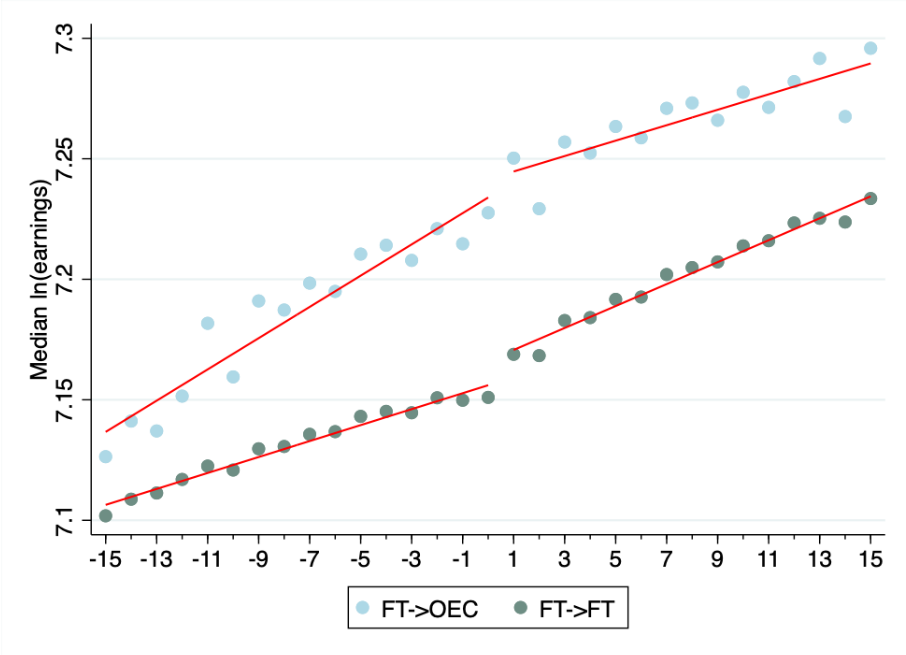
To assess this possibility, we examine whether workers with open-ended and fixed-term contracts follow parallel earnings paths before they are promoted. Following a similar methodology as Card et al. (2013) and Card et al. (2023), we classify workers by type of contract and evaluate the trajectory of each group before and after switching to a new contract. Figure 2 displays the median (ln) earnings of each group of workers relative to the month in which they started a new position (fixed-term or open-ended). Unlike other settings,[10] we observe that workers who will eventually switch to a permanent position, are already in the different path even *before* the transition takes place, i.e. when all workers are still in a fixed-term contract. These patterns in the raw data point to a dynamic selection problem.

To study these differences in earning trends more formally, we adopt an event-study design. For each worker in the data, we denote the precise month in which the individual

[9] One alternative that Garcia-Louzao et al. (2023) implement later on is to instrument experience and tenure using their deviations relative to the contract-specific averages, thereby aligning with each worker's history. Additionally, they exploit supplementary instruments based on regional variations in the availability of subsidies for hiring workers under open-ended contracts (OECs). In this paper, we leverage another form of variation using precise high-frequency data available in Spanish administrative records.

[10] Card et al. (2023) conducts a similar exercise to argue about the causal effect of places. The authors show that earnings are quite stable before workers move to a new commuting zone and that the trajectories only differ *after* changing locations.

Figure 2: Evolution of earnings: transitioning to a new contract



Notes: Median (ln) earnings for workers that transition to either open-ended or fixed-term contracts. We follow workers 15 months before and after transitioning to a new contract.

ends a temporary contract by $t = 0$ and index future and past months relative to that moment. We use the last complete month in the old contract ($t = -1$) as our base period. After the contract ends, we categorize workers based on their future type of contract, distinguishing workers transitioning from an FT to an open-ended contract (FT→OEC, $T_i = 1$) and workers transitioning to another FT contract (FT→FT, $T_i = 0$). Our baseline specification considers a balanced panel of workers whom we observe fifteen periods (months) before and after the event.[11] The event time t runs from -15 to $+15$. We denote by y_{ist} the log earnings of individual i, in year-month s and at event time t, and estimate the following regression:

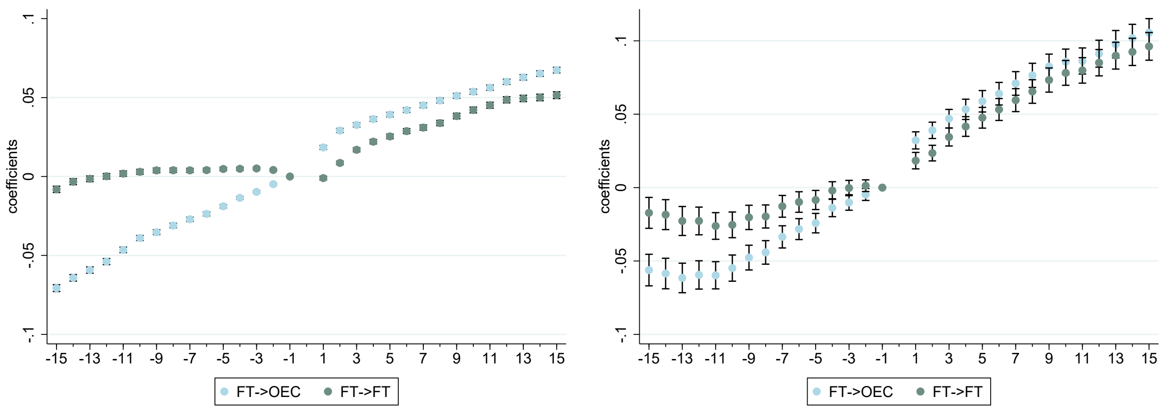
$$
\begin{aligned}
y_{ist} = & \sum_{j \neq -1} \alpha_j^T \cdot \mathbf{I}[j = t] \cdot \mathbf{I}[T_i = 1] + \sum_{j \neq -1} \alpha_j^{NT} \cdot \mathbf{I}[j = t] \cdot \mathbf{I}[T_i = 0] \\
& + \sum_k \beta_k \cdot \mathbf{I}\left[k = age_{is}\right] + \sum_p \gamma_p \cdot \mathbf{I}[p = s] + \lambda \mathbf{I}[T_i = 1] + \nu_{ist},
\end{aligned}
\tag{2}
$$

where we include a complete set of event time dummies (first term and second term on the right-hand side), age dummies (third term), and year $\times$ month dummies (fourth term). As we omit the event time dummy at $t = -1$ from the estimation, the event time coefficients measure the impact of moving into a new contract relative to the earnings just

[11] We allow for short unemployment spells occurring before or after the transition but only consider periods with non-zero earnings. Consequently, event time may differ from calendar months due to this flexibility in workers' restrictions.

before the termination of the previous fixed-term contract. By including a complete set of age dummies, we control non-parametrically for underlying life-cycle trends. We also control non-parametrically for time trends such as business cycle variation, including a full set of time dummies. Including age dummies in the comparison is important because workers in open-ended positions tend to be older than workers in temporary positions.

Figure 3: Earnings consequences from transitioning to OEC or FTC



(a) No controls

(b) Additional controls

Notes: The figure shows event time coefficients estimated from Equation **??** for workers transitioning to OEC and FTC. These coefficients are derived from a balanced sample of workers observed between January 1998 and March 2020. The analysis tracks workers for 15 months before and after contract change, accounting for short unemployment spells between contracts. Therefore, the periods observed do not necessarily align with calendar months. The base category is $t = -1$, and each specification controls for age (measured annually) and year-by-month fixed effects. Panel (a) presents our baseline specification. Panel (b) incorporates additional interactions of event time with education attainment and sector-fixed effects.

Results are presented in Figure 3. Panel (a) controls for the full set of time and age dummies discussed above. Additionally, Panel (b) also accounts for interactions between event time and worker's education attainment and sector, accounting for earnings growth explained by differences in observable characteristics. We would expect that workers face a differential earnings path after event period 0, as temporary contracts may be subject either to earnings penalties or premia (Albanese and Gallo 2020; Kahn 2016), and because returns to experience depend on contract type. However, this formal specification confirms that earnings evolve differently even *before* workers start their new contract: those workers who subsequently switch into open-ended contracts enjoy *much* faster earnings growth than those who do not, even while both groups are still in fixed-term contracts. The finding of higher wage returns among workers with more open-ended work experience in the Mincerian regressions, therefore, reflects this difference in worker selection. The difference in earnings growth between worker types is much more pronounced before any transitions to open-ended contracts take place.

6 Identification

To deal with the endogeneity of upgrades into permanent positions, we propose an instrumental variable strategy. As an exogenous source of variation, we combine individual variation in the expiration date of a fixed-term contract and transitory fluctuations in the opening of new open-ended jobs over time and space (i.e., variation in their arrival rate). We exploit that workers face greater chances to find a permanent position if there is a spike in permanent openings in the labor market just before their contract expires. This affects promotion probabilities in direct and indirect ways: in the most direct channel, workers have greater chances to land a permanent job inside or outside their current firm as more permanent openings become available. Moreover, other workers might switch to a job in a new firm, creating vacancies that could be filled by promoting fixed-term workers whose contract is about to end.

Exploiting the high frequency of our data, we can precisely match the month when the individual's fixed-term contract is about to end with the job openings at the regional level that exact month. We argue that facing more job openings precisely in the month a contract is about to end is as good as random for the worker, conditional on time (year) and seasonal (month) fixed effects. The approach is conceptually related to previous work on the effects of macroeconomic conditions at labor market entry ("graduating into a recession", e.g. Kahn 2010; Hershbein 2012; Wachter and Bender 2006; Altonji et al. 2016 or Schwandt and Von Wachter 2019) or compositional changes in labor demand (Arellano-Bover, 2024). However, while previous work considers yearly fluctuations in labor demand, we exploit high-frequency information in administrative employment spells to abstract from general business cycle conditions.

Specifically, using a leave-one-out approach, we estimate the following first-stage equation:

$$p_{it+1} = \sum_{k=-24}^{24} \alpha_k logOEC_{-i,t+k,r} + X_{it}\theta + \epsilon_{it}, \tag{3}$$

where t refers to monthly periods relative to each worker's last month in a fixed-term position (at t). Thus, p_{it+1} indicates whether the worker starts an open-ended contract in $t+1$, after their current fixed-term contract ends. The variable $logOEC_{-i,t+k,r}$ is constructed as the sum of all new open-ended positions in period $t+k$ in worker i's initial province of residence, leaving out individual i herself. Therefore, we allow promotions to depend on the total number of new open-ended contracts in period t and leads and lags of this variable, excluding individual's i promotion in the calculation. The first lead, $logOEC_{-i,t+1,r}$, is our instrumental variable. As we control for year-fixed effects as well as month-fixed effects, the instrument $logOEC_{-i,t+1,r}$ captures regional fluctuations in the supply of new open-ended contracts that are as good as random from the perspective of the worker ("instrument independence"). We provide evidence to support this assumption below. Under our identification assumptions, we would expect the effect of this first lead, captured by coefficient α_1, to be the strongest predictor of an individual's probability to switch into a permanent position. The coefficients on other leads and lags (α_k for $k \neq 1$) should be smaller in magnitude but might be non-zero, as they capture general business cycle conditions that might affect promotion probabilities or wage growth.

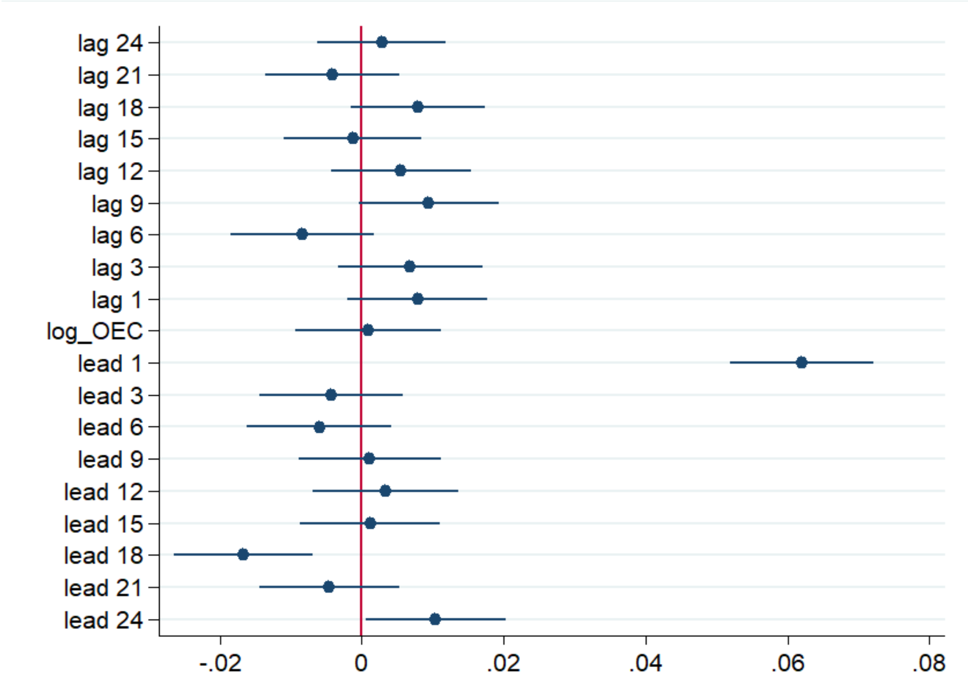
The inclusion of leads and lags of the instrument serves two purposes. First, to illustrate that transitory fluctuations matter if they hit a worker in exactly the month her previous contract runs out, i.e. to show that the first lag has strong predictive power even conditional on a complete set of other leads and lags (instrument relevance). Second, these other leads and lags control for general business cycle conditions, which would violate the instrument exclusion restriction. To further partial-out the effect of the business cycle and seasonal variations in job openings, we add an extensive set of controls, including leads and lags of the total number of new fixed-term contracts, as well as year, month, province, and sector fixed effects. The last two are defined at the baseline period (i.e. the last month employed in a fixed-term contract). At the individual level, X_{it} accounts for gender, overall experience, experience squared, and interactions of age categories with education attainment.

The results from this regression are presented in Figure 4. As expected, the effect of the first lead of new permanent positions stands-out strongly. Consistent with our identification strategy, we find that the openings of new open-ended contracts when the worker's contract expires are the strongest predictor of the probability of finding a permanent position immediately after. Moreover, the absence of strong correlations with the rest of the leads and lags indicates that the instrument is capturing the effect of transitory shocks on job market matches, as opposed to general business cycle conditions.

Figure 4 depicts the leads and lags in the number of new open positions on the *regional* level. We can apply the same logic to exploit new openings of permanent positions at the national or industry level instead. As shown in Figure A.2.1 in the Appendix, we find similar patterns in these alternative specifications. The instrument is, therefore, relevant, irrespective of whether we measure it at the national, regional (baseline), or industry level. These findings are robust to excluding from the dataset months of potentially high job-seasonality (see Appendix A.3). Furthermore, the instrument is also likely to satisfy the monotonicity condition, as the opening up of more permanent positions is unlikely to decrease the chances of promotion for any worker.

We argue next that the instrument also satisfies the independence assumption and

Figure 4: The effect of new open-ended contracts on contract upgrade probability



Notes: The sample is restricted to workers in the last month of a fixed-term contract of at least 0.8 years of tenure but less than 1.2 years. Coefficients of the probability of being promoted to an open-ended contract in $t+1$ on leads and lags of the log of new open-ended contracts by month. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

exclusion restriction. The instrumental variable identifies, therefore, the labor market consequences of entering a permanent contract for "compliers", i.e., workers who find a permanent contract only if the local labor market conditions are sufficiently favorable. This local average treatment effect (LATE) may differ from the returns to contract type for another type of worker but is a parameter of high policy relevance – it is precisely those marginal workers who would be affected by policy changes that affect the relative provision of open-ended vs. fixed-term contracts on the labor market.

6.1 Instrument exogeneity

Instrument independence

We argue that the number of new permanent positions in the market when the worker's contract is about to end is exogenous from the worker's perspective or as good as randomly assigned. While we cannot test it directly, to strengthen the validity of the independence assumption, we evaluate whether the instrument correlates with observable individual and firm characteristics. The results are provided in Appendix Figure A.4.1. Each panel presents the coefficients from a regression of $logOEC_{-i,t+1,r}$ on worker and firm characteristics at time t, with additional controls for year, month, and region fixed effects, along with leads and lags of $logOEC_{-i,t+1,r}$— mirroring the approach adopted in the first stage. Panel (a) reveals no significant relationship between the number of open-ended contracts in the period leading up to the termination date of a worker's fixed-term contract and the worker's characteristics. Likewise, Panel (b) explores the correlation between the worker's employment sector at time t and the number of open-ended contracts in the following period. Although the correlation between the number of open-ended contracts and being employed in extractive activities and the construction sector is statistically significant, the effect is small. Finally, Panel (c) examines the correlation with specific firm characteristics, specifically the firm's age and size, categorized as very small, small, medium, large, and very large. Again, the correlation between these dimensions and the count of new open-ended contracts is negligible. The results suggest that the degree of selection in this setting is rather limited and can be accounted for by sectoral firm and regional characteristics.

Exogeneity of contract termination date

Our identification strategy relies on two key elements: fluctuations in the opening of new open-ended contracts and the exact timing of the expiration of the worker's fixed-term contract. One potential concern is a direct link from the former on the latter, i.e. whether the number of newly opened permanent contracts would affect the termination date for some workers. For instance, a recurrent renewal of fixed-term contracts from firms as they await for economic conditions to change could create such link.

Two key aspects alleviate this potential concern. First, at the time of the analysis, there were legal limitations regarding the sequential renovation of temporary contracts.[12] Specifically, the workers' statute stipulated that after 24 months of temporary employment within the same firm or group of firms, individuals would be entitled to permanent worker status.[13] As a result, employers were subject to restrictions regarding extending and renewing a worker's fixed-term contract, limiting such arrangements to a maximum duration of two years.

Second, our sample restrictions. To conduct our analysis, we focus on workers who are about to end a fixed-term contract. A large proportion of contracts were stipulated to last one year, after which the contract could be renewed within the legal limit. Appendix Figure A.1.2 corroborates this fact. By assessing the distribution of contracts' duration,

[12]We conduct the study using data up to February 2020, before the December 2021 labor reform.

[13]According to Article 15.5, *"employees who, within thirty months, have been employed for a term exceeding twenty-four months, with or without continuity, in the same or different job positions with the same company or group of companies, through two or more temporary contracts, either directly or through placement by temporary employment agencies, with the same or different types of fixed-term contracts, will acquire the status of permanent employees."* The 2021 reform reduced the thirty and twenty-four months periods to twenty-four and eighteen, respectively.

we can observe that the largest fraction of contracts effectively ended after a year. We restrict the sample to those temporary workers with tenure ranging from 0.8-1.2 years when observed. This approach aims to avoid potential manipulations of the termination date. Likewise, it excludes extremely brief work contracts, which are common in our context (Bentolila et al. 2020).

7 Results

7.1 Reduced-form evidence

We showed that regional fluctuations in the opening of new contracts generate exogenous variation in the probability that workers transition from a fixed-term to a permanent position. This section employs a reduced-form approach to investigate how such promotion opportunities impact workers' labor market outcomes in the short and long term.

For this analysis, we limit the sample to individuals holding contracts that are about to expire. Specifically, we focus on workers who are in the last month of a fixed-term contract and examine their outcomes up to 60 months before and after this period. Table B.1.1 provides descriptive statistics on demographic characteristics alongside our instrument. Employing this group of workers, we proceed to estimate the following reduced-form equation:

$$y_{it+h} = \sum_{k=-24}^{24} \alpha_k logOEC_{-i,t+k,r} + \mathbf{X}_{it}\theta + \epsilon_{it}, \tag{4}$$

where y_{it+h} is the worker's i outcome in period $t+h$, with $h = -60, \ldots, 60$. Each outcome is studied up to 60 months before and after fixed-term contract expiration – which occurs at month t for each worker – allowing us to explore the long-term effects of contract type and to verify that workers had similar career trajectories in the pre-treatment period. We include 24 leads and lags of the *log* of new open-ended contracts ($logOEC$) in region r relative to the last month of the worker's current fixed-term contract. To control for business cycle variation and job creation seasonality, we also include the same number of leads and lags of the *log* total number of new fixed-term contracts in region r denoted by ($logFTC$). In addition, we add individual and regional controls including year, month, province, and sector fixed effects, overall experience and experience squared (measured at baseline), gender, and interactions of age categories with education attainment.

We can go further and control for business cycle variation more aggressively by additionally controlling for the aggregate leave-one-out average of the outcomes, $\overline{Y}_{-i,t+h,r}$, as in

$$y_{it+h} = \sum_{k=-24}^{24} \alpha_k logOEC_{-i,t+k,r} + \sum_{k=-24}^{24} \gamma_k logFTC_{-i,t+k,r} + \delta \overline{Y}_{-i,t+h} + X_{it}\theta + \epsilon_{it}, \qquad (5)$$

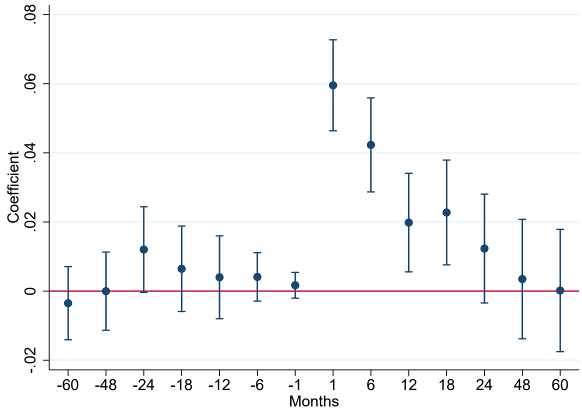
we construct $\overline{Y}_{-i,t+h}$, based on the full sample of workers, irrespective of the timing of their contract expiration date (i.e., there is no mechanical link between y_{it+h} measured for recently hired workers and $\overline{Y}_{-i,t+h}$ measured for all workers).[14] This control further ensures that economic conditions are held constant, such that our instrument only captures atypical variation in the availability of open-ended positions that are uncorrelated with general business-cycle trends.

In terms of outcomes, we first evaluate earnings growth, measured as the ratio between each earnings at $t + h$ and the monthly earnings at the baseline period t: i.e., during the last month of the contract before expiring. Thus, the coefficients capture the effect on workers' outcomes compared to their last contract before switching to a new (fixed-term or open-ended) position. In terms of employment, we evaluate employment status, the probability of being employed in an open-ended contract, and cumulative experience in open-ended contracts measured in months. Furthermore, we study mobility responses, examining transitions into alternative sectors and regions. The results of the baseline specification described in equation 4 are illustrated in Figure 5.

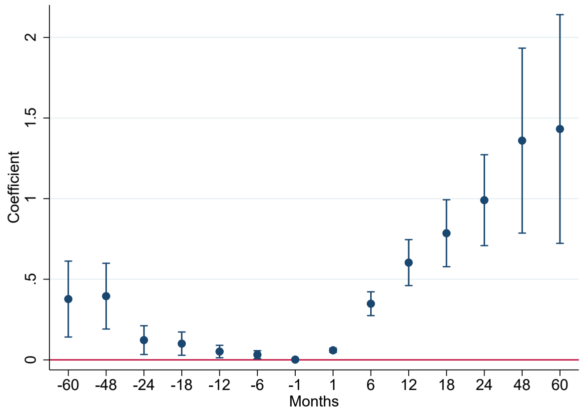
[14]For example, when studying wage effects then y_{it+h} captures the individual wage growth of workers in our analysis sample (workers whose fixed-term contracts ended at time t) between the end of their fixed-term contract in period t and the period $t+h$, while $\overline{Y}_{-i,t+h}$ would capture the growth in wages of all workers during that same period (irrespective of the timing of their contract end and start dates).

Figure 5: Reduced-Form Evidence

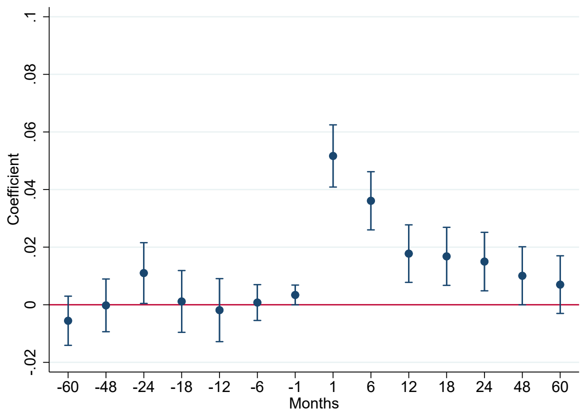
(a) Earnings



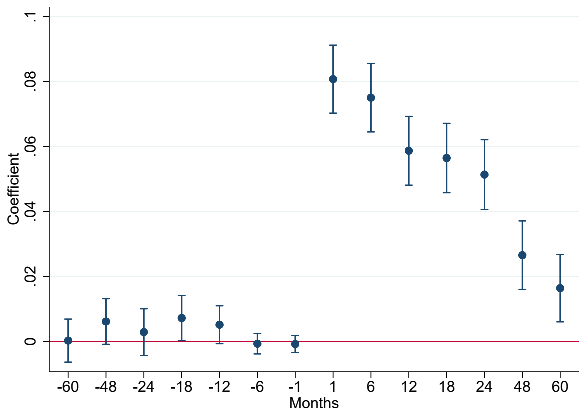
(b) Cumulative Earnings



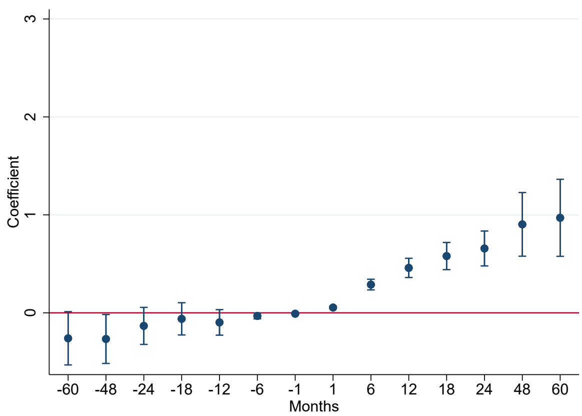
(c) Employment



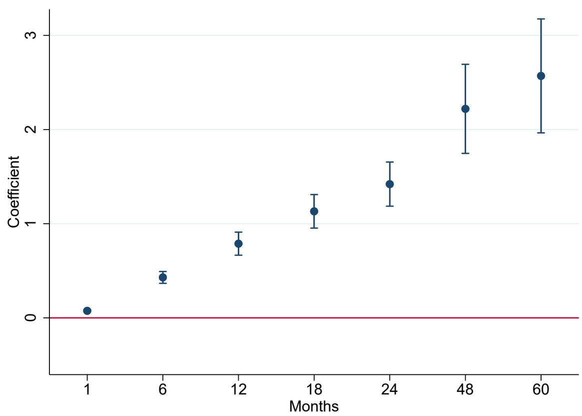
(d) OEC Status



(e) Experience



(f) OEC Experience



Notes: The sample is restricted to workers who held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline and were in the last month of a fixed-term contract between 1998-2017. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as the log of total new contracts. We also control for the mean of the outcomes at the time for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

7.1.1 Earnings

Panel (a) in Figure 5 presents the long-term effects of promotion opportunities on workers' earnings. Specifically, we estimate equation (4) separately for different event periods h, and then plot the coefficients on our instrument $logOEC_{-i,t+1,r}$. As shown in this figure, we find a sharp and large positive effect on workers' earnings in event period $h = 1$, i.e. one month after being exposed to better employment opportunities. As illustrated in the figure, a 10 percent increase in the number of permanent contracts raises the wage of exposed workers by 0.5 percent. This effect dissipates over time, although the earning gains remain positive for two years after exposure. As we show in the next section, both the sharp increase in earnings at event period $h = 1$ and the subsequent eroding of this earnings advantage is explained by the impact of promotion opportunities on employment trajectories.

Interestingly, the impact on earnings diminishes over time. After five years of exposure, the point estimate is close to zero and not statistically significant. This reduction is mechanic to some extent. A fraction of workers who were not *lucky* at $t = 0$ and remain in fixed-term contracts may eventually attain a contract upgrade after several years. Consequently, the disparity between those promoted at $t = 0$ and the remainder diminishes over time, explaining partially the observed effects. In the next sections, we examine other mechanisms that explain further this decline.

7.1.2 Employment

Figure 5 also shows the effects of contract upgrade opportunities on employment trajectories. As illustrated in panel (c), the effect of enhanced opportunities to switch to an open-ended contract translate into a higher probability of employment in the short run. The probability to be employed increases sharply in event period $h = 1$, with a 10 percent increase in the number of permanent contracts raising employment by 0.5 percentage points. This effect size is similar to the corresponding effect on wages (panel a). As shown in panel (c), the effect on the probability to work in a permanent contract increases by 0.8 percentage points.

As for wages, these employment effects dissipate over time. A "lucky draw" in promotion opportunities provides thus only a temporary boost, and has no long-term consequences on employment and promotions.[15] But while the employment effects are temporary, they have a lasting effect on work experience. As shown in panels (e) and (f), workers exposed to favorable contract-upgrade opportunities accumulate more work experience,

[15] This finding is in line with the observation that the sorting of workers to contracts is highly selective.

in particular in open-ended contracts.

As mentioned above, the overall pattern in employment – with sharp initial gains that then decrease over time – resembles the corresponding effects on wages. However, the employment effect diminishes less rapidly over time than the effect on wages: while the wage effects turn negative after only two years, the employment effects remain positive for four years. The pattern in employment can therefore explain the sharp initial gains in earnings, but not why the impact on earnings eventually fades away. We next study whether other margins might explain the null long-run effect on earnings.

7.1.3 Sectoral and Regional Mobility

An evident key advantage of permanent versus temporary contracts is job security. The prospect of higher stability is also reflected in workers' mobility decisions. Figure 6 analyzes this margin by looking at the effect of enhanced contract upgrade opportunities on the probability of moving to a different sector or region over time. These outcomes are measured via indicator variables that take a value of one if the worker has moved after ending her current fixed-term position, i.e. between period t and t+h. As shown in panels (a) and (b) we find a reduction in the likelihood of changing sectors and relocating to a different region. These effects persist in the long-run. The results suggest that the security and satisfaction derived from permanent employment may influence workers' choices, potentially acting as a deterrent against pursuing alternative career paths. These findings prompt an alternative potential explanation for the null long-term response in earnings that we documented earlier. While stability is a desired job characteristic for most workers, one may wonder whether it could come at the expense of career flexibility and progression. In the next section, we quantify the impact of actually switching to an open ended contract in a 2SLS specification.

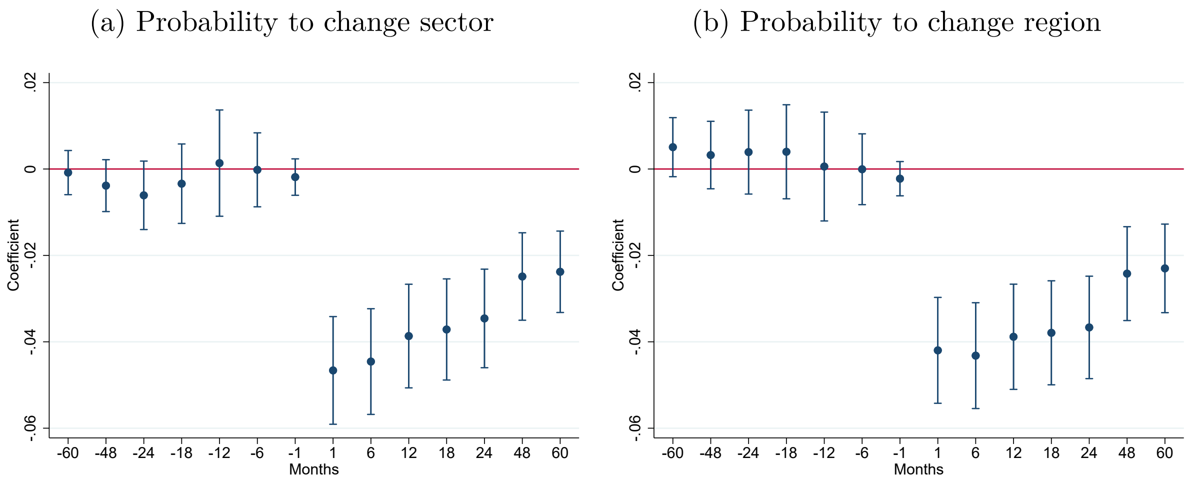
7.2 IV estimates

We now shift from the reduced-form analysis and provide 2SLS estimates of contract upgrade. Exploiting the availability of open-ended contracts at the expiration time of a worker's fixed-term position, we instrument permanent contract status in the following IV model:

$$y_{it+h} = \beta p_{i,t+1} + \sum_{k\neq 1, k=-24}^{24} \alpha_k logOEC_{-i,t+k,r} + X_{it}\theta + \epsilon_{it}, \tag{6}$$

where $p_{i,t+1}$ is an indicator that takes a value of one if the worker switches to an open-ended contract in $t+1$, after terminating its current fixed-term position. The dependent variable y_{it+h} is the worker's i outcome in period $t+h$, with $h = 1, \ldots, 60$. As before, we

Figure 6: Effect of OEC regional shock on workers' mobility



Notes: Baseline sample restrictions and empirical specification are described in Figure 5 notes.

study each outcome up to 60 months after fixed-term contract expiration – which occurs at month t for each worker – allowing us to explore the long-term effects of contract type. Again, we include 24 leads and lags of the *log* of new open-ended contracts ($logOEC$) relative to the last month of the worker's current fixed-term contract, along with the set of controls described in the reduced-form analysis in Section 7.1. As mentioned before, our instrument is the first lead of the number of open-ended positions in the province r where the worker was located: $\log OEC_{-i,t+1,r}$.

Table 2 summarizes our findings looking at earnings, employment, and mobility outcomes in the short and long run. Panel A displays short-term effects (12 months after ending their current fixed-term contract), and Panel B long-term effects looking at outcomes 5 years in the future. Qualitatively, the dynamics are in line with our reduced-form findings. In the short-run, we observe a notable increase in earnings and employment probability for workers transitioning into open-ended contracts. The results are in line with what we find in the reduced-form analysis. We can observe that soon after contract upgrade, earnings growth face a positive impact. In particular, we observe that switching to a permanent position translate into a 25% increase in earnings. However, the effect is not long-lasting, dropping to close to zero effect after five years. Still, in terms of cumulative earnings growth workers retain a considerable advantage, likely attributable to the initial *boost* in wages and employment effects. In particular, we observe that workers are 23 percentage points more likely to be employed after one year (around 31% of the mean), and 10 percentage points more likely after five years, albeit the effect is not statistically significant.Likewise, we observe that workers accumulate more experience overall, with a little bit more than one year of advantage when we observe them five years after having switched to a permanent contract.

Regarding mobility, we can observe substantial and persistent negative effects. After finding a permanent contract, the probability of changing region during the first year drops by 72 percentage points and by 42 percentage points within 5 years (around 57% of the average likelihood). Relative to the mean, the effects on sectoral mobility are slightly less pronounced. After finding a permanent contract, the probability of switching to a new sector within the first year drops by 70 percentage points and by 40 percentage points within the first 5 years, about half of the average sectoral mobility rate.

Table 2: Effect of Open-Ended Employment on Earnings and Employment Outcomes

Panel A: Short-term effects (12 months)						
	(1)	(2)	(3)	(4)	(5)	(6)
	Earnings	Cum. Earnings	Employment	Experience	Change Region	Change Sector
$p_{i,t+1}$	0.254***	7.720***	0.234***	6.105***	-0.723***	-0.704***
	(0.091)	(0.885)	(0.064)	(0.574)	(0.061)	(0.056)
Obs.	199,155	199,155	199,155	198,852	199,155	199,155
R2	0.086	0.171	0.127	0.997	0.328	0.427
Panel B: Long term effects (60 months)						
	(1)	(2)	(3)	(4)	(5)	(6)
	Earnings	Cum. Earnings	Employment	Experience	Change Region	Change Sector
$p_{i,t+1}$	0.002	18.325***	0.103	13.035***	-0.424***	-0.404***
	(0.115)	(4.508)	(0.066)	(2.425)	(0.059)	(0.053)
Obs.	199,155	199,155	199,155	192,525	199,155	199,155
R2	0.213	0.184	0.260	0.944	0.208	0.200

Notes: The table reports IV estimated coefficients based on Equation 6. The sample restrictions and controls are the same as in the reduced form exercise reported in Figure 5 notes. Robust standard errors in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

8 Robustness checks

Exclusion restriction

In this subsection, we provide further evidence that our instrument does not have a systematic relationship with economic conditions. As mentioned earlier, a key aspect of our identification strategy relies on whether our instrument reflects random fluctuations orthogonal to the business cycle. While we exhaustively control for economic trends and cycle conditions in our model via i) time fixed effects ii) leads and lags of our instrument iii) leads and lags of all new contracts and iv) leave-one-out average outcomes; one could still be concerned about unobserved factors challenging our identification assumptions. For instance, one might wonder whether the large positive employment effects reflect a strong economic environment and therefore, a violation of the exclusion restriction. If this was the case, we would expect a positive effect on employment irrespective of contract transition.

To alleviate this concern, we run the following placebo test. Similar to the specification on 4, we evaluate the reduced-form effect of our instrument on employment restricting the sample to workers that remain in a temporary worker status after their fixed-term contract ends, i.e.: those that switch from a fixed-term to a new fixed-term contract. If our instrument reflected economic conditions, we would also expect a positive impact on employment for these workers too. Appendix Figure A.7.1 illustrates that there is no significant nor systematic employment response, ruling out this possibility.

Tenure

As described in section 6.1, we impose a tenure window surrounding one year to select workers who ended their fixed-term contract at the stipulated original duration. While one-year is the most common contract duration, we also observe a non-negligible concentration on contracts that lasted 6 months (or 0.5 years). In addition, some contracts could be extended up to two-years. In Appendix A.6 we repeat the reduced-form analysis relaxing the duration restriction by widening the tenure window to 0.4 to 2 years. Although the estimates become somewhat noisier, we can still observe a suggestive positive effect on earnings growth along with an increased likelihood of employment.

9 Conclusion

The matching of workers to firms, jobs and contract types has important implications both for individual careers and aggregate outcomes. However, it is difficult to provide causal evidence on this question, as workers may sort non-randomly into jobs. The key challenge is to disentangle whether differences in career trajectories are due to unobserved heterogeneity on the supply side or whether they reflect true causal effects from characteristics of the labor market.

By examining the Spanish context as a case study, we investigate how different types of contracts affect workers' careers. Consistent with recent evidence by Garcia-Louzao et al. (2023), workers who spent more time in fixed-term contracts experience lower earnings growth than workers who spent time in open-ended positions. Nevertheless, differences in earnings growth may reflect not only differences in returns between contract types but also heterogeneity among employees.

An event study graph reveals suggestive evidence of the absence of "parallel pretrends", which is crucial to distinguish these explanations. The earnings trajectories of workers who switch from fixed-term to open-ended contracts differ even before the termination of their original contract. The difference is sizable: while the earnings of workers switching to an open-ended contract grow, on average, by 5% in the year before the switch, earnings growth is negligible for workers who switch to another fixed-term contract instead. Next, we provide an alternative to fixed effects methods widely applied in this literature.

We propose a novel identification strategy to address selection bias stemming from the non-random sorting of workers into jobs. Using rich matched employer-employee data, we isolate quasi-random variation in worker-firm matches by interacting high-frequency information on the duration of contracts on the supply side of the labor market and transitory fluctuations in job creation on the demand side.

We find that individual promotion probabilities and experience accumulation in permanent positions are highly correlated to transitory variation in the opening of permanent contracts. Moreover, we find sharp initial gains in terms of earnings growth an employment, however these effects dissipate in the long-run. Looking at sectoral and regional mobility patterns, we find that workers are considerably less mobile. While workers that switch earlier to a permanent position do accumulate more experience and aggregate earnings over time, it might be the case that the stability inherent to these contracts could come, to some extent, at the expense of career flexibility and progression.

The methodology we use is general, and not restricted to the dual labor market context.

The key idea is to exploit two advantages of administrative registers, namely their high frequency, such that we know when exactly a worker's contract ends, and their large size, such that we can measure fluctuations in local labor market conditions. As most administrative registers share those same advantages, our method is widely applicable to address (dynamic) selection in the matching between workers and firms, jobs and contracts on the labor market.

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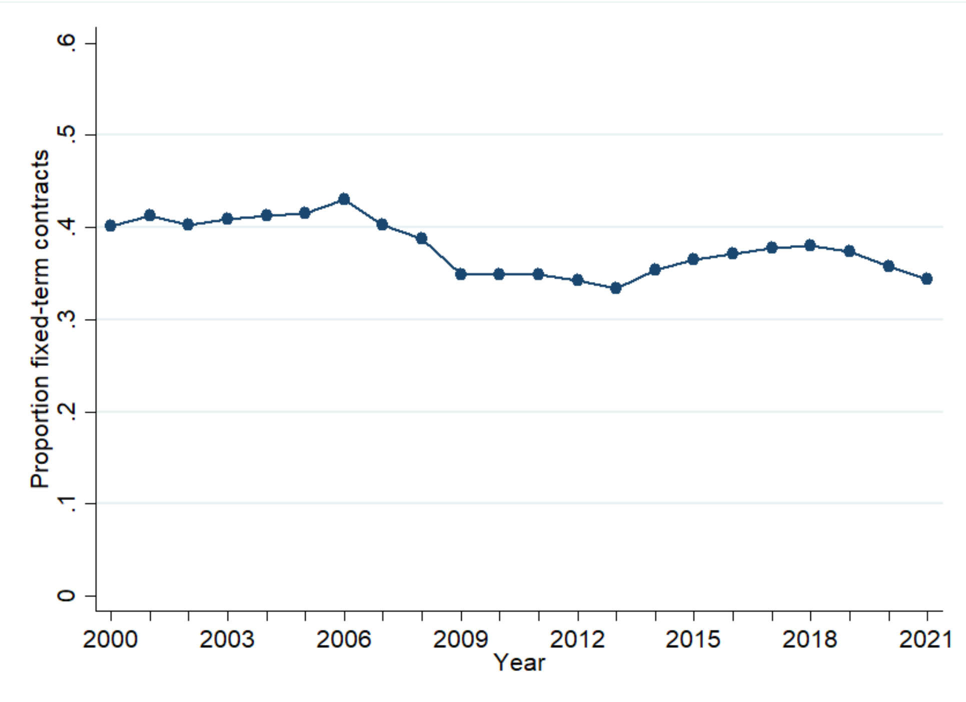
Appendix

A Supplementary Figures

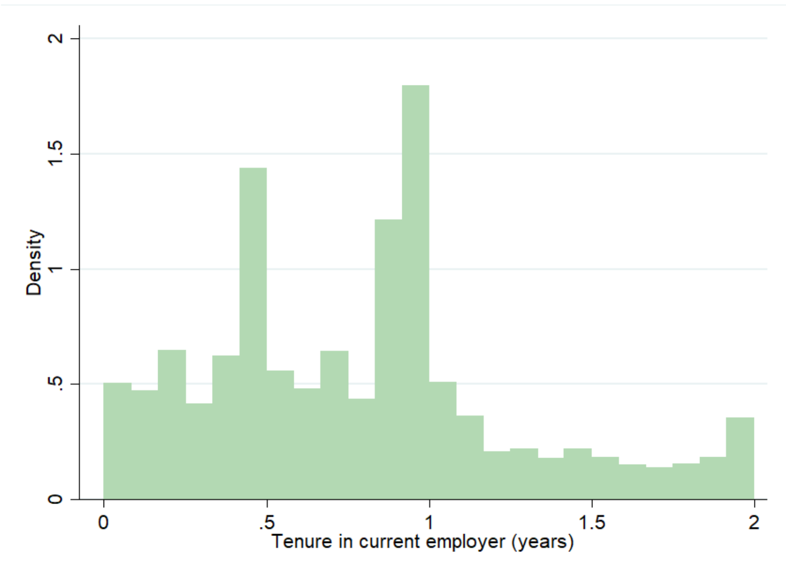
A.1 Fixed-term contracts in Spain

Figure A.1.1: Proportion of workers in fixed-term contracts, by year



Source: MCVL

Figure A.1.2: Maximum tenure at expiration fixed-term contracts

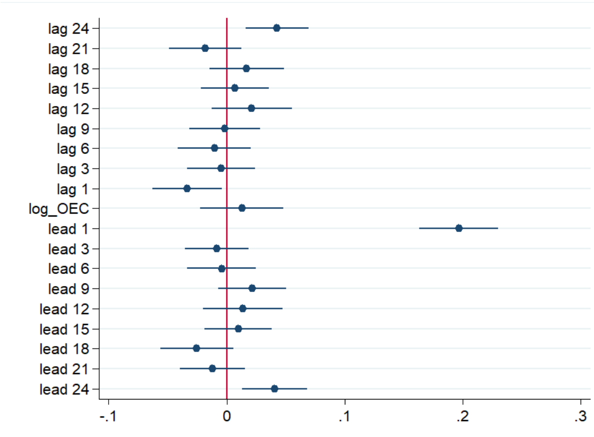


Notes: Distribution of maximum tenure in fixed-term contracts 1998-2021.

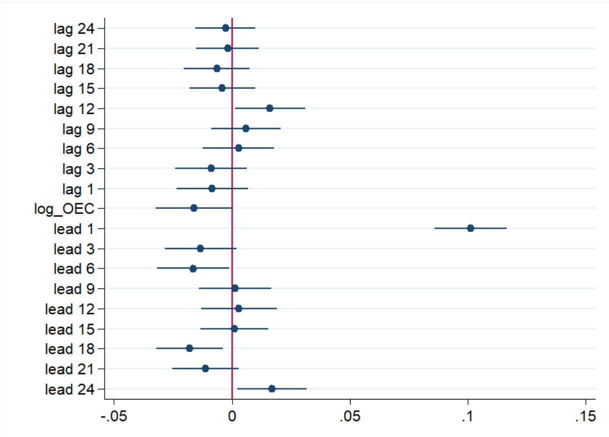
A.2 First Stage

Figure A.2.1: First stage: National, Sectoral and Sector x Region Instrument

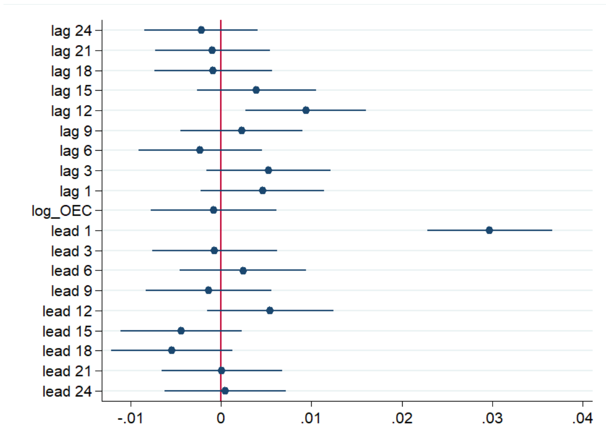
(a) National



(b) Sectoral



(c) Sector $\times$ Region

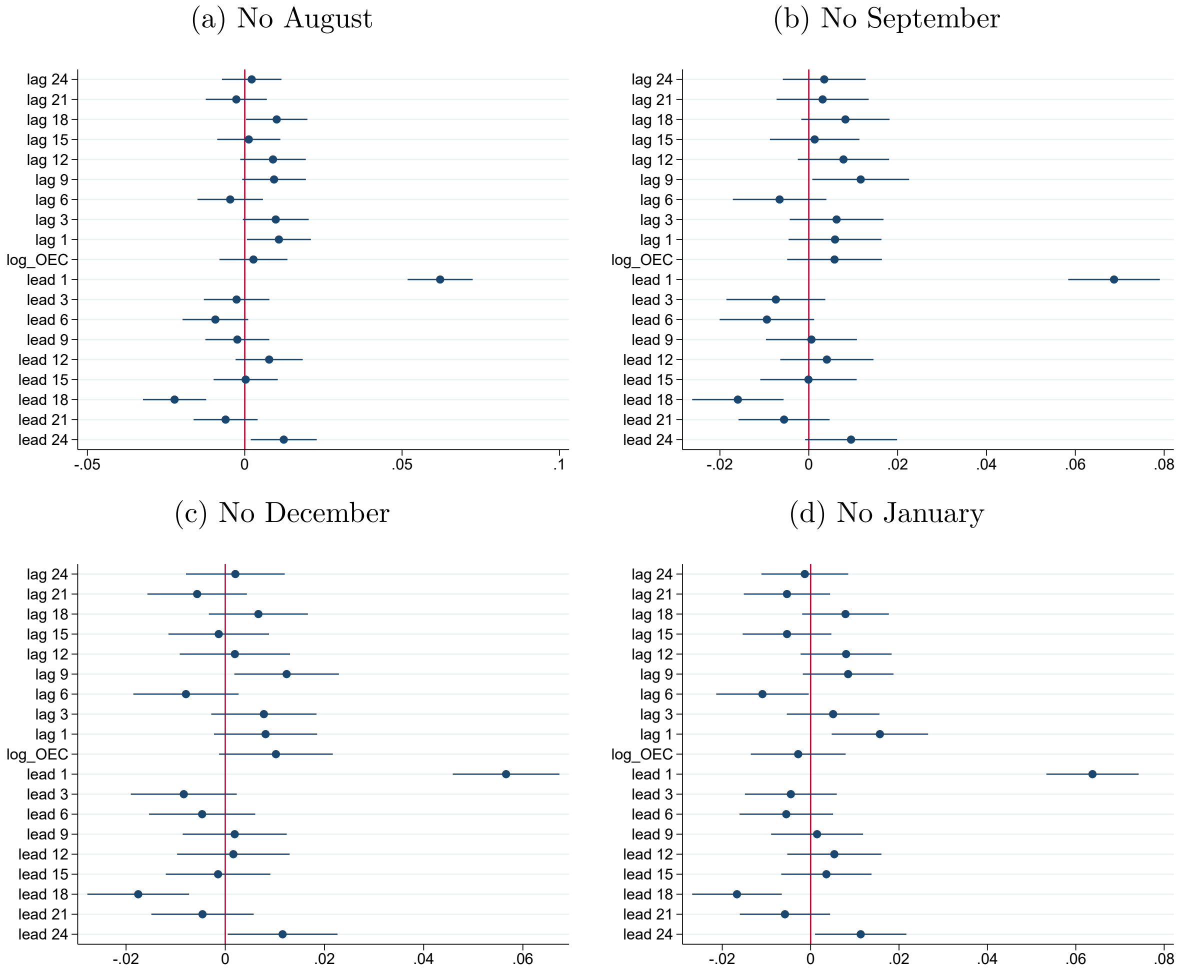


Notes: The sample is restricted to workers in the last month of a fixed-term contract with a tenure of at least 0.8 years but less than 1.2 years. Coefficients of the probability of being promoted to an open-ended contract in $t+1$ on leads and lags of the log of new open-ended contracts by month. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts. Panel (a) employs the variation in opening positions by month at the national level. Panel (b) the newly opened positions by month and sector. Panel (c) counts new open-ended positions by month, province, and sector.

A.3 Robustness: Job Seasonality

Figure A.3.1: Regional Instrument

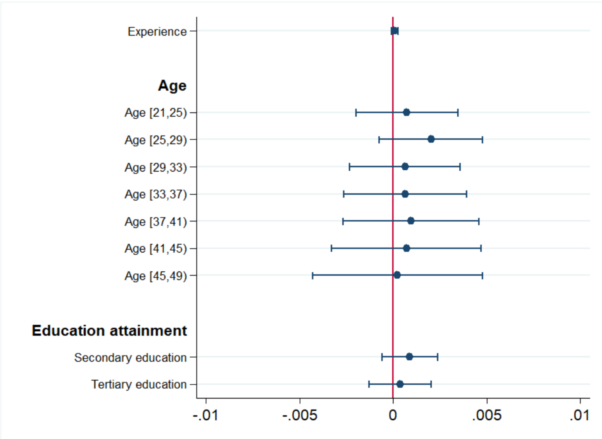
Removing specific months



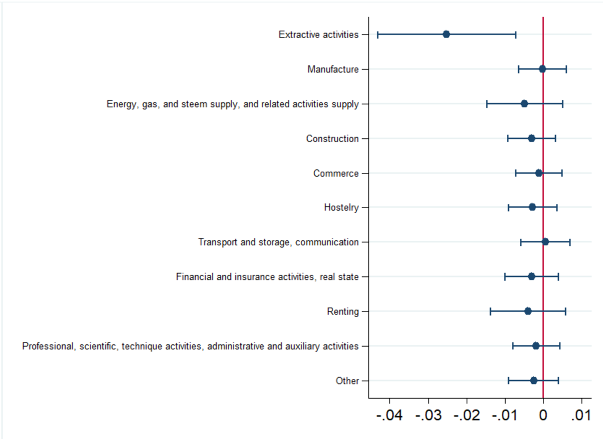
Notes: Baseline sample restrictions and empirical specification are described in Figure 4 notes.

A.4 Instrument Independence

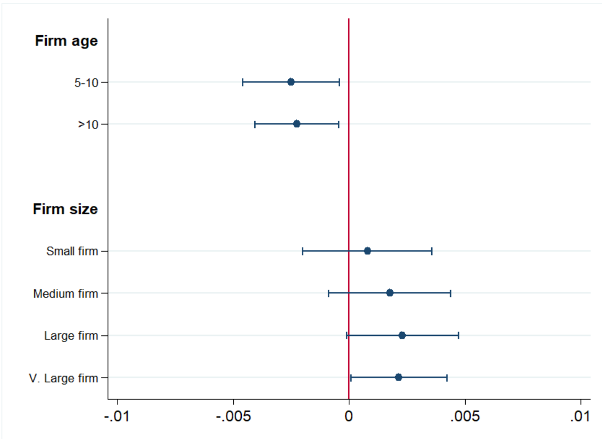
Figure A.4.1: Effect of individual characteristics and sector on $logOEC_{t+1}$



(a) Individual characteristics



(b) Sector



(c) Firm characteristics

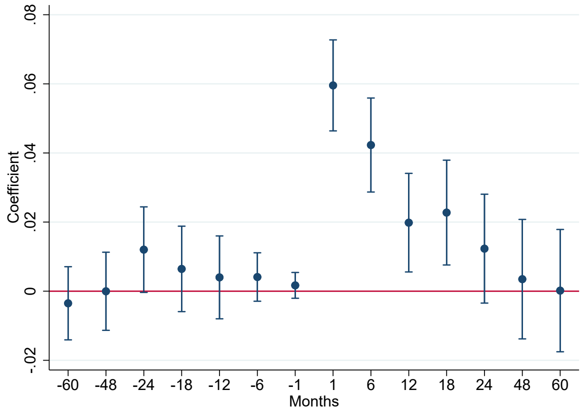
Notes: All regressions include leads and lags of logOEC, year, month, and province fixed effects. For this exercise we standardize the instrument.

A.5 Robustness: Reduced-Form Alternative Specifications

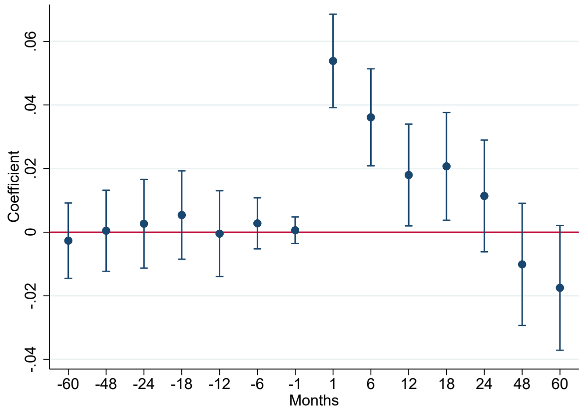
Figure A.5.1: Effect of OEC regional shock on earnings growth

Alternative Specifications

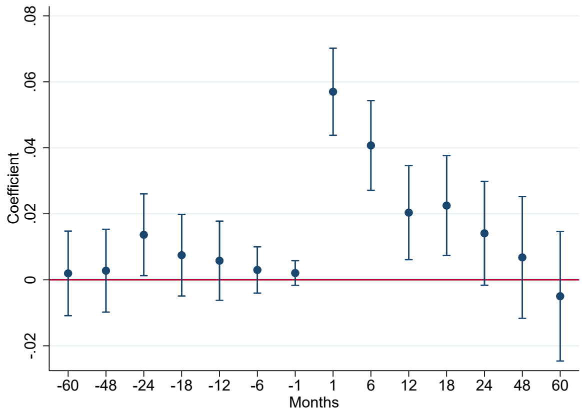
(a) Baseline



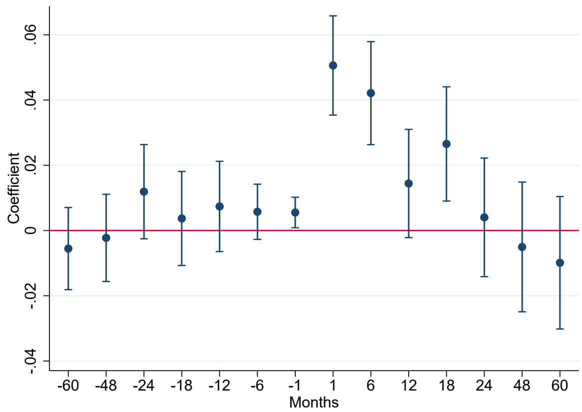
(b) Date FE



(c) Aggregate average earnings growth control



(d) Total new contracts control

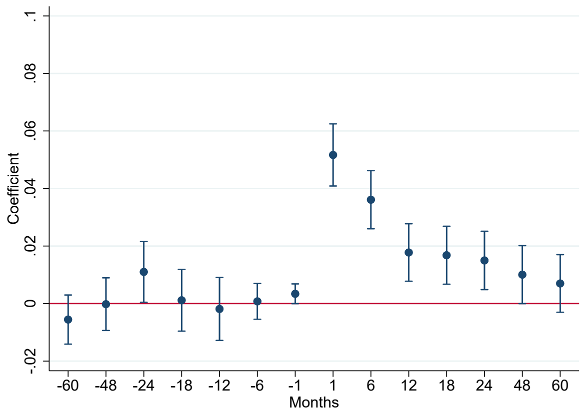


Notes: Baseline sample restrictions and empirical specification are described in Figure 5 notes.

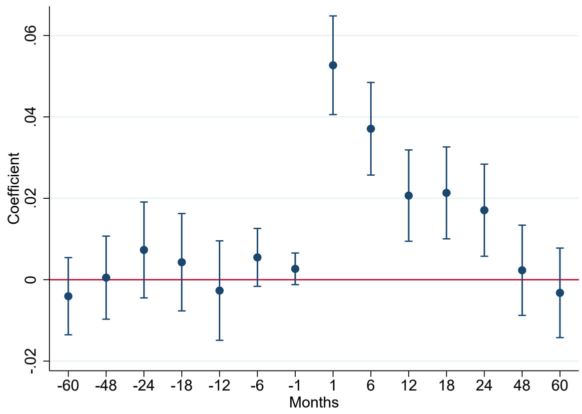
Figure A.5.2: Effect of OEC regional shock on employment

Alternative Specifications

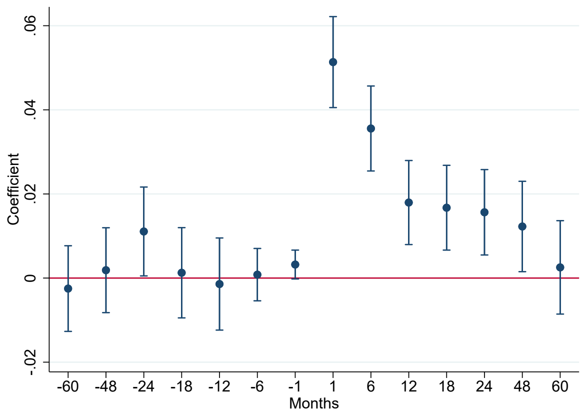
(a) Baseline



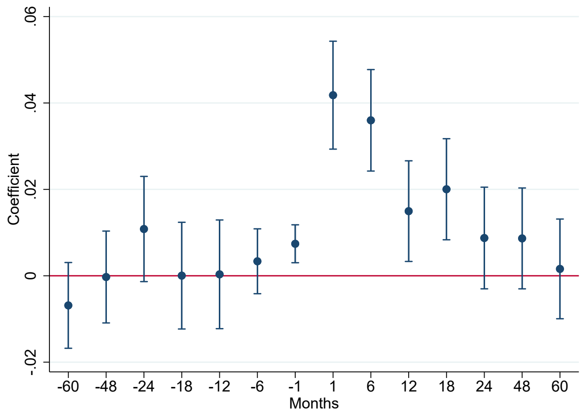
(b) Date FE



(c) Aggregate employment control



(d) Total new contracts control

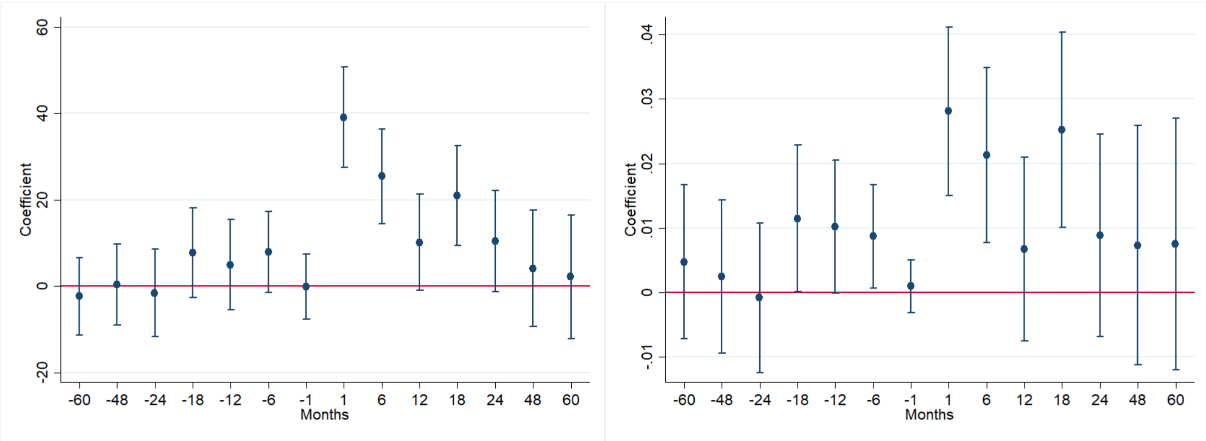


Notes: Baseline sample restrictions and empirical specification are described in Figure 5 notes.

A.6 Alternative Tenure Restrictions

Figure A.6.1: Effect of OEC regional shock on earnings. Tenure 0.4-2 years

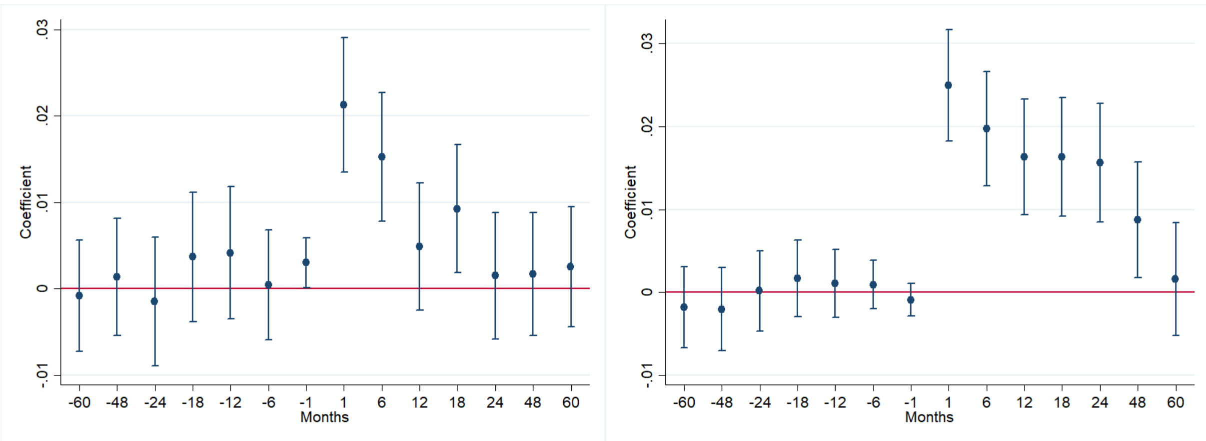
(a) Earnings (b) Earnings growth



Notes: The sample is restricted to workers who held a fixed-term position of at least 0.4 but less than 2 years of tenure at baseline and who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as the log of total new contracts. We also control for the mean of the outcomes at time t for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

Figure A.6.2: Effect of OEC regional shock on employment. Tenure 0.4-2 years

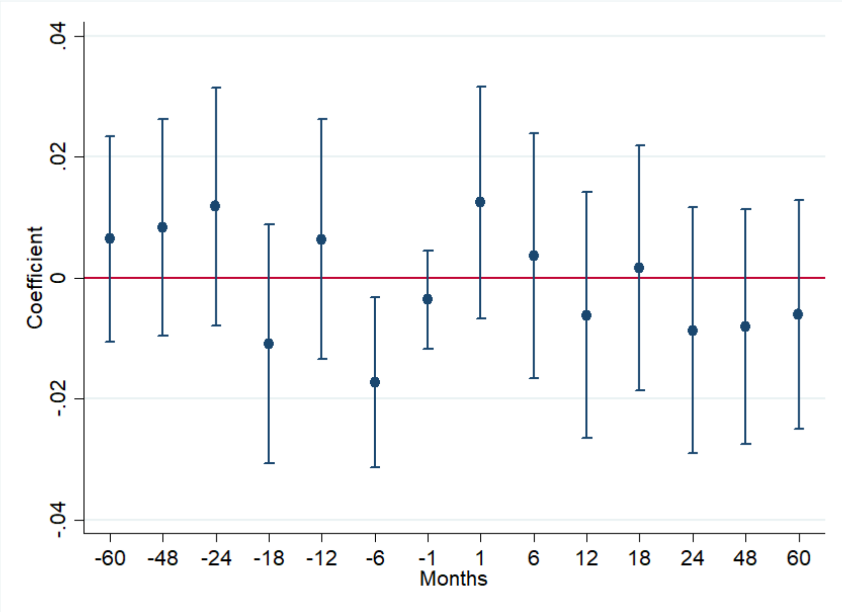
(a) Probability of employment (b) OEC Status



Notes: The sample was restricted to workers who held a fixed-term position of at least 0.4 but less than 2 years of tenure at baseline and who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as the log of total new contracts. We also control for the mean of the outcomes at time t for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

A.7 Robustness: Exclusion Restriction

Figure A.7.1: Effect of OEC regional shock on employment: restriction to fixed-term employment



Notes: The sample was restricted to workers who held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline and who were in the last month of a fixed-term contract between 1998-2017. The sample was also constrained to workers not in an open-ended contract in period $t + k$. The coefficients correspond to the effect of the first lead of OEC regional openings on employment probabilities. All regressions control for the leads and lags of logOEC as well as the log of total new contracts. We also control for the mean of the outcomes at time t for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

B Descriptive Statistics

B.1 Estimation Sample

Table B.1.1: Descriptive statistics of estimation sample

	Mean	**Standard Deviation**
Age	30.7	7.437
Female	0.435	0.495
Education		
Below secondary	0.558	0.496
Secondary	0.234	0.423
Tertiary	0.206	0.404
Tenure	0.970	0.091
Experience	6.635	5.372
Earnings (EUR 2009)	1,099.63	555.76
$\log OEC_{lead1}$	4.919	1.323
Observations		166.817

Notes: Descriptive statistics for the estimation sample, which consists of native workers aged 18-49 years who were in the last month of a fixed-term contract between 2003 and 2017.

C Descriptive Evidence

C.1 Mincer Regression Results

For comparability with previous studies on returns to experience (Roca and Puga, 2017; Garcia-Louzao et al., 2023; Arellano-Bover and Saltiel, 2021), we estimate the contribution of contract-specific experience to earnings growth using a Mincer-type regression. We account for differential returns to experience by explicitly modeling combinations of experience accumulated in fixed-term and open-ended (permanent) contracts. We estimate the following equation by OLS:

$$\ln w_{irt} = \beta_1 exp_{it}^{FT} + \beta_2(exp_{it}^{FT} \times exp_{it}) + \beta_3 exp_{it}^{OEC} + \beta_4(exp_{it}^{OEC} \times exp_{it}) + X_{it}'\mathbf{\Omega} + \sigma_r + \psi_t + \varepsilon_{irt}, \tag{7}$$

where exp_{it}^{FT} and exp_{it}^{OEC} denote the worker's experience accumulated until period t in fixed-term and in open-ended contracts, respectively. The variable exp_{it} is the total experience of individual i up to period t. X_{it} is a vector of time-varying individual and job characteristics, including gender and occupation-skill group interacted with educational attainment, sector fixed-effects, age, age squared, and an interaction of tenure with a fixed-term contract indicator, σ_r is a province fixed effect, ψ_t is a year-month fixed-effect, and ε_{ict} is the error term.

Instead of the typical quadratic form of homogeneous returns to experience, equation (7) considers the product between overall experience and contract-specific experience. This interaction captures that the moment at which workers accumulate experience in each type of contract matters. In other words, the returns to an extra year of lower-quality experience at the beginning of the career may differ from the returns at mid-career. The estimates are shown in Appendix Table C.1.1. Disregarding the distinction between fixed-term and open-ended contracts, column (1), shows that one extra year of experience is associated with a 2.5% increase in individual earnings for workers with ten years of experience. Column (2) breaks down experience by the type of contract where it was accumulated. While the coefficients on linear experience are similar for both contract types, the main differences in workers' trajectories arise from the interaction terms. While the first years of experience in open-ended or fixed-term contracts yield similar wage returns, the growth rate for those in fixed-term contracts is lower in subsequent years. For a worker with ten years of experience, an additional year on a fixed-term contract translates into a 3.0% increase in earnings. In contrast, an additional year in an open-ended contract is associated with a 4.5% surge.

Although this specification acknowledges that the value of accumulated experience in each type of contract might differ, it ignores the potential sorting of workers into each type of contract. For instance, if high-ability workers are over-represented in open-ended positions, the coefficients of Column (2) might reflect that more able workers tend to enjoy higher earnings irrespective of contract type. Previous work has addressed this concern by including worker-fixed effects, as in Column (3). The worker-fixed effect slightly attenuates the gap between fixed-term and open-ended contract returns, but the overall pattern remains unchanged. For a worker with ten years of experience, an additional year in a fixed-term position is associated with a wage growth of 4.6% as compared to 5.6% if this experience was accumulated in a permanent contract.[16] These findings are consistent with the work of (Garcia-Louzao et al., 2023). The authors document lower returns to experience acquired in fixed-term contracts than in permanent contracts, suggesting that this discrepancy cannot be attributed to unobserved firm heterogeneity or match quality. However, the fixed-effects (FE) strategy initially followed by the authors and shown above could be significantly enhanced.[17]

As we show next, our initial descriptive estimates have, however, no causal interpretation. They reflect that more able workers are (i) more likely to enter an open-ended contract and (ii) enjoy faster earnings growth irrespective of contract type, a form of selection that is not captured by the fixed-effects approach.

[16]Based on these results, Figure C.1.1 illustrates the earnings trajectory for workers who accumulate experience in a fixed-term, open-ended contract, or a combination of both. While wage growth is almost equal over the first years, the gap in favor of open-ended positions rapidly widens after six years. After ten years, the earnings of a worker employed only in open-ended contracts differ from those who only accumulated fixed-term experience by 21%.

[17]One alternative that Garcia-Louzao et al. (2023) implement later on is to instrument experience and tenure using their deviations relative to average computed within contract and match history of the worker. Additionally, they exploit supplementary instruments based on regional variations in the availability of subsidies for hiring workers under open-ended contracts (OECs). In this paper, we leverage another form of variation using precise high-frequency data available in Spanish administrative records.

Table C.1.1: Wage growth in fixed-term and open-ended contracts: Results from Mincer regression

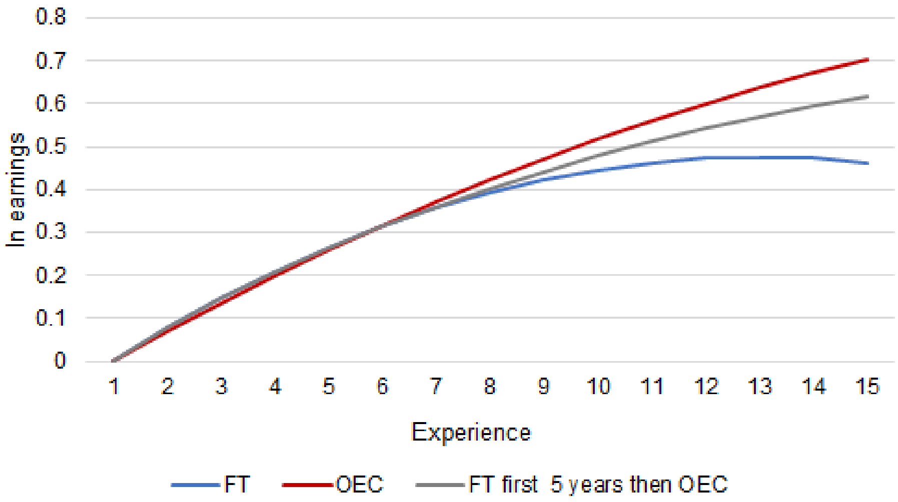
	(1)	(2)	(3)
		ln earnings	
exp	0.051***		
	(0.001)		
$exp^2/1000$	-1.314***		
	(0.032)		
exp_{FT}		0.064***	0.0794***
		(0.001)	(0.001)
exp_{OEC}		0.056***	0.0706***
		(0.001)	(0.001)
$exp \times exp_{FT}/1000$		-3.373***	-3.312***
		(0.063)	(0.055)
$exp \times exp_{OEC}/1000$		-1.049***	-1.446***
		(0.039)	(0.031)
Obs.	16,266,496	16,266,496	16,255,262
R^2	0.475	0.478	0.754
Controls	Yes	Yes	Yes
Ind. FE	No	No	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: exp, exp_{FT}, and exp_{OEC} account for experience, experience in fixed-term, and experience in open-ended contracts, respectively. Controls include gender and occupation-skill group interactions on education attainment, sector, region and time fixed-effects, age, age squared, and interactions of tenure with an indicator for a fixed-term contract. Errors are clustered at the worker level.

Figure C.1.1: Heterogeneous returns to experience by contract type

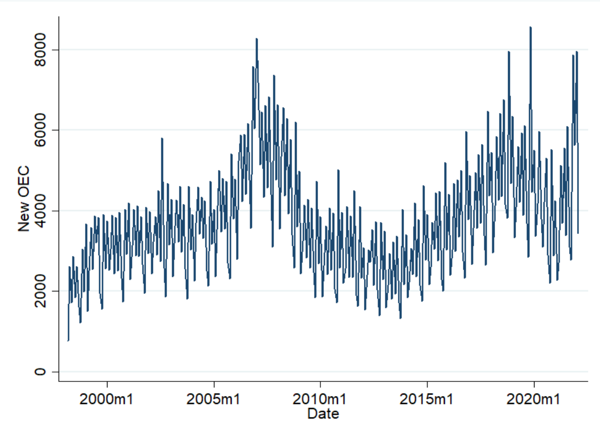


Notes: Fitted values based on experience coefficients from Column (3) in Table C.1.1.

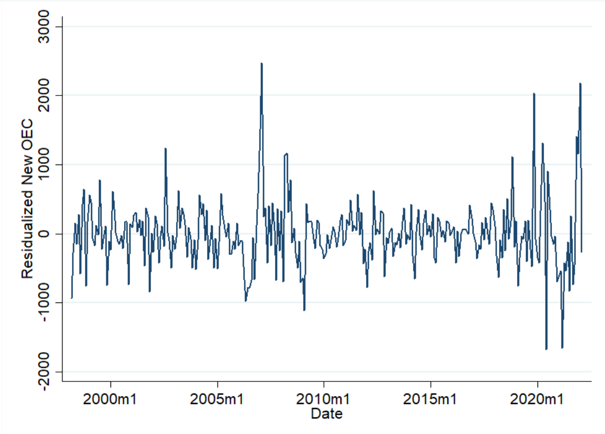
D Creation of New Open-Ended Contracts

Figure D.0.1: Comparison of New OEC measures

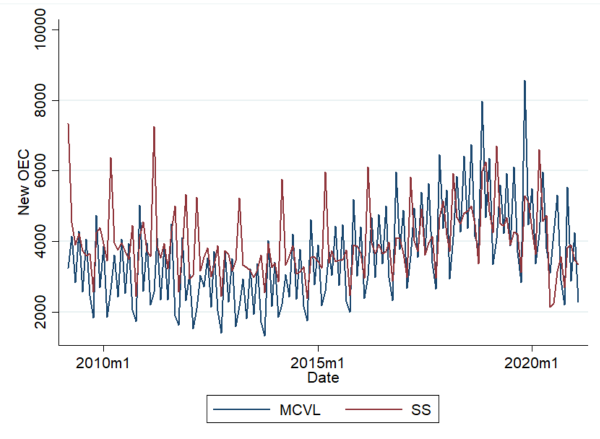
(a) New OEC



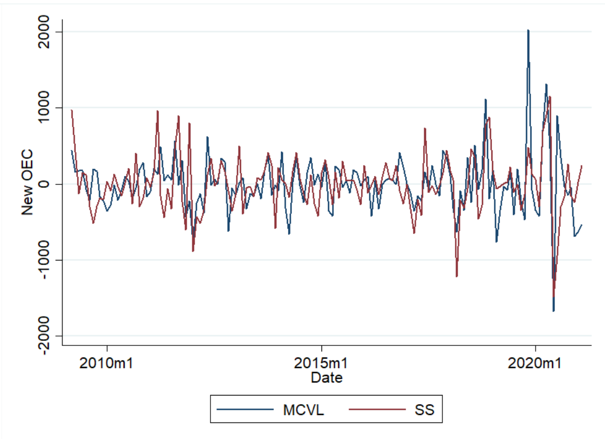
(b) Residualized



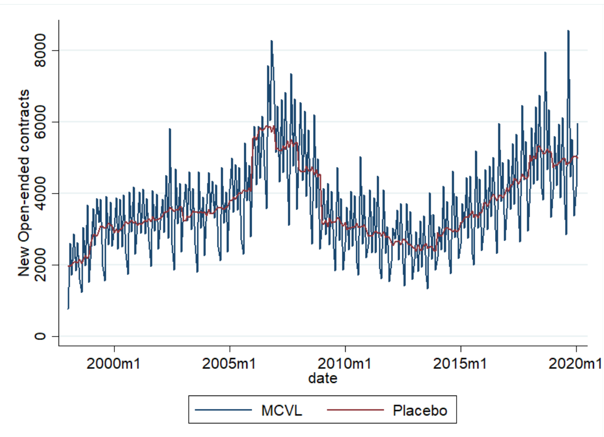
(c) New OEC MCVL/SS



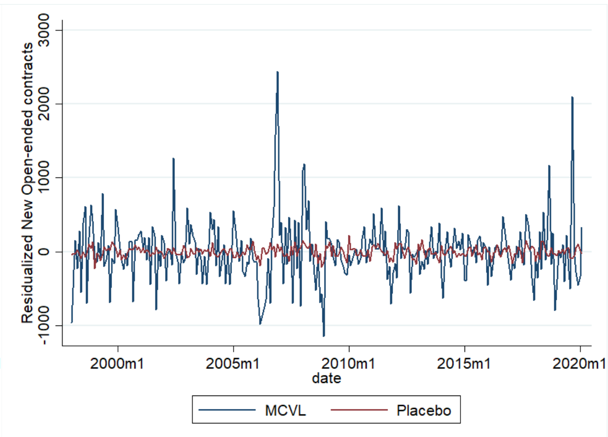
(d) New OEC MCVL/SS Residualized



(e) Placebo



(f) Placebo residualized



Panel (a) displays the number of new open-ended contracts at the national level from January 1998 to December 2021. Panel (b) shows the residualized number of new open-ended contracts after regressing the original data on year and month FE. Panel (c) and Panel (d) additionally include the number of new open-ended contracts by month, using data from social security records. Panel (e) and Panel (d) present a placebo exercise on creating open-ended contracts. In this exercise, the annual number of new open-ended contracts is preserved, but the month each contract is created is randomized.

E Population Social Security Information

E.1 Social security records

One potential concern with measuring the opening of permanent contracts *logOEC* is that our 4% sample of individuals registered with Social Security between 2006 and 2021 may provide a noisy measure of the actual creation of open-ended contracts during this period. However, we argue that the MCVL is sufficiently rich to provide representative variation in the creation of open-ended contracts by month and province, both essential for constructing our instrument. To address this matter, we compare our measure derived from the MCVL with registry data from Social Security. Our analysis indicates that the MCVL-derived measure closely mirrors the registry data, alleviating this concern.

Specifically, we use two time series obtained from Social Security: the count of affiliates per month and province since January 2009, and the data on new open-ended contracts per month since January 2009. We use the affiliate data to compute the variation in the count of affiliates as a proxy for the creation of permanent positions over time and by province. Additionally, we analyze the actual number of new open-ended contracts. This data is limited compared to the MCVL, as it is only available from 2009 onward. Besides it is only available at the national level so we cannot directly compare it to the regional or industry level.In any case, using national aggregates we are able to show that our measure of open-ended contracts constructed from the MCVL closely aligns with the population data from Social Security.

The analysis draws upon data from social security records which is accessible online using PX-Web.[18] This dataset provides a comprehensive depiction of the average monthly number of affiliates categorized by province, type of contract, and gender, spanning from 2009 to the present day. We calculate the average number of full-time affiliates across fixed-term and open-ended contracts, compared with the count of New OEC constructed from the MCVL.

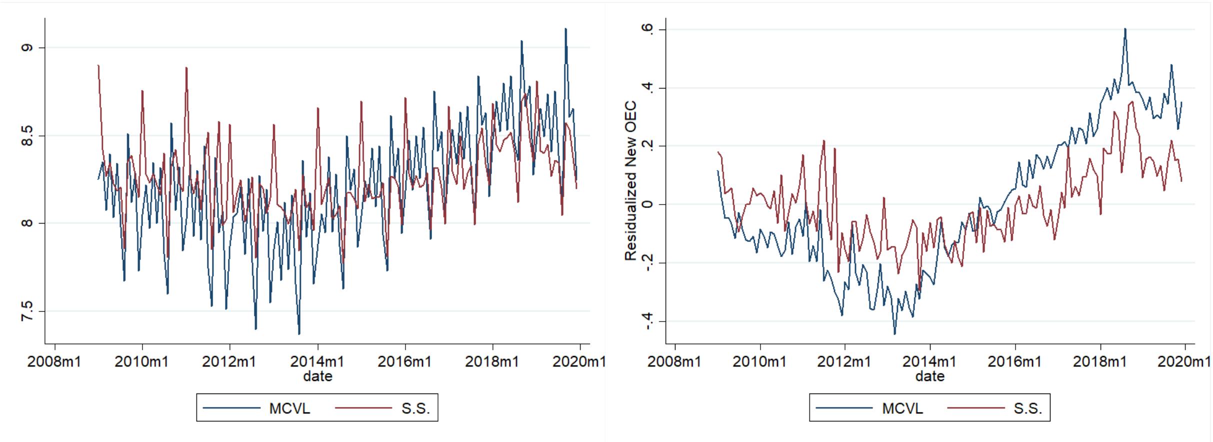
Figure E.1.1 presents the number of new Open-Ended Contracts (OECs) derived from the MCVL alongside the count of new OECs from the Social Security registry from January 2009 to March 2020. Panel (a) shows a strong relationship between both series. However, they are not perfectly aligned, which is understandable considering that the MCVL is a sample rather than the entire population.

As an additional check, Table E.1.1 use the instrument presented in the main text: the logarithm of New Open-Ended Contracts by province and month. The analysis is limited

[18] https://w6.seg-social.es/PXWeb/pxweb/es/ "Afiliados R. GENERAL por sexo, tipo de contrato y jornada, provincial".

Figure E.1.1: New OEC and Total OEC from MCVL and SS records

(a) Baseline (b) Residualized



Notes: Count of NewOEC obtained from Social Security records (OEC_{Total}) and from the MCVL. Panel (a) displays the monthly count of New open-ended contracts from both data sources. Panel (b) Residualizes the count of new Open-Ended Contracts (OEC) by subtracting the variation explained by month Fixed Effects.

to January 2009 and March 2020 to align with data availability from Social Security records. Since the number of new OEC by province and month is unavailable in the population records, we use the logarithm of affiliates in OEC by province and month. These findings further confirm the close relationship between both series, as evidenced by each specification's high R^2 values. This indicates that the our data effectively captures a significant portion of the variation expected from the month-province variation in the opening of open-ended contracts.

Table E.1.1: Relationship between New Open-Ended Contracts and Total OEC from Social Security records

	(1)	(2)	(3)
	$logNewOEC_{prov}$		
$logOEC_{total}$	1.084***	1.083***	1.309***
	(0.006)	(0.005)	(0.138)
Constant	-1.753***	-1.829***	-4.479**
	(0.072)	(0.071)	(1.547)
Obs.	6,697	6,697	6,697
R^2	0.810	0.907	0.941
Time FE	No	Yes	Yes
Region FE	No	No	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The table presents the regression coefficients representing the relationship between the logarithm of New Open-Ended Contracts by province from MCVL and the logarithm of total OEC registered with Social Security between January 2009 and March 2020. Column (1) presents the baseline relationship between these variables. Columns (2) and (3) additionally control for year-month and province-fixed effects.