# Quasi-Random Matches: Evidence from Dual Labor Markets*

María Alexandra Castellanos† Henry Redondo ‡ Jan Stuhler §

– Preliminary draft –

October 1, 2024

**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 contract upgrade probabilities. Reaching a permanent position translates into higher employment probability and earnings growth in the short-run. However, the stability derived from these positions does not seem to lead to long-lasting gains in earnings.

JEL Codes: J29, J31, J41, J60

*This paper has been earlier presented under the title "Wage growth in a dual labor market". We thank Samuel Bentolila, Juan Dolado, Luigi Minale, Jesús Fernández-Huertas Moraga, José Ignacio García Pérez, Judit Vall Castello, Jérome Valette and seminar participants at UC3M for their helpful comments.

†IMF. 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., 2023). 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. Therefore, they would 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 selection problem and our identification strategy, respectively and Section 7 analyses the effect of contract upgrade in workers’ career trajectory by evaluating a series of labor market outcomes. Section 8 provides additional robustness checks and Section 9 concludes.

## 2 Institutional Background

After the democratic transition, Spain's institutions underwent major changes, including reforming its labor market legislation. The approval of the Workers' Statute in 1980 stipulated the use of open-ended contracts as the general case, reserving temporary contracts for seasonal work or short-term replacements. The Statute also introduced specific severance regulations, with severance costs for temporary positions being substantially lower than for permanent contracts – 8 days of wage per year of seniority for temporary contracts, compared to up to 45 days for permanent ones. Despite these regulations, the use of fixed-term contracts was heavily restricted.[2]

The establishment of a dual labor market structure began a few years later in 1984. Enacted by Law 32/1984, this labor reform liberalized the use of temporary contracts with the objective of promoting job creation amid high unemployment rate levels. The use of fixed-term contracts became widespread given that the modality was not limited by the seasonal nature of firms' activities. With the coexistence of both temporary and permanent contracts, firms could choose between opening a permanent or temporary vacancy, the former with a considerably higher severance payment compared to the latter. Since the reform did not alter the conditions for permanent contracts, temporary contracts became particularly appealing for firms (Bentolila and Dolado, 1994; García-Pérez and Muñoz-Bullón, 2011; 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 only for seasonal jobs (Bentolila et al., 2012; García-Pérez et al., 2019). This perceived ineffectiveness of the 1994 reform led to additional reforms in 1997 and 2001. The 1997 reform created a new type of permanent contract with a smaller severance payment – to 33 days per year of seniority in some cases– and encouraged via fiscal incentives the conversion of temporary to open-ended contracts of certain demographic groups.[3] The 2001 reform extended lower subsidies for additional groups of workers (García-Pérez and Muñoz-Bullón, 2011; García-Pérez et al.,

[2]During the initial years of the political transition (1975-1982), there was an intention to prioritize permanent hiring. However, as time passed, it became clear that this goal was unattainable. This led to an increase in temporary employment and, in the early stages, a rise in illegal temporary contracts (Galacho, 2006).

[3]For instance, workers under 30 years old, over 45, women in under-represented occupations, and disabled workers.

2019) with similar incentives being introduced or restructured in the 2006 and 2010 subsequent reforms.

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.[4] In addition, the reform eliminated interim wages in judicial processes and a new open-ended contract was introduced for firms below 50 employees, entailing no severance pay during an extended probationary period of one year. After this period, workers were entitled to the same severance payments than those on ordinary permanent contracts. A concern regarding these so-called *entrepreneurship contracts* was that given the initial zero costs, the *"discrete jump"* in employment protection after 12 months was larger than the employment protection gap between ordinary permanent and temporary contracts (Dolado, 2017). Despite these changes, the share of fixed-term contracts was still above 20% after the reform. Furthermore, a significant decline in temporary employment during this period was largely attributed to the Great Recession.

Concerns about the lack of job stability for workers in temporary contracts and its potential adverse consequences motivated several labor reforms in the past 30 years. Before 2021, most of them targeted different aspects such as severance costs, duration and roll-over penalization, but did not alter substantially the duality driven by the marked flexibility of temporary employment. This is the setting that we analyze in this paper. In contrast, the recent –and latest– reform of December 2021 seems to have caused a significant reduction in fixed-term contracts (Conde-Ruiz et al., 2023). Fully effective since March 2022, the reform prohibited project-based labor contracts and introduced a *single* temporary contract for structural reasons denominated *circumstances of production* contract. With a 6 months duration limit and extendable for up to one year, subject to collective bargaining agreements, it can only be used for production-related needs such as seasonality peaks (e.g. Christmas or agricultural seasons).[5] However, the reform also facilitated a broader use of the so-called *intermittent-permanent* contracts, which are highly flexible and their use increased after the 2021 reform. It remains to be seen to what extent

[4]With the 2012 reform, the compensation for terminating a temporary contract in Spain increased from 8 to 12 days of wages per year worked whereas the compensation for *unfair dismissals* of permanent contracts was reduced from 45 to 33 days. The compensation for *fair* separations of permanent contracts remained unchanged at 20 days per year worked (see Dolado (2017)).

[5]Additionally, this temporary contract can be used to cover workers who are on temporary leave. Moreover, the reform established that workers would be entitled to *permanent* status if they are continuously employed more than 18 months in a fixed-term modality or within a 24-month period in the same or different job positions within the same company or group of companies, through two or more contracts. The reform also introduced a special training-based temporary contract for individuals up to 30 years old, within three years following their completion of studies, supervised by a mentor, with a minimum duration of three months and extendable up to two years.

the recent reform will reduce the existing labor market segmentation.[6]

# 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.[7] 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 allows 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 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 employment

[6] Conde-Ruiz et al. (2023) make a distinction between *contractual* and *empirical* temporary employment rate in Spain, arguing that the 2021 reform primarily addressed the former.

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

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,[8] we compute monthly real earnings from tax records whenever available,[9] 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, reliable information on the type of contract for all workers is available 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 to 49 to reduce the potential influence of early retirement on labor market outcomes and the lack of complete labor market histories for foreign workers. 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 access to permanent positions relies on a special process.[10] 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. Regionally, we exclude information from Ceuta and Melilla, for which the sample of workers is very

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

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

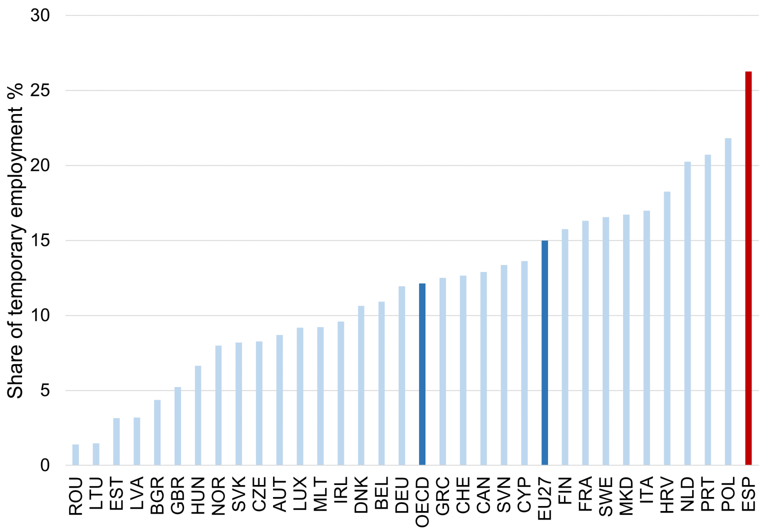
[10] Workers in the public sector are usually required to approve specific exams and fulfill special requirements to get permanent position. This process is quite different from the promotion path of private sector workers.

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).[11]

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




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

As previously discussed, the significant 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). Although this problem is more pronounced for low-skilled occupations, it is also relevant at the top of the skill distribution. As shown in Table 1, the share of high-skilled occupations among temporary contracts has steadily increased. In terms of gender, the share of fixed-term contracts is similar for both women and men. 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 by 2016.

For comparability with previous studies on returns to experience (Roca and Puga,

[11]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 Appendix Figure A.1.3.

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.

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:

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

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, $\sigma_r$ and $\psi_t$ are province and year-month fixed-effects, and $\varepsilon_{ict}$ is the error term.

Few years of experience in either open-ended or fixed-term contracts yield similar wage returns, but the growth rate for those in fixed-term contracts is lower with several years of accumulated experience in such contracts (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% increase in earnings. 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 studies have tackled this concern by including worker-fixed effects. This approach slightly narrows 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.[12] 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.

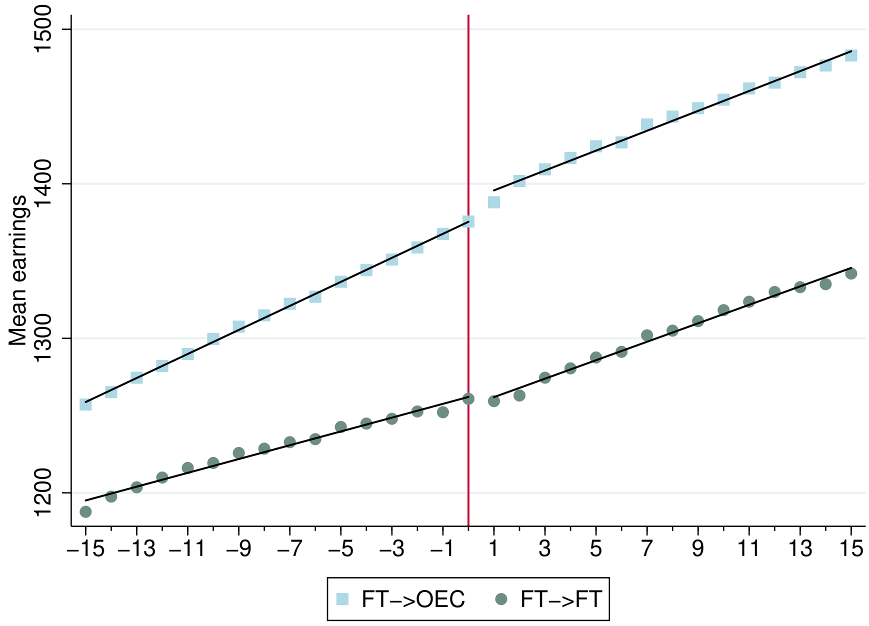
To assess this possibility, we examine whether workers with open-ended and fixed-term contracts follow parallel earnings paths before their contract change, i.e. when all hold a fixed-term position. Following a similar methodology as Card et al. (2013) and Card et al. (2023), we begin with a sample of workers in their final month of a fixed-term contract. We then categorize these workers based on the type of contract they transition to and evaluate the earnings trajectory of each group by examining the 15 months of non-zero earnings before and after the switch to their new positions. Figure 2 displays the average earnings of each group of workers relative to the month in which they started a new position (fixed-term or open-ended).[13] Unlike other settings,[14] we observe that workers who will eventually switch to a permanent position are already on a different path even *before* the transition takes place, i.e., when all are still in a fixed-term contract. These

[12] 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.

[13] In Figure A.2.1 in the appendix, we present evidence from a similar analysis using median earnings. Additionally, we distinguish between transitions to open-ended contracts (OEC) that occur within the same firm and those associated with moving to a different firm.

[14] 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 before and after switching to a new contract




**Notes:** Average earnings for workers transitioning to open-ended or fixed-term contracts. We follow workers 15 months before and after switching to a new contract.

patterns in the raw data point to a dynamic selection problem.

To study these differences in earning trends formally, we adopt an event-study design. For each worker in the data, we denote the last month before the individual ends a temporary contract by $h = 0$ and index future and past months relative to that moment. We categorize workers based on their future type of contract, distinguishing workers transitioning from a fixed-term to an open-ended contract (FT→OEC, $C_i = 1$) and workers transitioning to another fixed-term contract (FT→FT, $C_i = 0$). Our baseline specification considers a balanced panel of workers for whom we observe fifteen periods (months) before and after the event.[15] We denote by $y_{ith}$ the log earnings of individual $i$ at year-month $t$ and event-time $h$. We then estimate the following regression:

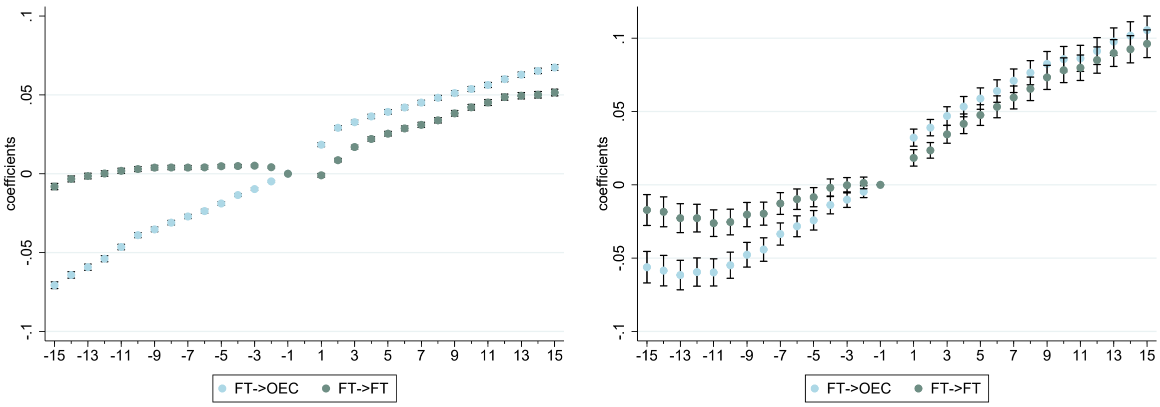
$$\begin{aligned} y_{ith} = & \sum_{k \neq -1} \alpha_k^{FT-OEC} \cdot \mathbf{I}[k = h] \cdot \mathbf{I}[C_i = 1] + \sum_{k \neq -1} \alpha_k^{FT-FT} \cdot \mathbf{I}[k = h] \cdot \mathbf{I}[C_i = 0] \\ & + \sum_j \beta_j \cdot \mathbf{I}\left[j = age_{it}\right] + \sum_q \gamma_q \cdot \mathbf{I}[q = t] + \lambda_C + \nu_{ist}, \end{aligned} \tag{2}$$

where we include a complete set of event time dummies (first term and second term on the right-hand side), age dummies (third term), year × month dummies (fourth term), and an

[15] We allow for short unemployment spells before or after switching contracts but only include periods with non-zero earnings in the regression. Consequently, the event time, which covers 15 months before and after the end of the fixed-term contract, may differ from standard calendar months.

indicator for the type of contract transition. As we omit the event time dummy $h = -1$ from the estimation, the event time coefficients measure the impact of moving into a new contract relative to the earnings just 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. Including age dummies in the comparison is important because workers in open-ended positions tend to be older than workers in temporary positions. We also control non-parametrically for time trends such as business cycle variation by including a full set of calendar time dummies.

Figure 3: Event Study: Evolution of earnings before and after switching to a new contract




(a) No controls

(b) Additional controls

**Notes:** The figure shows event time coefficients estimated from Equation 2 for workers transitioning to open-ended contracts (OEC) and fixed-term contracts (FT). These coefficients are derived from a sample of workers observed between January 1998 and February 2020. The analysis tracks workers log-earnings for 15 months before and after contract change. Since we allow for short unemployment spells between contracts, the periods observed (employment periods) do not necessarily align with calendar months. The base category is $h = -1$, and each specification controls for age and time (year-by-month) fixed effects. Panel (a) presents our baseline specification. Panel (b) incorporates additional interactions of event time with educational attainment and sector-fixed effects. Additional figures where we examine changes within or to a new firm are provided in Appendix A.2.

Results are presented in Figure 3. Panel (a) controls for the full set of time and age dummies discussed above. Additionally, Panel (b) also includes interactions between event time and workers' educational attainment and sector, accounting for earnings growth differences due to 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), Panel (a) illustrates that workers transitioning to an open-ended contract experience an approximate increase of 2 log points in earnings during the first month of the new contract. In contrast, workers who secure another fixed-term contract show no significant earnings difference compared to their earnings in the preceding contract.

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. Panel (a) shows that workers who secure an open-ended contract experience a growth of 7 log points over a 15-month period, in stark contrast to the negligible earnings difference observed in workers who remain on 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. In fact, the difference in earnings growth between worker types is much more pronounced before any transitions to open-ended contracts take place.

This evidence suggests that controlling for individual fixed effects is insufficient to account for unobserved worker differences in this context. By comparison, Card et al. (2013) show that workers who switch from low- to higher-paying firms tend to experience similar wage growth as those making the reverse switch, suggesting that worker-firm matching is sufficiently random in a dynamic sense. One factor that might explain why this is not the case in our setup is that transitions to open-ended contracts often occur within firms; therefore, they are based on more information than workers switching between firms. Figure A.2.2 in the appendix separately examines workers who switch to an open-ended contract within the same firm and those who secure an open-ended contract in a different firm. Consistent with the previous discussion, workers who secure an open-ended contract within the same firm exhibit a markedly different earnings trajectory before the transition compared to those who remain on fixed-term contracts and even compared to those who find open-ended positions at other firms.

# 6 Identification

To deal with the endogeneity of contract 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 when their contract expires. This affects contract upgrade probabilities in direct and indirect ways: in the most direct channel, workers have greater chances of securing permanent positions within 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:

$$p_{it+1} = \sum_{k=-24}^{24} \alpha_k logOEC_{-i,r,t+k} + X'_{it}\theta + \epsilon_{it}, \tag{3}$$

where $t$ refers to the month in which the worker's fixed-term position ends. Thus, $p_{it+1}$ indicates whether the worker starts an open-ended contract in $t+1$, after their current fixed-term contract finishes. The variable $logOEC_{-i,r,t+k}$ is constructed as the sum of all new open-ended positions in period $t+k$ in worker $i$'s initial province of residence $r$, leaving out individual $i$ herself. Therefore, we allow contract upgrade to depend on the total number of new open-ended contracts in period $t$ and leads and lags of this variable, excluding individual $i$ in the calculation. The first lead, $logOEC_{-i,r,t+1}$, is our instrumental variable. As we control for year-fixed effects as well as month-fixed effects, the instrument $logOEC_{-i,r,t+1}$ 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 the coefficient $\alpha_1$, to be the strongest predictor of an individual's probability of switching into a permanent position. The coefficients on other leads and lags ($\alpha_k$ for $k \neq 1$) should be smaller in magnitude but might be non-zero, as they also capture general business cycle conditions that might affect contract upgrade 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 lead 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 ($logFTC$), 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 and experience squared at baseline, as well as interactions of age categories with educational 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.

As mentioned above, Figure 4 depicts the leads and lags in the number of new open-ended positions at the *regional* level. We can apply the same logic to exploit variation in the number of new permanent positions aggregated at the national or by worker's baseline industry instead. As shown in Figure A.4.1 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. Moreover, these findings are robust to excluding from the dataset months of potentially high job-seasonality (see Appendix A.5). 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 contract upgrade for any worker.

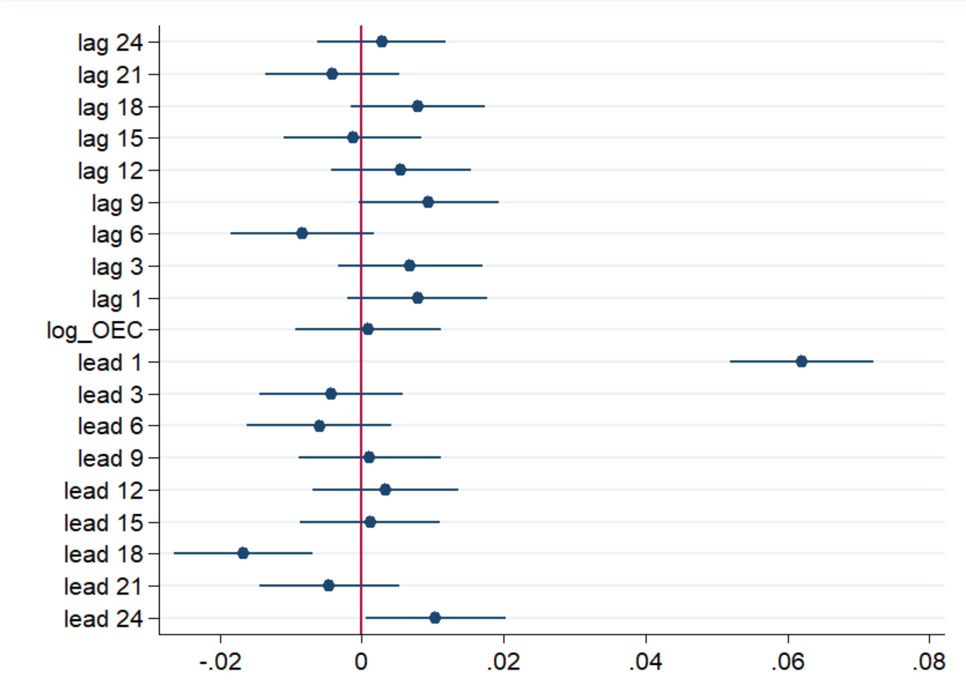
We argue next that the instrument also satisfies the independence assumption and exclusion restriction. The instrumental variable identifies, therefore, the labor market consequences of entering a permanent contract for "compliers", i.e., workers who find a permanent contract only if the local labor market conditions are sufficiently favorable. This local average treatment effect (LATE) may differ from the returns to contract type for other types 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

Figure 4: Effect of new open-ended contracts in the region on individual contract upgrade probability




**Notes:** The sample consists of workers who were in the last month of a fixed-term contract in event period $h = 0$, with at least 0.8 but less than 1.2 years of tenure. The coefficients correspond to the effect of leads and lags of the log of new open-ended contracts in the region on the probability of switching to an open-ended contract in $t+1$. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and educational attainment, experience and experience squared at baseline, and leads and lags of the *log* of new fixed-term contracts.

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 support 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.6.1. Each panel presents the coefficients from a regression of $logOEC_{-i,r,t+1}$ on worker and firm characteristics at time $t$, with additional controls for year, month, and region fixed effects, along with leads and lags of $logOEC_{-i,r,t+1}$— 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 firm characteristics, specifically the firm's age and size. Again, the correlation between these dimensions and the sum 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 regional, sectoral, and firm 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 to 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 a link. This concern might be relevant, but we have already addressed it by accounting for year and month-fixed effects and leads and lags of new open-ended and fixed-term contracts. This approach allows us to control for the influence of the business cycle, even in situations where employers extend fixed-term contracts for workers they want to retain.

Additionally, two key aspects of our strategy mitigate this potential concern. First, at the time of the analysis, legal limitations existed on the consecutive renewal of temporary contracts. 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.[16] 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, 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 restrict to those workers who terminate their contract at the initially intended date. Likewise, it excludes extremely brief work contracts, common in our context (Bentolila et al. 2020).

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

# 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 contract-upgrade opportunities impact workers' labor market outcomes in the short and long term.

For this analysis, we define a sample of workers 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 and our instrument. Focusing on this group of workers, we proceed to estimate the following reduced-form equation:

$$y_{it+h} = \sum_{k=-24}^{24} \alpha_k logOEC_{-i,r,t+k} + \mathbf{X}'_{it}\theta + \epsilon_{it}, \tag{4}$$

where $y_{it+h}$ is the worker's $i$ outcome in period $t+h$, with $h = -60, \ldots, 60$. Hence, 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 long-term effects ($h > 0$) and to evaluate pre-trends on the outcomes in the pre-treatment period ($h < 0$). To control for business cycle variation and job creation seasonality, 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 and also include the same number of leads and lags of the *log* of new fixed-term contracts in province $r$ denoted by ($logFTC$), as we did in the first stage. 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 educational 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, $\overline{Y}_{-i,r,t+h}$, as in

$$y_{it+h} = \sum_{k=-24}^{24} \alpha_k logOEC_{-i,r,t+k} + \sum_{k=-24}^{24} \gamma_k logFTC_{-i,r,t+k} + \delta\overline{Y}_{-i,r,t+h} + X'_{it}\theta + \epsilon_{it}, \tag{5}$$

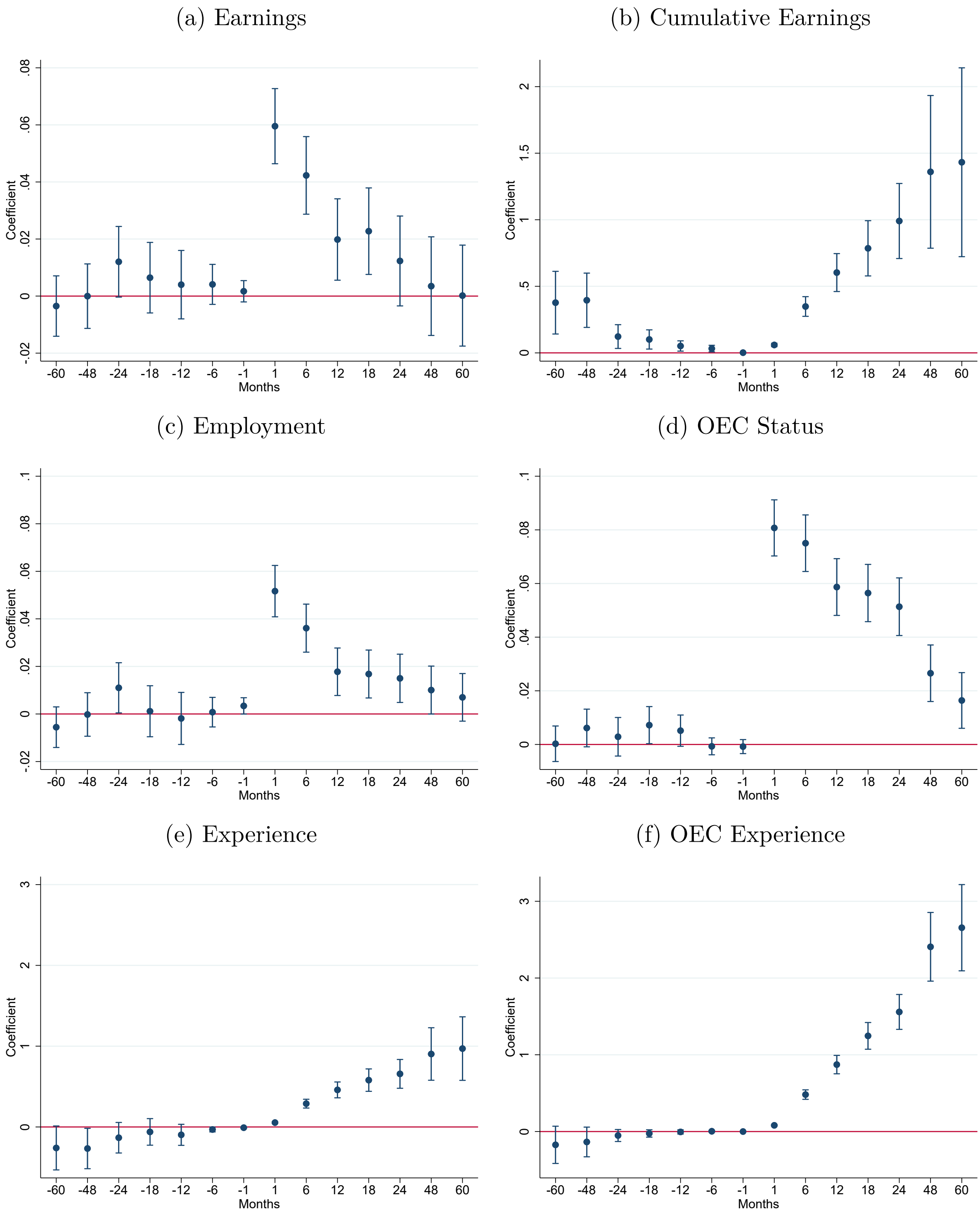
where we construct $\overline{Y}_{-i,r,t+h}$, based on the full sample of workers aged 18 to 49 years old, irrespective of the timing of their contract expiration date (i.e., there is no mechanical link between $y_{it+h}$ measured for recently hired workers and $\overline{Y}_{-i,r,t+h}$ measured for all workers[17] in the labor market).[18] This control further ensures that economic conditions are held constant, such that our instrument only captures transitory variation in the availability of open-ended positions that are uncorrelated with general business-cycle trends. The results from this specification are presented in Figures A.7.1 and A.7.2 in the Appendix.

In terms of outcomes, we first evaluate the effect of our instrument on both earnings and employment outcomes. For earnings, we track the earnings and cumulative earnings relative to their earnings from the expiring contract. In terms of employment, we evaluate employment status, cumulative experience, 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 as well as the number of firm switches since the worker's fixed-term contract end. The results of the baseline specification described in equation (4) are illustrated in Figure 5. Additionally, Appendix A.7 shows that the results are robust to the alternative specification proposed in equation (5) and when considering different sets of control variables.

[17] Table B.1.2 in the appendix presents descriptive statistics for the entire sample. For comparison, this sample comprises an average of 311,150 monthly workers, whereas the estimating sample includes only 1,200 monthly workers.

[18] For example, when studying wage effects $y_{it+h}$ 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 $\overline{Y}_{-i,r,t+h}$ would capture the growth in wages of all workers during that same period and province of employment (irrespective of the timing of their contract end and start dates).

Figure 5: Reduced-Form Evidence: Effect of New Open-Ended Contracts in the Region on Workers' Individual Outcomes




**Notes:** The sample consists of workers who were in the last month of a fixed-term position in event period $h = 0$, with at least 0.8 but less than 1.2 years of tenure. Period 1998-2017. The coefficients correspond to the effect of the first lead of the *log* number of new permanent contracts ($logOEC$) on each outcome. All regressions control for the leads and lags of $logOEC$ as well as the *log* of the number of new fixed-term contracts. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and educational attainment, experience and experience squared at baseline.

### 7.1.1 Earnings

Panel (a) in Figure 5 presents the long-term effects of contract upgrade opportunities on workers' earnings. We estimate equation (4) separately for different event periods $h$ and then plot the coefficients of our instrument $logOEC_{-i,r,t+1}$. Earnings are measured as the ratio between monthly earnings[19] at $t+h$ and their earnings during the baseline period $t$, which corresponds to the last month of the expiring contract. Thus, the coefficients capture the effect on workers' earnings compared to their last contract before switching to a new (fixed-term or open-ended) position. Similarly, Panel (b) explores the effect on workers' cumulative earnings, computed as the sum of monthly earnings from period $t$ to $t+h$, also normalized by the monthly earnings in period $t$.

As shown in Panel (a) of this figure, we find a sharp and large positive effect on workers' earnings in the event period $h = 1$, i.e., one month after being exposed to better employment opportunities. A 10 percent increase in the number of permanent contracts raises the wage of exposed workers by 0.6 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 are explained by the impact of contract upgrade 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 $h = 0$ and remained in fixed-term contracts eventually attain a contract upgrade after several years. Consequently, the disparity between those with a contract upgrade at $h = 1$ and the remainder diminishes over time, explaining part of the observed effects. However, as we discuss below, this catch-up process of the control group only partially explains the decline in the wage effect. In the next sections, we examine other mechanisms that explain further this decline.

### 7.1.2 Employment

Panels (c) to (f) in Figure 5 also show the effects of contract upgrade opportunities on employment trajectories. Panel (c) examines how these upgrade opportunities influence the likelihood that a worker is employed at period $t+h$. Similarly, Panel (d) analyzes the probability that workers are employed on an open-ended contract in period $t+h$. Finally, Panels (e) and (f) extend this analysis by focusing on cumulative experience

[19] Calculated as daily earnings multiplied by the number of days in the month.

and experience in open-ended positions. Specifically, Panel (e) explores total experience, measured by the total number of days worked since the termination of the fixed-term contract, and Panel (f) examines open-ended contract experience, measured by the number of days employed under an OEC (both expressed in months). As illustrated in panel (c), the effect of enhanced opportunities to switch to an open-ended contract translates into a higher probability of employment in the short run. The probability of being employed increases sharply in the 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 earnings (panel (a)). As shown in panel (d), the effect on the probability of working in a permanent contract increases by 0.8 percentage points.

As for wages, these employment effects dissipate over time. A "lucky draw" in upgrade opportunities provides thus only a temporary boost and has no long-term consequences on employment and contract upgrades.[20] 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, 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 earnings. However, the employment effect diminishes less rapidly over time than the effect on earnings: while the earnings effects are close to zero after four years, the employment effects remain positive. The pattern in employment can, therefore, explain the sharp initial gains in earnings, but not why the impact on earnings eventually fades away entirely. The zero long-run impact on earnings is difficult to square with the permanent gains in work experience documented in panels (e) and (f). Next, we will study whether other margins might explain the null long-run effect on earnings.

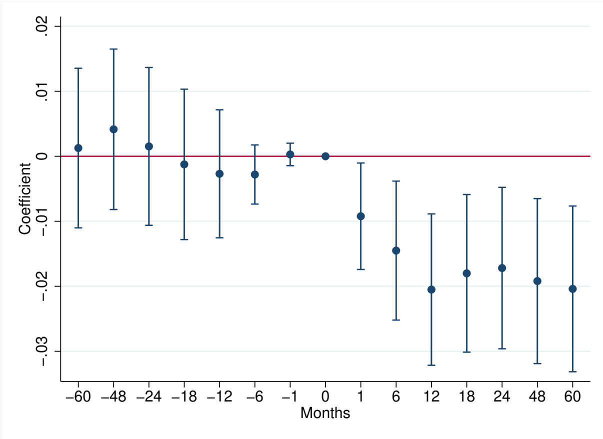
### 7.1.3 Sectoral and Regional Mobility

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 number of firm switches since period $t$ and the likelihood of moving to a different sector or region over time. The number of firm moves is measured as the total firm switches following the expiration of the fixed-term contract and the difference between the number of firm moves and firm moves in period $t + h$ for the pre-periods. For the

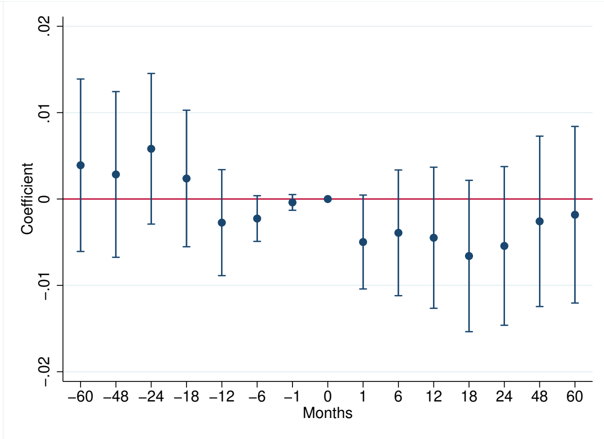
[20] This finding aligns with the observation that sorting workers into contracts is highly selective.

Figure 6: Effect of new open-ended contracts in the region on workers' mobility

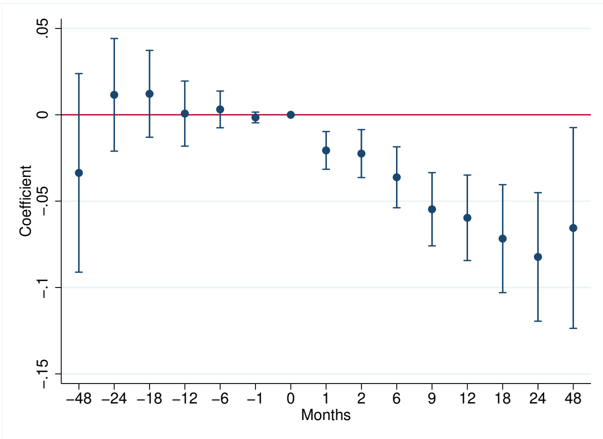
(a) Probability to change sector




(b) Probability to change region




(c) Number of firm moves




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

regional and sectoral mobility, these outcomes are measured using an indicator variable that takes a value of one if the worker has changed sector or province 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 reduced likelihood of changing sectors and a lower, though statistically insignificant, probability of 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. This idea is supported by the evidence in Panel (c), where a 10% increase in new open-ended contracts is associated with an 8 percentage point reduction in firm switches two years after the expiration of the fixed-term contract.; these findings prompt a potential explanation for the null long-term response in earnings 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. Figures A.8.1 and A.8.2 in the Appendix present additional specifications, and the results are consistent with prior interpretations. These specifications employ alternative definitions of sectoral and regional mobility and restrict to to employed workers in each period, respectively. In the next section, we quantify the impact of switching to an open-ended contract in a 2SLS specification.

## 7.2 IV Estimates

We now move on from the reduced-form analysis to provide 2SLS estimates of the effect of upgrading to a permanent contract. 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:

$$y_{it+h} = \beta p_{i,t+1} + \sum_{k\neq 1, k=-24}^{24} \alpha_k logOEC_{-i,r,t+k} + X_{it}'\theta + \epsilon_{it}, \tag{6}$$

where $p_{i,t+1}$ is an indicator that equals one if the worker switches to an open-ended contract in $t+1$, after terminating its current fixed-term position.[21] The dependent variable $y_{it+h}$ is the worker's $i$ outcome in period $t+h$, with $h = 1, \ldots, 60$. As before, we 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: $\log OEC_{-i,r,t+1}$.

Table 2 summarizes our findings regarding 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. In the short-run, we observe a notable increase in earnings and the probability to be employed for workers who transitioned into open-ended contracts. The results are in line with our reduced-form evidence. We observe that switching to a permanent position translates into a 25% increase in earnings after one year. However, the effect is not long-lasting, dropping to an almost zero effect after five years. Still, in terms of cumulative earnings, workers retain a considerable advantage, attributable to the initial *boost* in wages and employment. In this regard, 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 in the long-run. As a consequence, workers also gain more (total) experience over

[21] Notice that those that do not switch into a permanent position at $t+1$ can either start a new fixed-term contract or enter non-employment.

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

| | Earnings | Cum. Earnings | Employment | Experience | Change Region | Change Sector |
|---|---|---|---|---|---|---|
| **Panel A: Short-term effects (12 months)** | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $p_{i,t+1}$ | 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)** | | | | | | |
| | Earnings | Cum. Earnings | Employment | Experience | Change Region | Change Sector |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $p_{i,t+1}$ | 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 described in Figure 5 notes. Robust standard errors in parentheses. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

time, accumulating more than one additional year of experience when observing workers five years after having switched to a permanent contract. Appendix Table B.2.1 shows that this long-term advantage in cumulative experience is, as expected, even larger for open-ended contract-specific experience with nearly 31 additional accumulated months.

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. As argued earlier, the considerable reduction in mobility once workers enter a permanent position may account for the short-lived positive effect on earnings growth.

## 7.3 Heterogeneity

We now assess whether an increase in new open-ended contracts impacts worker outcomes, varying across different worker and firm characteristics. This analysis serves two key purposes. First, it allows us to examine how our instrument influences the likelihood of workers being upgraded to permanent positions, which constitutes our first stage. Second, it enables us to explore how an increase in upgrade probabilities affects workers' earnings and employment, both in the short and long term.

Table 3 presents the estimated short-term and long-term effects from our 2SLS estimation, but we split the sample across various subgroups. Specifically, we analyze workers based on education, gender, age, and baseline earnings, distinguishing between those above and below the median earnings. Additionally, we split sectors based on whether the share of fixed-term contracts is above or below the median, categorizing them as either high or low fixed-term contract sectors.

Panel (A) presents the short-term effects on workers' earnings and employment. A key finding is that the instrument has a stronger impact on upgrade probabilities for workers with at least secondary education than those with less education. Additionally, workers with higher initial earnings are more likely to be promoted as the number of new open-ended contracts increases. Regarding outcomes, the largest effects on cumulative earnings are observed among lower-educated workers, women, and younger workers. This is intuitive for younger workers, as they tend to have steeper earnings trajectories, allowing them to accumulate higher earnings than slightly older workers.

Panel (B) adopts a similar approach but focuses on workers' outcomes 48 months after the expiration of their fixed-term contracts. Consistent with Panel (A), workers with initially higher earnings exhibit a stronger first-stage coefficient. Additionally, compared to female workers, male workers have a higher coefficient, which may be linked to differences in labor market attachment, particularly relevant early in their careers.

# 8 Additional Robustness Checks

***Social security records***

One potential concern with our measure of new open-ended contracts, $logOEC_{-i,r,t+1}$, is that it is based on a 4% random sample of the workers registered with Social Security, which could reduce precision in capturing the actual number of open-ended contracts during each period. However, we argue that the MCVL dataset is sufficiently rich to capture meaningful variation in openings of open-ended contracts by month and province—key factors for constructing our instrument. This section compares our MCVL-based measure with Social Security registry data to address this concern.

Specifically, we use two time series from Social Security records: the monthly count of affiliates by province and contract type, beginning in January 2009, and the data on newly created open-ended contracts per month over the same period. The affiliate data enables us to track variations in the number of affiliates as a proxy for the creation of open-ended contracts over time and across provinces. Additionally, we examine the data on new open-ended contracts, which directly captures the dynamics of each contract type, but lacks regional variation, relying solely on the time dimension. Both analyses show that our MCVL-derived measure of new open-ended contracts closely aligns with Social Security population data, supporting its validity.

The analysis use data from Social Security records, accessible online via PX-Web.[22] This dataset offers a detailed view of the average number of affiliates by province, contract type, and month, from 2009 to the present. Using this data, we compute the monthly count of full-time affiliates across fixed-term and open-ended contracts and compare these figures with the count of new open-ended contracts constructed from the MCVL.

Figure A.3.1 in the Appendix presents the number of new open-ended contracts derived from the MCVL alongside the count of new OECs from the Social Security registry from January 2009 to March 2020. Panel (a) demonstrates a strong correlation between the two series. While we observe minor discrepancies, these are expected as the MCVL restricts to 4% of the population, which introduces some noise. Nevertheless, the overall trends are consistent, indicating that the MCVL provides a reliable representation of open-ended contract dynamics compared to the full Social Security data. Panel (b) shows the residuals from a regression of each time series in panel (a), controlling for year and month-fixed effects. This approach helps to reduce volatility in both series and support a strong correlation between them.

As an additional robustness check, Table B.3.1 uses our instrument as described in the main text: the logarithm of new open-ended contracts by province and month. This is compared to the actual number of affiliates in open-ended contracts by month and province. The analysis covers the period from January 2009 to March 2020. The results demonstrate a strong alignment between the two series, as evidenced by the high $R^2$ values across all specifications. This indicates that our measure effectively captures a significant portion of the variation associated with monthly and provincial fluctuations in the creation of open-ended contracts.

### *Alternative instrument measures*

Our baseline specification examines the leads and lags in the number of new open positions at the *regional* level. This same approach can be extended to analyze new openings for permanent positions at national or more granular levels, such as industry-specific data or combining industry and province variation. This introduces an important

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

consideration regarding the appropriate level of disaggregation that accurately reflects the relevant labor market for workers and influences the worker's chances of securing a permanent position.

Previous work by Marinescu and Rathelot (2018) and Manning and Petrongolo (2017) show that workers tend to focus on job opportunities nearby and are discouraged by the distance in job vacancies. This motivates our preference for exploiting the variation at the specific month of a fixed-term contract's expiration within the same province of the previous job. However, we also present evidence in this section that our findings are robust across alternative instrument definitions.

As shown in Figure A.4.1 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. Moreover, these findings are robust to excluding from the dataset months of potentially high job-seasonality (see Appendix A.5).

***Exclusion restriction***

Next, we further prove that our instrument does not show 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. We exhaustively control for economic trends and cycle conditions in our model through time-fixed effects, leads and lags of our instrument, and new fixed-term contracts. Despite these controls, one might still be concerned about unobserved factors that could challenge our identification assumptions. If this was the case, we would expect a positive effect on employment irrespective of which contract type a worker found in event period $h = 1$.

To alleviate this concern, we run the following placebo test. Similar to the specification on 4, we estimate the reduced-form effect of our instrument on employment but restrict the sample to workers who remain in temporary positions after their fixed-term contracts end—specifically, those who transition from one fixed-term contract to another. If our instrument reflected economic conditions, we would also expect a positive impact on employment for these workers. Appendix Figure A.9.1 illustrates that there is no significant nor systematic employment response, ruling out this possibility.

***Tenure***

As described in Section 6.1, we restrict the sample to those workers in the last month of a fixed-term contract with a tenure of 0.8-1.2 years. 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 Appendix A.10, we extend the reduced-form analysis by widening the tenure window to 0.4–1.2 years. The results remain qualitatively similar, showing a positive effect on short-term earnings growth and a higher likelihood of continued employment. The former, however, dissipates over time.

***Additional controls***

We can extend our baseline specification to more rigorously control for business cycle fluctuations. While the baseline already includes year and month fixed effects to account for business cycle and seasonal variations that could influence labor market outcomes, a more aggressive approach would incorporate year $\times$ month fixed effects. This would allow us to capture all variations affecting workers uniformly within the same month, further isolating the impact of our instrument. Additionally, we can control for the aggregate leave-one-out average of the outcomes, $\overline{Y}_{-i,r,t+h}$. This measure is constructed using the full sample of workers aged 18 to 49 years old, 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 $\overline{Y}_{-i,r,t+h}$ measured for all workers in the labor market). This control further ensures that economic conditions are held constant, such that our instrument only captures transitory variation in the availability of open-ended positions uncorrelated with general business-cycle trends.

The results of these robustness checks are shown in Appendix Figures A.7.1 and A.7.2. While there is a slight attenuation in the estimated coefficients compared to the baseline specification, the impact is minimal, and the overall conclusions from the previous sections remain essentially unaffected.

# 9 Discussion & 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 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 job or other attributes on the demand side.

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 open-ended contracts experience higher earnings growth than workers who instead spent time in fixed-term positions. However, such differences in earnings growth may reflect not only differences in returns between contract types but also heterogeneity between workers.

A crucial test to discriminate between these explanations is the pattern of earnings growth *before* workers enter a permanent contract. Using an event study approach, we reject the assumption of "parallel pre-trends", as workers who switch from a fixed-term into an open-ended contract experience high earnings growth even before that switch: 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. A fixed effects approach accounting for time-constant wage differences between workers, as typically used to account for the selection of worker into firms or regions, is therefore not sufficient to address selection into contract types.

We therefore propose a novel identification strategy to address the non-random sorting of workers into jobs. Using 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 labor supply side and transitory fluctuations in job creation on the demand side. Our proposed instrumental variable is uncorrelated to workers' characteristics and past employment history (instrument independence) but highly predictive of their probability to secure a permanent position (instrument relevance). This allows us to study the causal effect of entering a permanent contract for "compliers", i.e. workers who are on the margin of finding a permanent contract and whose contract status is sensitive to labor market conditions.

We find that workers securing a permanent contract experience a large gain in earnings in the short run. These earning gains are primarily due to more stable employment relationships; while workers in permanent contracts are employed uninterruptedly, workers in the control group tend to experience breaks in their employment status when switching from one fixed-term contract to the next. As a result, workers in permanent contracts gain more work experience, especially more experience in open-ended positions, than workers who do not find a permanent position as soon as their fixed-term contract ends.

However, these initial earnings gains shrink over time. As a qualitative pattern, this is not surprising, as it reflects a catching-up process in the control group: some workers who initially did not find a permanent position become increasingly likely to find such position as time goes by; and once they do, their employment relationships and therefore earnings stabilize. After five years, the probability to be in a permanent position is only 22 p.p. lower in the control than the treatment group.

What is surprising is that the initial earning gains vanish *entirely* over time, as the estimated effect of entering a permanent contract on wages reaches zero after five years. This absence of long-run effects on earnings is striking, given that the treated workers do accumulate substantially more work experience than the control group: five years after entering a permanent contract, treated workers have accumulated 13 more months work experience, and spent 31 more months in permanent contracts, than the control group, who did not secure an open-ended contract immediately after the expiration of their fixed-term contract.

One potential explanation for this pattern is that the former are substantially more mobile; workers in permanent contracts tend to remain in the same region and industry, whereas workers in fixed-term contracts move more frequently to job opportunities in other regions or new industries. While workers in permanent positions accumulate more experience, the stability inherent to these contracts could come, to some extent, at the expense of job flexibility and long-run career progression. This would be the case if for instance, workers were to forgo growth prospects in different regions or sectors in order to maintain their stable positions.

To sum up, our findings do not support the idea that shifting "marginal" workers from fixed-term into permanent contracts would automatically increase their long-run productivity or wages; instead, the positive correlation between experience in permanent contracts and wages reflects selection – securing a permanent contract is, to an important extent, a consequence of a favorable career progression, rather than its cause.

These findings have implications for policy. Neglecting the dynamic selection issue may lead to suboptimal policy recommendations, especially in segmented labor markets where such selection can be easily confounded with the labor market structure. In Spain and other European labor markets, the high prevalence of fixed-term contracts has been assessed as a potential cause for low productivity growth (Bentolila et al., 2019; Dolado et al., 2016). However, our findings suggest that merely shifting workers from fixed-term to permanent contracts may not yield significant long-term benefits on wages, a proxy for productivity. What is more, shifting workers into permanent positions might reduce workers' geographic and inter-industry mobility, reinforcing another structural problem of European labor markets (Blanchard and Katz, 1992). Taken together, our findings serve as a cautionary tale for policy design.

While our focus here is on dual labor markets and the selection into contract types, the methodology we propose can be applied more generally. 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.

Table 3: Heterogeneity of the effect of Open-Ended Contract on Earnings and Employment Outcomes

| | First Stage | Earnings | Cum. Earnings | Employment | Experience OEC | Change Region | Change Sector |
|---|---|---|---|---|---|---|---|
| **Panel A: Short-term effects (12 months)** | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Education** | | | | | | | |
| Below secondary | 0.069*** | 0.253 | 6.303*** | 0.183 | 8.939*** | -0.139 | -0.222* |
| Obs. 92,688 | (0.007) | (0.218) | (1.996) | (0.126) | (0.811) | (0.087) | (0.122) |
| At least secondary | 0.082*** | -0.091 | 2.153 | 0.116 | 9.851*** | 0.001 | -0.413*** |
| Obs. 73,200 | (0.009) | (0.236) | (2.241) | (0.109) | (0.843) | (0.089) | (0.115) |
| **Gender** | | | | | | | |
| Female | 0.071*** | -0.021 | 5.163** | 0.163 | 8.801*** | -0.047 | -0.165 |
| Obs. 71,980 | (0.009) | (0.265) | (2.435) | (0.139) | (0.983) | (0.089) | (0.137) |
| Male | 0.077*** | 0.168 | 3.604* | 0.128 | 9.778*** | -0.082 | -0.395*** |
| Obs. 93,908 | (0.007) | (0.199) | (1.864) | (0.105) | (0.714) | (0.085) | (0.108) |
| **Age** | | | | | | | |
| $Age < 30$ | 0.077*** | 0.230 | 5.541*** | 0.205* | 8.873*** | -0.077 | -0.273** |
| Obs. 87,463 | (0.008) | (0.213) | (2.048) | (0.107) | (0.775) | (0.081) | (0.110) |
| $30 < Age$ | 0.070*** | -0.102 | 2.318 | 0.071 | 9.921*** | -0.041 | -0.345*** |
| Obs. 78,425 | (0.008) | (0.240) | (2.130) | (0.133) | (0.880) | (0.093) | (0.127) |
| **Earnings at baseline** | | | | | | | |
| Low Earnings | 0.062*** | -0.065 | 1.337 | 0.226 | 9.132*** | 0.011 | -0.229 |
| Obs. 82,931 | (0.008) | (0.351) | (3.320) | (0.153) | (1.072) | (0.104) | (0.151) |
| High Earnings | 0.086*** | 0.194* | 6.213*** | 0.081 | 9.366*** | -0.140* | -0.379*** |
| Obs. 82,957 | (0.008) | (0.104) | (1.018) | (0.0941) | (0.669) | (0.0765) | (0.0967) |
| **Share of FTC at initial sector** | | | | | | | |
| Low FTC sector | 0.074*** | 0.135 | 7.133*** | 0.241** | 8.914*** | -0.037 | -0.260** |
| Obs. 83,928 | (0.008) | (0.171) | (1.601) | (0.106) | (0.819) | (0.074) | (0.107) |
| High FTC | 0.074*** | 0.065 | 1.193 | 0.055 | 9.790*** | -0.107 | -0.360*** |
| Obs. 81,960 | (0.007) | (0.277) | (2.632) | (0.133) | (0.850) | (0.102) | (0.134) |
| **Panel B: Long-term effects (48 months)** | | | | | | | |
| | First Stage | Earnings | Cum. Earnings | Employment | Experience OEC | Change Region | Change Sector |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Education** | | | | | | | |
| Below Secondary | 0.073*** | 0.077 | 14.030* | 0.074 | 19.230*** | -0.014 | -0.172 |
| Obs. 82,495 | (0.007) | (0.256) | (7.773) | (0.131) | (3.945) | (0.106) | (0.137) |
| At least Secondary | 0.081*** | -0.442 | -7.205 | 0.183* | 27.360*** | -0.087 | -0.435*** |
| Obs. 64,411 | (0.009) | (0.355) | (9.571) | (0.111) | (4.045) | (0.107) | (0.123) |
| **Gender** | | | | | | | |
| Female | 0.069*** | -0.324 | 4.503 | 0.208 | 19.240*** | -0.023 | -0.244* |
| Obs. 63,831 | (0.009) | (0.378) | (9.676) | (0.142) | (4.835) | (0.111) | (0.147) |
| Male | 0.082*** | -0.081 | 3.074 | 0.063 | 25.550*** | -0.052 | -0.316*** |
| Obs. 83,075 | (0.00763) | (0.256) | (7.808) | (0.109) | (3.479) | (0.101) | (0.118) |
| **Age** | | | | | | | |
| $Age < 30$ | 0.080*** | -0.319 | -0.634 | 0.031 | 19.910*** | -0.059 | -0.276** |
| Obs. 79,524 | (0.00820) | (0.296) | (8.358) | (0.112) | (3.555) | (0.0990) | (0.118) |
| $30 < Age$ | 0.072*** | 0.020 | 9.046 | 0.248* | 26.460*** | -0.008 | -0.321** |
| Obs. 67,382 | (0.009) | (0.302) | (8.628) | (0.133) | (4.655) | (0.114) | (0.144) |
| **Earnings at baseline** | | | | | | | |
| Low Earnings | 0.063*** | -0.472 | -10.080 | 0.217 | 14.470*** | 0.075 | -0.137 |
| Obs. 72,693 | (0.008) | (0.455) | (13.450) | (0.157) | (4.904) | (0.129) | (0.166) |
| High Earnings | 0.091*** | 0.060 | 13.260*** | 0.071 | 28.900*** | -0.138 | -0.421*** |
| Obs. 74,213 | (0.009) | (0.177) | (4.333) | (0.097) | (3.424) | (0.092) | (0.107) |
| **Share of FTC at initial sector** | | | | | | | |
| Low FTC sector | 0.076*** | -0.053 | 10.57 | 0.120 | 19.56*** | -0.005 | -0.310*** |
| Obs. 74,595 | (0.008) | (0.231) | (6.790) | (0.112) | (4.013) | (0.0946) | (0.119) |
| High FTC sector | 0.076*** | -0.272 | -2.876 | 0.139 | 26.72*** | -0.097 | -0.283** |
| Obs. 72,311 | (0.008) | (0.374) | (10.35) | (0.134) | (4.091) | (0.120) | (0.142) |

**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 described in Figure 5 notes. Robust standard errors are in parentheses. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

# References

Abowd, J. M., F. Kramarz, and D. N. Margolis (1999): "High Wage Workers and High Wage Firms," *Econometrica*, 67, 251–333.

Aguirregabiria, V. and C. Alonso-Borrego (2014): "Labor Contracts and Flexibility: Evidence from a Labor Market Reform in Spain," *Economic Inquiry*, 52, 930–957.

Albanese, A. and G. Gallo (2020): "Buy Flexible, Pay More: The Role of Temporary Contracts on Wage inequality," *Labour Economics*, 64, 101814.

Albert, C., C. García-Serrano, and V. Hernanz (2005): "Firm-provided Training and Temporary Contracts," *Spanish Economic Review*, 7, 67–88.

Altonji, J., L. Kahn, and J. Speer (2016): "Cashier or Consultant? Entry Labor Market Conditions, Field of Study, and Career Success," *Journal of Labor Economics*, 34, S361–S401.

Arellano-Bover, J. (2024): "Career Consequences of Firm Heterogeneity for Young Workers: First Job and Firm Size," *Journal of Labor Economics*, 42, 549–589.

Arellano-Bover, J. and F. Saltiel (2021): "Differences in on-the-job learning across firms," *Working Paper*.

Auer, W. and N. Danzer (2016): "Fixed-term employment and fertility: Evidence from German micro data," *CESifo Economic Studies*, 62, 595–623.

Bentolila, S., P. Cahuc, J. J. Dolado, and T. Le Barbanchon (2012): "Two-Tier Labour Markets in the Great Recession: France Versus Spain," *The economic journal*, 122, F155–F187.

Bentolila, S. and J. J. Dolado (1994): "Labour Flexibility and Wages: Lessons from Spain," *Economic policy*, 9, 53–99.

Bentolila, S., J. J. Dolado, and J. F. Jimeno (2019): "Dual Labour Markets Revisited," .

——— (2020): "Dual Labour Markets Revisited," *Oxford Research Encyclopedia of Economics and Finance*.

Blanchard, O. and L. Katz (1992): "Regional Evolutions," *Brookings Papers on Economic Activity*, 23, 1–76.

Bratti, M., M. Conti, and G. Sulis (2021): "Employment Protection and Firm-Provided Training in Dual Labour Markets," *Labour Economics*, 69, 101972.

Cabrales, A., J. J. Dolado, and R. Mora (2017): "Dual Employment Protection and (lack of) On-the-Job Training: PIAAC Evidence for Spain and Other European Countries," *SERIEs*, 8, 345–371.

Cahuc, P., O. Charlot, and F. Malherbet (2016): "Explaining the Spread of Temporary Jobs and its Impact on Labor Turnover," *International Economic Review*, 57, 533–572.

Card, D., A. R. Cardoso, J. Heining, and P. Kline (2018): "Firms and Labor Market Inequality: Evidence and Some Theory," *Journal of Labor Economics*, 36, S13 – S70.

Card, D., J. Heining, and P. Kline (2013): "Workplace Heterogeneity and the Rise of West German Wage Inequality," *The Quarterly Journal of Economics*, 128, 967–1015.

Card, D., J. Rothstein, and M. Yi (2023): "Location, Location, Location," *National Bureau of Economic Research.*

Conde-Ruiz, J. I., M. García, L. A. Puch, and J. Ruiz (2023): "Reforming Dual Labor Markets: "Empirical" or "Contractual" Temporary Rates?" Tech. rep., FEDEA.

Dolado, J. J. (2017): *European Union Dual Labour Markets: Consequences and Potential Reforms*, Cambridge University Press, 73–112.

Dolado, J. J., S. Ortigueira, and R. Stucchi (2016): "Does Dual Employment Protection affect TFP? Evidence from Spanish Manufacturing Firms," *SERIEs*, 7, 421–459.

Gagliarducci, S. (2005): "The Dynamics of Repeated Temporary Jobs," *Labour Economics*, 12, 429–448.

Galacho, E. R. (2006): "Las reformas laborales en España (1977-2002)," *Filosofía, política y economía en el Laberinto*, 7–22.

Garcia-Louzao, J., L. Hospido, and A. Ruggieri (2023): "Dual Returns to Experience," *Labour Economics*, 80, 102290.

García-Pérez, J. I., I. Marinescu, and J. Vall Castello (2019): "Can fixed-term contracts put low skilled youth on a better career path? Evidence from Spain," *The Economic Journal*, 129, 1693–1730.

García-Pérez, J. I. and F. Muñoz-Bullón (2011): "Transitions into Permanent Employment in Spain: An Empirical Analysis for Young Workers," *British Journal of Industrial Relations*, 49, 103–143.

Gathmann, C. and U. Schönberg (2010): "How General is Human Capital? A Task-Based Approach," *Journal of Labor Economics*, 28, 1–49.

Güell, M. and B. Petrongolo (2007): "How Binding are Legal Limits? Transitions from Temporary to Permanent Work in Spain," *Labour Economics*, 14, 153–183.

Hershbein, B. J. (2012): "Graduating High School in a Recession: Work, Education, and Home Production," *The BE Journal of Economic Analysis & Policy*, 12.

Kahn, L. B. (2010): "The Long-Term Labor Market Consequences of Graduating from College in a Bad Economy," *Labour Economics*, 17, 303–316.

Kahn, L. M. (2016): "The Structure of the Permanent Job Wage Premium: Evidence from Europe," *Industrial Relations: A Journal of Economy and Society*, 55, 149–178.

Kambourov, G. and I. Manovskii (2009): "Occupational Mobility and Wage Inequality," *The Review of Economic Studies*, 76, 731–759.

Llull, J. and R. A. Miller (2018): "Internal Migration and Work Experience in Dual Labor Markets," *Unpublished Manuscript*.

Lopes, M. (2020): "Job Security and Fertility Decisions," *Available at SSRN 3543204*.

Manning, A. (2021): "Monopsony in Labor Markets: A Review," *ILR Review*, 74, 3–26.

Manning, A. and B. Petrongolo (2017): "How local are labor markets? Evidence from a spatial job search model," *American Economic Review*, 107, 2877–2907.

Marinescu, I. and R. Rathelot (2018): "Mismatch unemployment and the geography of job search," *American Economic Journal: Macroeconomics*, 10, 42–70.

Nieto, A. (2022): "Can Subsidies to Permanent Employment Change Fertility Decisions?" *Labour Economics*, 78, 102219.

Oreopoulos, P., T. Von Wachter, and A. Heisz (2012): "The Short and Long-Term Career Effects of Graduating in a recession," *American Economic Journal: Applied Economics*, 4, 1–29.

Pesola, H. (2011): "Labour Mobility and Returns to Experience in Foreign Firms," *The Scandinavian Journal of Economics*, 113, 637–664.

Roca, J. D. L. and D. Puga (2017): "Learning by Working in Big Cities," *The Review of Economic Studies*, 84, 106–142.

Schwandt, H. and T. Von Wachter (2019): "Unlucky Cohorts: Estimating the Long-Term Effects of Entering the Labor Market in a Recession in Large Cross-Sectional Data Sets," *Journal of Labor Economics*, 37, S161–S198.

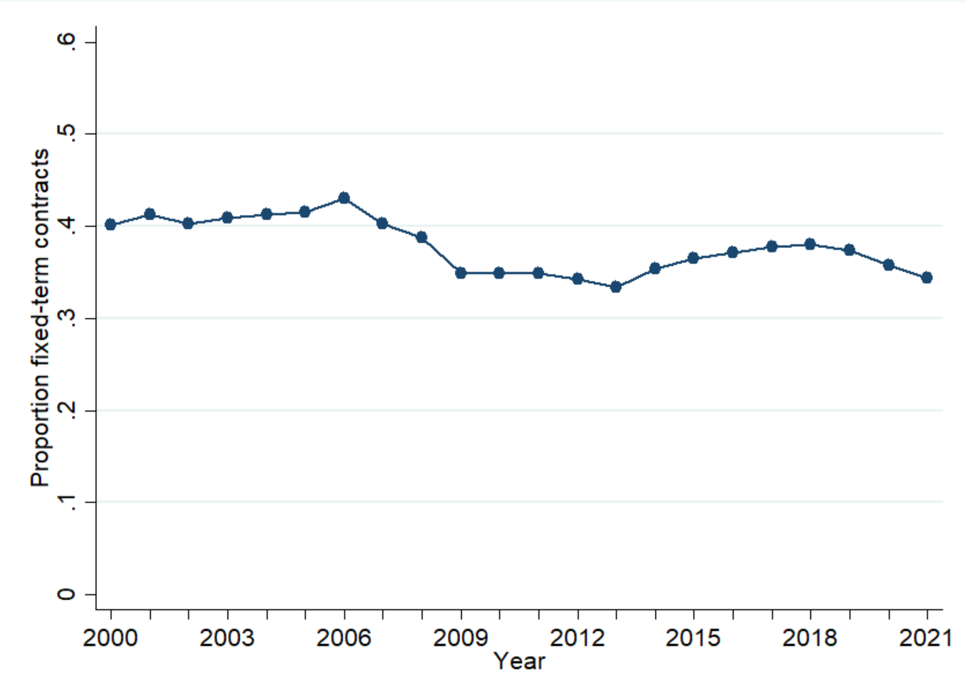
Wachter, T. v. and S. Bender (2006): "In the Right Place at the Wrong Time: The Role of Firms and Luck in Young Workers' Careers," *American Economic Review*, 96, 1679–1705.

# Appendix

## A Supplementary Figures

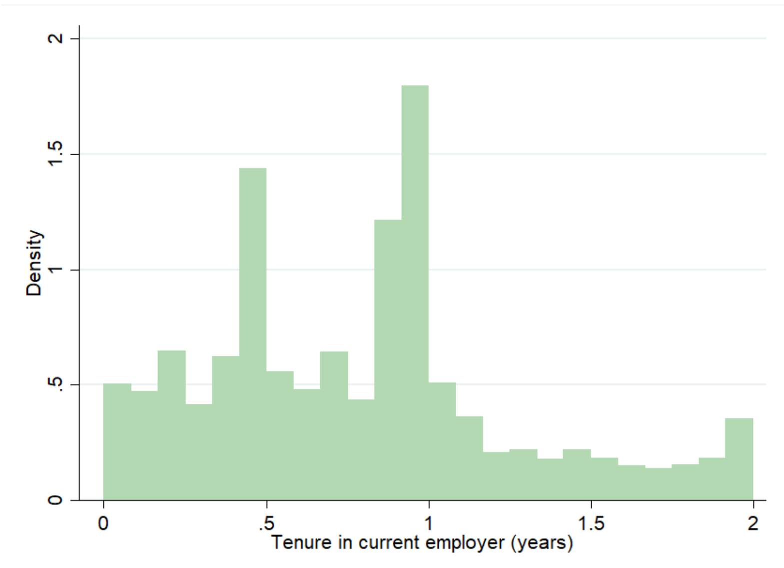
### A.1 Fixed-term contracts in Spain

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



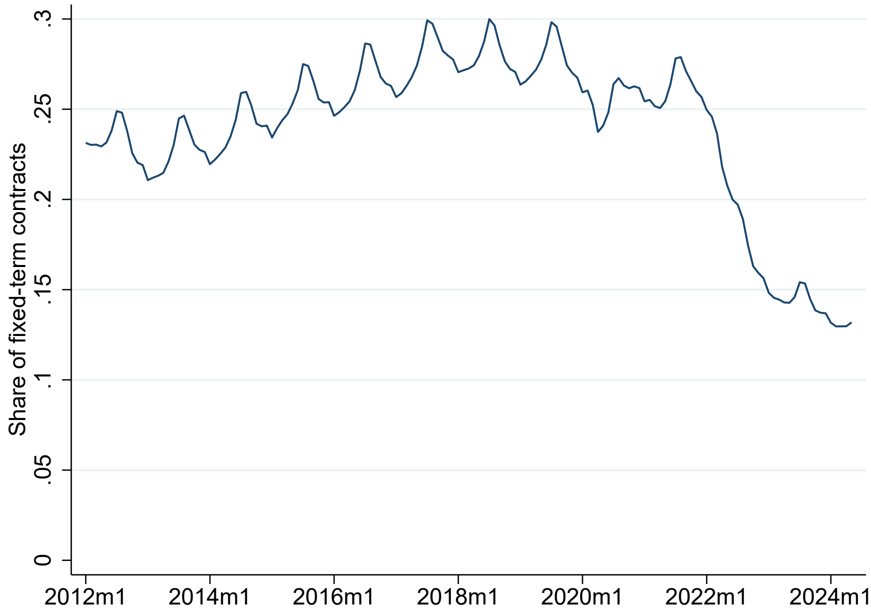

**Source:** MCVL

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



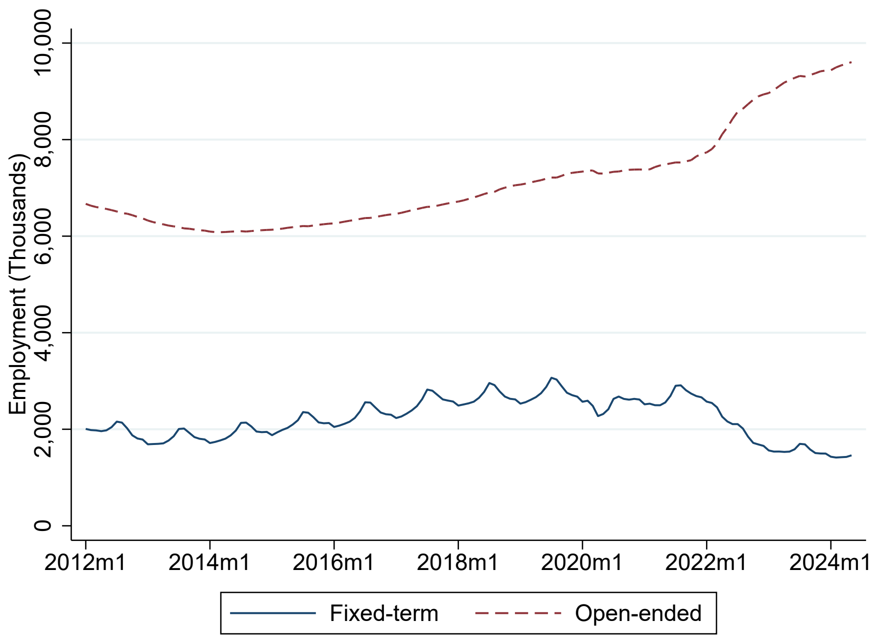

**Notes:** Distribution of maximum tenure in fixed-term contracts 1998-2021.

Figure A.1.3: Share of workers in fixed-term contracts. 2012-2024




**Notes:** Share of fixed-term contract workers calculated as the number of fixed-term contract workers divided by total fixed-term and open-ended contract workers, all in full-time employment. Source: BBDD ESTADÍSTICAS TGSS.

Figure A.1.4: Number of workers by type of contract. 2012-2024

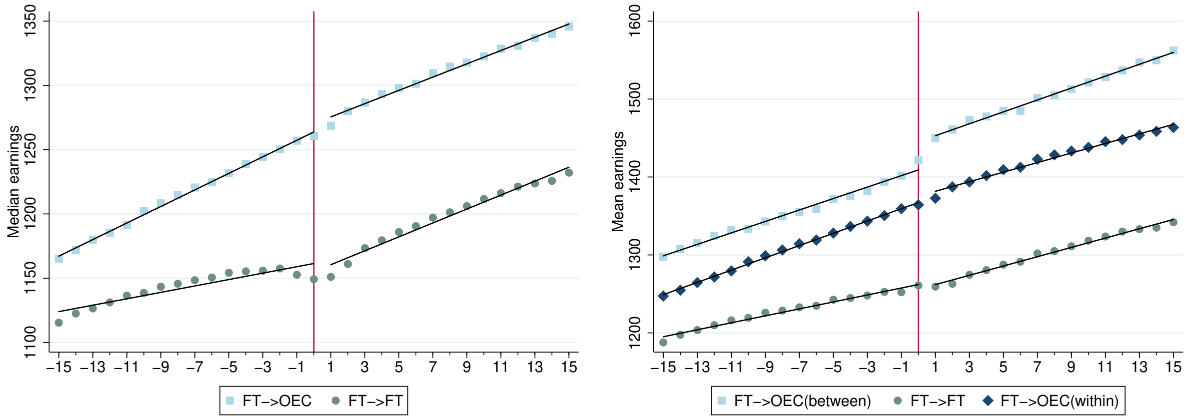



**Notes:** Workers in fixed-term and open-ended contracts from January 2012 to May 2024. Full-time employment. Source: BBDD ESTADÍSTICAS TGSS

## A.2 Selection: Additional Results

Figure A.2.1: Mean and median earnings before and after a contract change, by destination contract

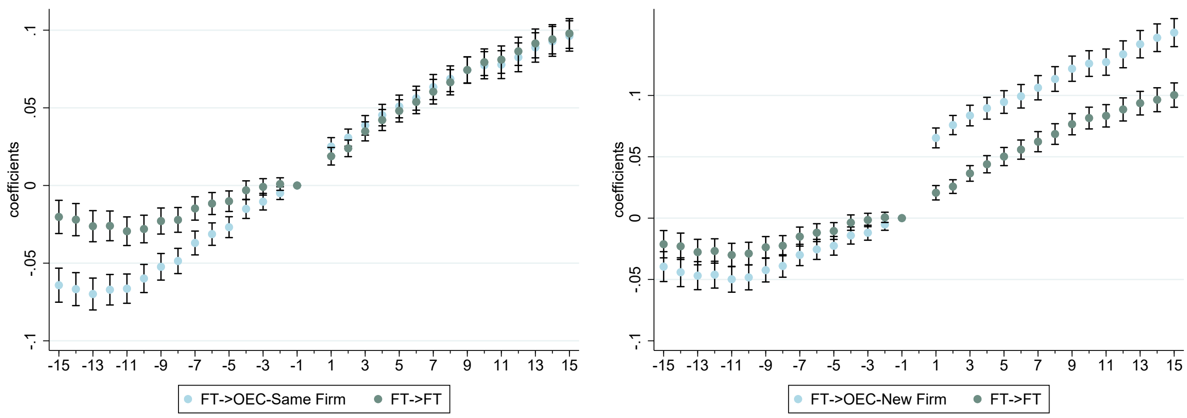
(a) Median earnings (b) Mean earnings




*Notes:* The figure shows Spanish workers' mean and median earnings from 1998 to 2020 in the final month of a fixed-term contract and 15 periods before and after that transition. Workers are categorized into two groups based on the subsequent contract: $FT \rightarrow OEC$ (transitioning to an open-ended contract) or $FT \rightarrow FT$ (transitioning to another fixed-term contract). Panel (a) presents the median earnings of workers transitioning to an open-ended or fixed-term contract in event time 1. Panels (b) distinguish between transitions to an open-ended contract in a different firm ($FT \rightarrow OEC - SameFirm$) and transitions to an open-ended contract within the same firm ($FT \rightarrow OEC - NewFirm$).

Figure A.2.2: Evolution of earnings: switching to a new contract by firm

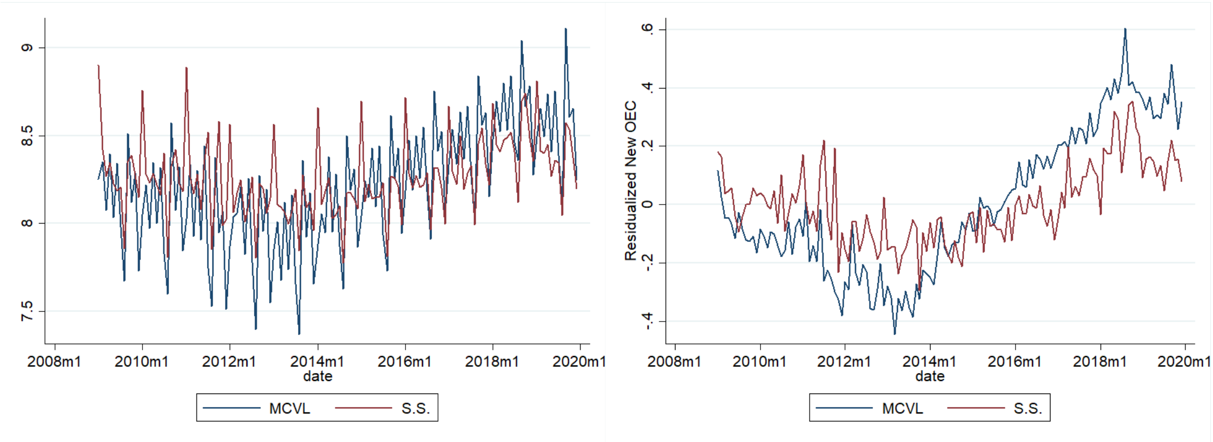
(a) FT-OEC Within Firm (b) FT-OEC New Firm




**Notes:** The figure shows event time coefficients estimated from Equation 2 for workers transitioning to open-ended contracts or a new fixed-term contract. The exercise is described in Figure 3 notes. Panel (a) presents fixed-term to fixed-term transitions (FT-FT) along with fixed-term to open-ended transitions (FT-OEC) that occur *within* the same firm. Panel (b) presents FT-FT transitions along with FT-OEC transitions to a *new* firm. Controls include interactions of event time with educational attainment and sector-fixed effects.

Figure A.3.1: New OEC and Total OEC from MCVL and SS records

(a) Baseline (b) Residualized



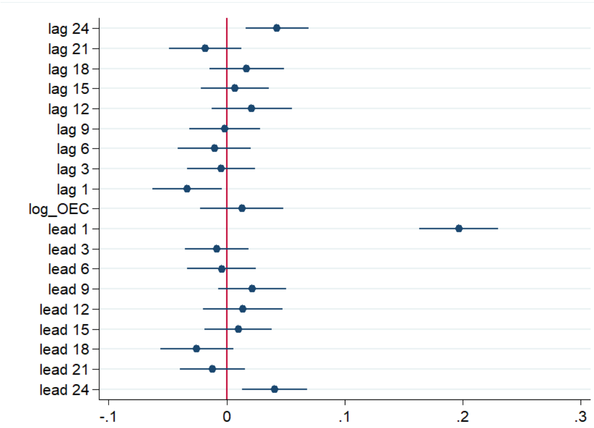

**Notes:** Sum of NewOEC obtained from Social Security records ($OEC_{Total}$) and from the MCVL. Panel (a) displays the monthly sum of New open-ended contracts from both data sources. Panel (b) residuals the sum of new Open-Ended contracts (OEC) by subtracting the variation explained by month-fixed effects.

## A.3 Social Security records

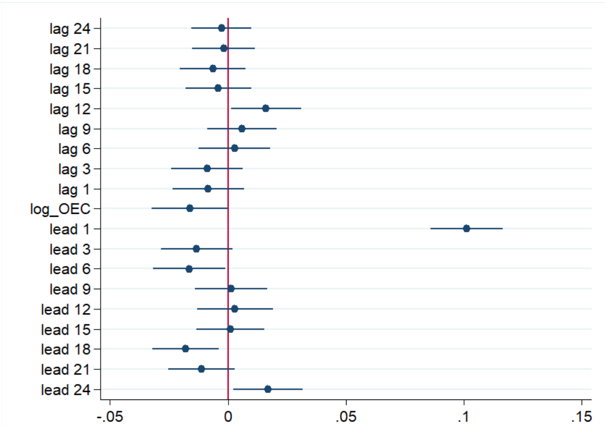
## A.4 First Stage

Figure A.4.1: First stage: National, Sectoral and Sector x Region Instrument

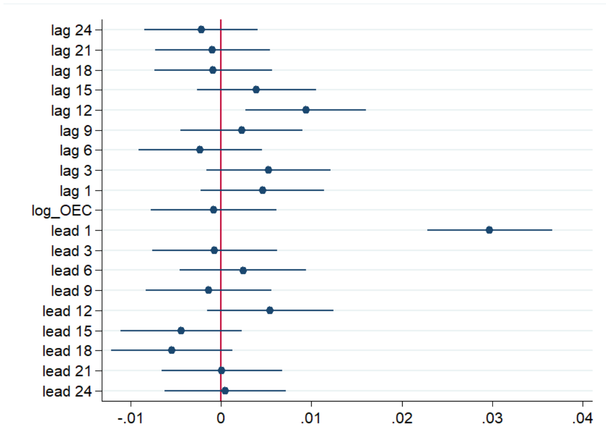
(a) National




(b) Sectoral




(c) Sector $\times$ Region




**Notes:** The sample consists of workers who were in the last month of a fixed-term contract in event period $h = 0$, with at least 0.8 but less than 1.2 years of tenure. The coefficients correspond to the effect of leads and lags of the *log* of new open-ended contracts on the probability of switching to an open-ended contract in $t + 1$. Panel (a) employs variation in opening of permanent positions at the national level. Panel (b) exploits the opening of permanent positions by sector. Panel (c) exploits province by sectoral variation. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and educational attainment, experience and experience squared at baseline as well as leads and lags of the *log* of new fixed-term contracts.

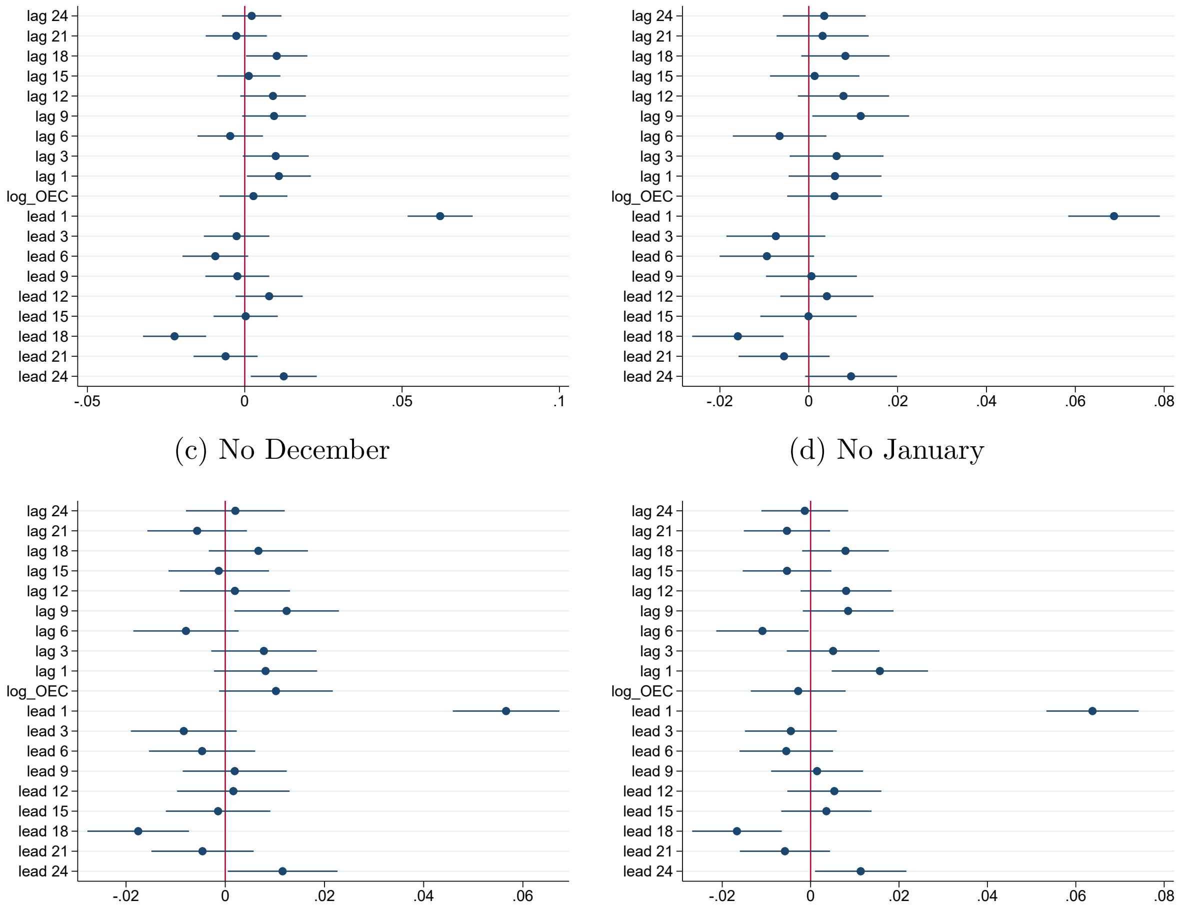
# A.5 Robustness: Job Seasonality

Figure A.5.1: Regional Instrument

Removing specific months

(a) No August (b) No September

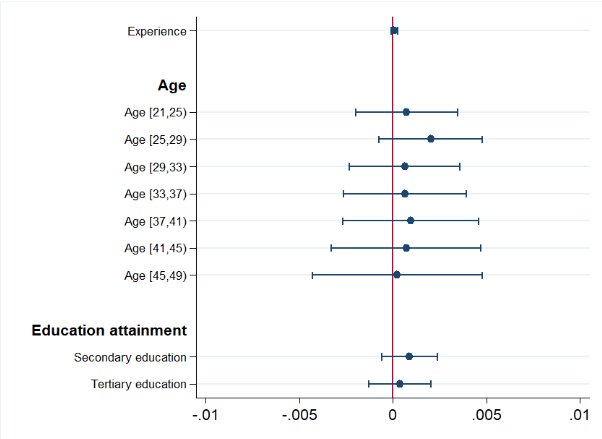
(c) No December (d) No January



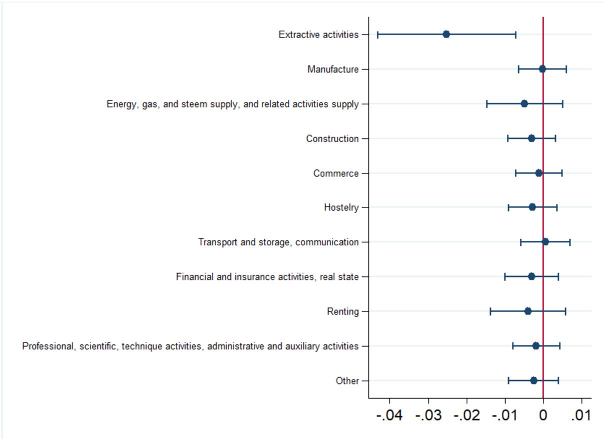

**Notes:** Baseline sample restrictions and empirical specification are described in Figure 4 notes. Panel (a) excludes observations from August. Panel (b) excludes observations from September. Panel (c) excludes observations from December. Panel (d) excludes observations from January.

## A.6 Instrument Independence

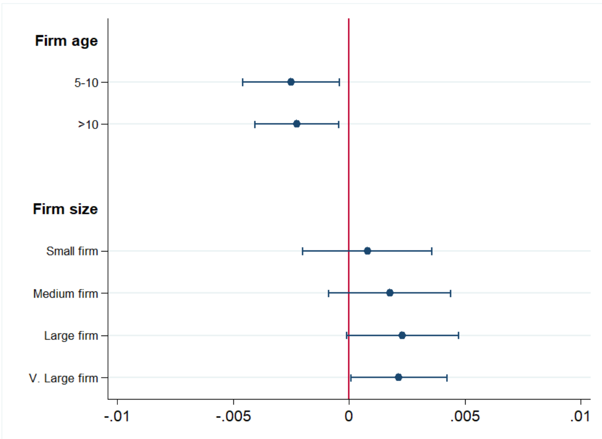
Figure A.6.1: Effect of individual characteristics and sector on $logOEC_{t+1}$




(a) Individual characteristics




(b) Sector




(c) Firm characteristics

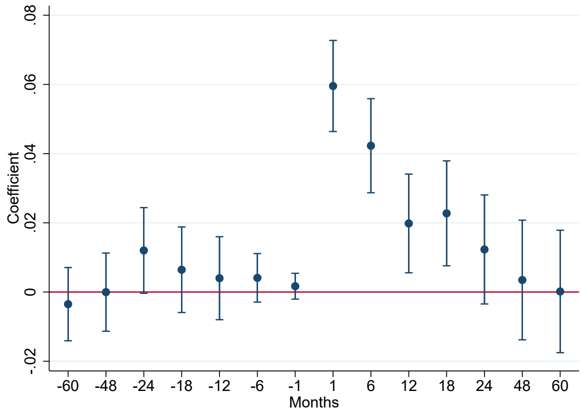
**Notes:** All regressions include leads and lags of the *log* of open-ended contracts, year, month, and province fixed effects. For this exercise we standardize the instrument (mean zero and standard deviation one).

## A.7 Robustness: Reduced-Form Alternative Specifications

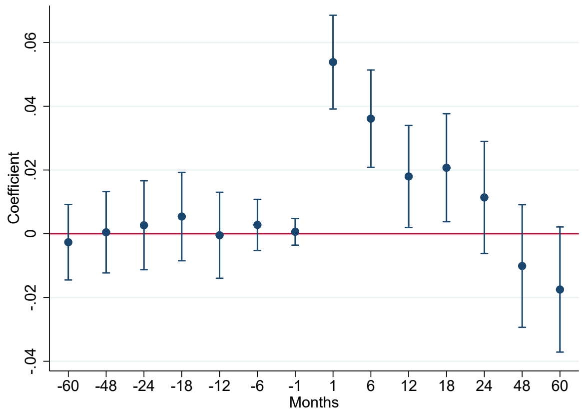
Figure A.7.1: Effect of OEC regional shock on earnings growth

Alternative Specifications

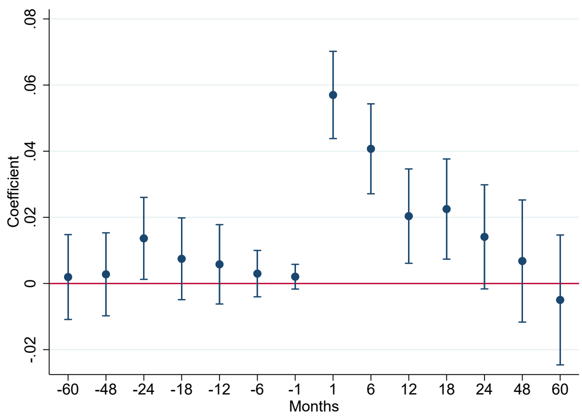
(a) Baseline



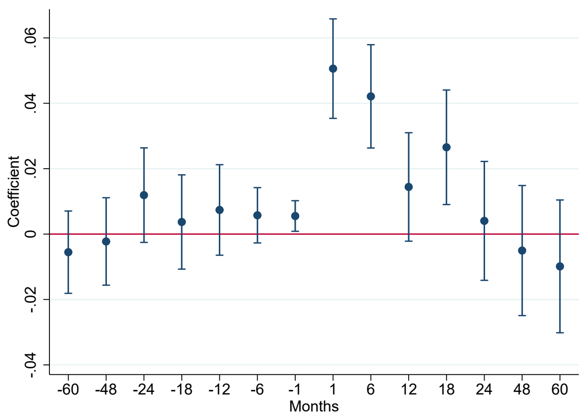

(b) Date FE




(c) Aggregate average earnings growth control




(d) Total new contracts control

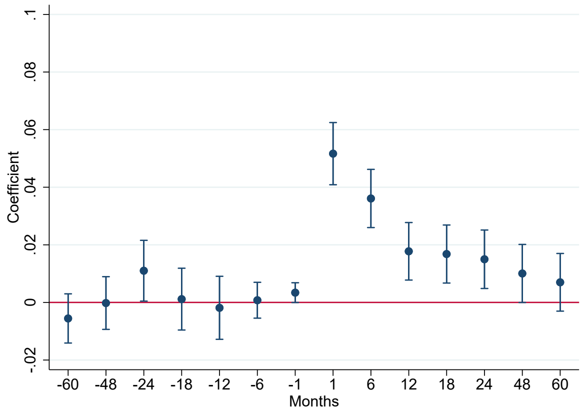



**Notes:** Impact of the first lead of the *log* of new open-ended in the region on individual earnings growth. Baseline sample restrictions and empirical specification are described in Figure 5 notes. Panel (a) presents the baseline specification from Figure 5 based on Equation 4. Panel (b) adds month x year fixed effects. Panel (c) includes the aggregate average outcome as control, following the specification of Equation 5. Panel (d) controls for the *log* of total new contracts (sum of fixed-term and open-ended).

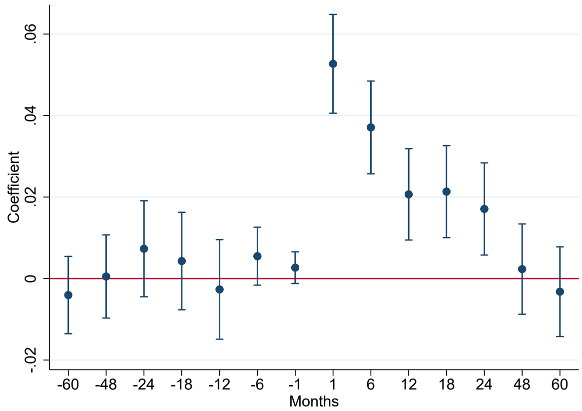
Figure A.7.2: Effect of OEC regional shock on employment

Alternative Specifications

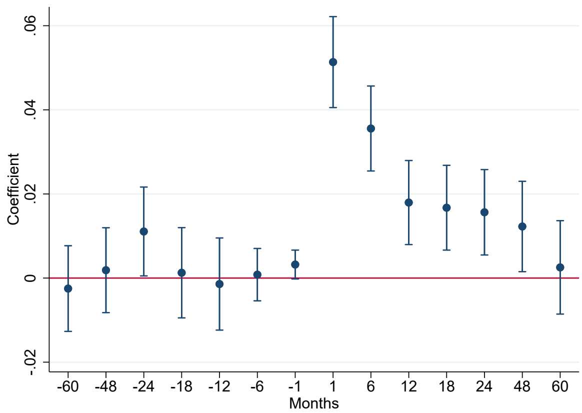
(a) Baseline



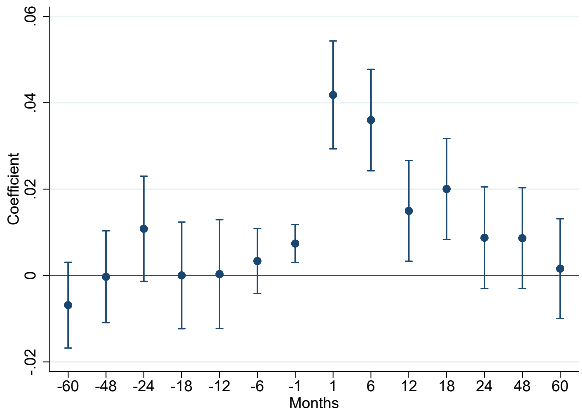

(b) Date FE




(c) Aggregate employment control




(d) Total new contracts control

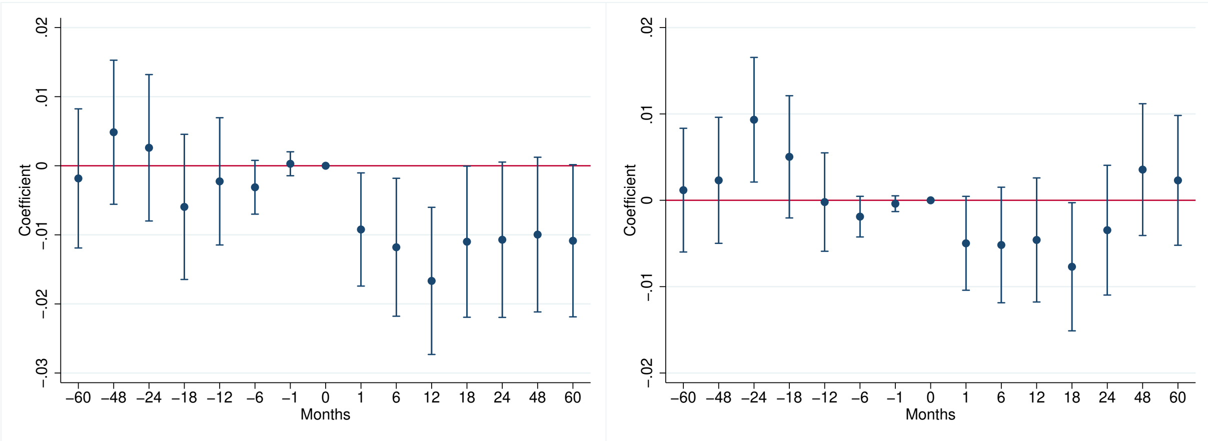



**Notes:** Impact of the first lead of the *log* of new open-ended in the region on individual employment probability. Baseline sample restrictions and empirical specification are described in Figure 5 notes. Panel (a) presents the baseline specification from Figure 5 based on Equation 4. Panel (b) adds month x year fixed effects. Panel (c) includes the aggregate average outcome as control, following the specification of Equation 5. Panel (d) controls for the *log* of total new contracts (sum of fixed-term and open-ended).

## A.8 Robustness: Additional mobility outcomes

Figure A.8.1: Effect of new open-ended contracts in the region on workers' mobility

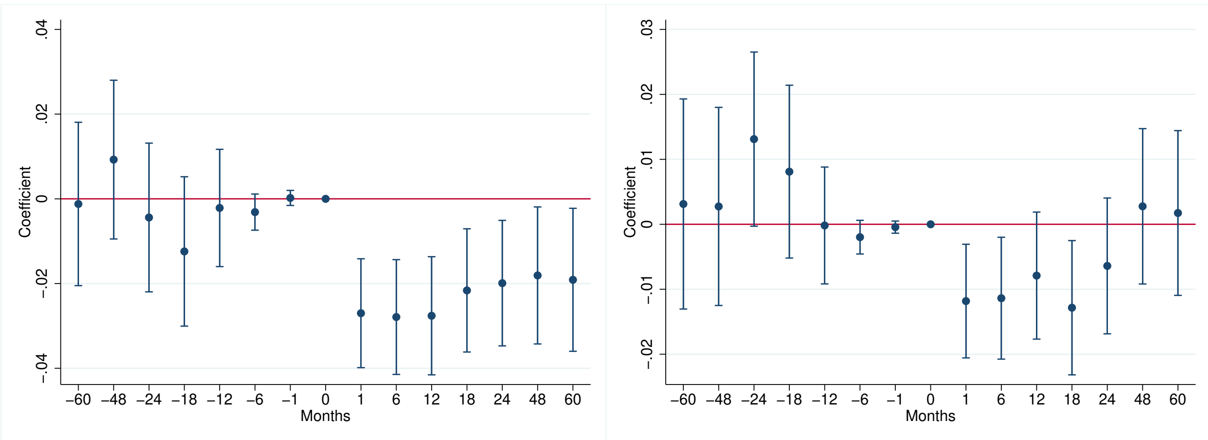
(a) Probability to change sector (b) Probability to change region




**Notes:** Baseline sample restrictions and empirical specification are described in Figure 5 notes. Regional and sectoral moves are measured using an indicator variable that equals one if a worker is employed in a different sector or province at time $t + h$ compared to their baseline status.

Figure A.8.2: Effect of new open-ended contracts in the region on workers' mobility

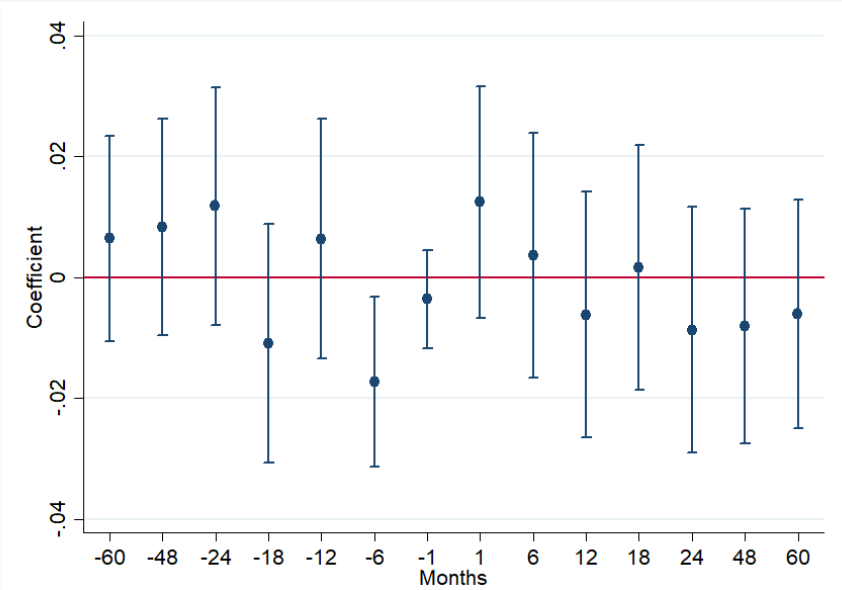
(a) Probability to change sector (b) Probability to change region




**Notes:** Baseline sample restrictions and empirical specification are described in Figure 5 notes. Each specification is further restricted to employed workers during period $t + h$.

## A.9 Robustness: Exclusion Restriction

Figure A.9.1: Effect of OEC regional shock on employment: restriction to fixed-term employment

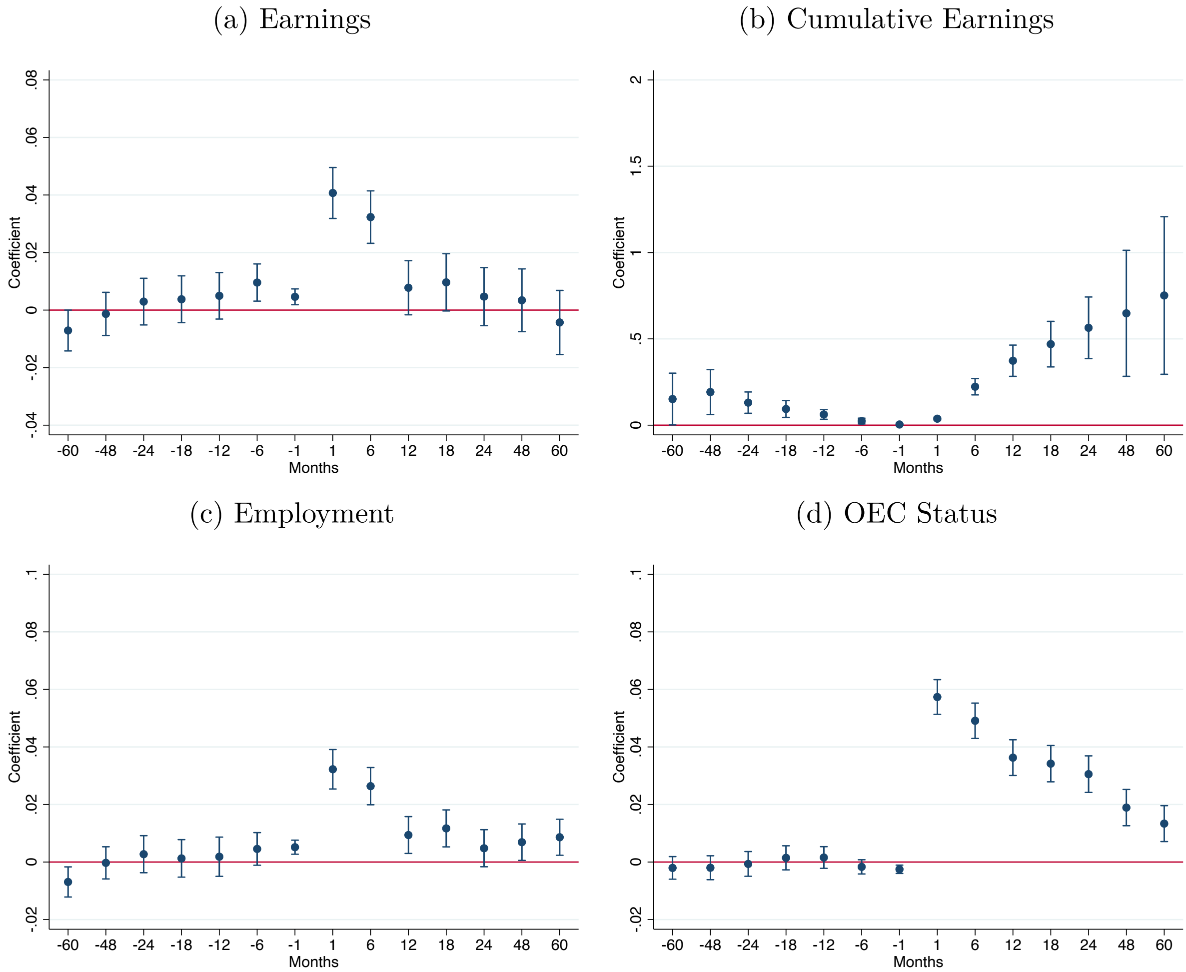



**Notes:** The sample consists of workers who were in the last month of a fixed-term contract in event period $h = 0$, with at least 0.8 but less than 1.2 years of tenure. We restrict the sample to those workers who *do not* start an open-ended position in period $t + h$. The coefficients correspond to the effect of the first lead of the *log* of new open-ended contracts in the region on the probability of employment. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and educational attainment, experience and experience squared at baseline as well as leads and lags of the *log* of new fixed-term contracts.

## A.10 Alternative Tenure Restrictions

Figure A.10.1: Effect of OEC regional shock on earnings. Tenure 0.4-1.2 years

(a) Earnings (b) Cumulative Earnings (c) Employment (d) OEC Status




**Notes:** The sample consists of workers who were in the last month of a fixed-term position in event period $h = 0$, with at least 0.4 but less than 1.2 years of tenure. Period 1998-2017. The coefficients correspond to the effect of the first lead of the *log* number of new permanent contracts ($logOEC$) on each outcome. All regressions control for the leads and lags of $logOEC$ as well as the *log* of the number of new fixed-term contracts. Additional controls: year and month FE, province FE, sector FE, gender, interactions of age FE and educational attainment, experience and experience squared at baseline.

# B Supplementary Tables

## B.1 Descriptive Statistics

Table B.1.1: Descriptive statistics of estimation sample

| | Mean | Standard Deviation |
|---|---|---|
| Age | 30.16 | 7.35 |
| Female | 0.43 | 0.49 |
| Education | | |
| *Below Secondary* | 0.56 | 0.50 |
| *Secondary* | 0.24 | 0.43 |
| *Tertiary* | 0.20 | 0.40 |
| Tenure | 0.98 | 0.09 |
| Experience | 6.19 | 5.22 |
| Earnings (EUR 2009) | 1,191.82 | 524.55 |
| log $OEC_{lead1}$ | 4.80 | 1.35 |
| Obs. | | 221,716 |

**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 1998 and 2017.

Table B.1.2: Descriptive statistics of the complete sample

| | Mean | Standard Deviation |
|---|---|---|
| Age | 34.51 | 7.74 |
| Female | 0.46 | 0.49 |
| Education | | |
| *Below Secondary* | 0.45 | 0.49 |
| *Secondary* | 0.25 | 0.44 |
| *Tertiary* | 0.29 | 0.46 |
| Tenure | 4.17 | 4.79 |
| Experience | 9.61 | 6.87 |
| Earnings (EUR 2009) | 1,633.20 | 994.30 |
| Monthly number of workers | 311,150 | 41,517 |
| Obs. | 80,972,294 | |

**Notes:** Descriptive statistics for the complete sample, which consists of workers aged 18-49 years who were in the last month of a fixed-term contract between 1998 and 2017.

Table B.3.1: 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 |

**Notes:** The table presents the regression coefficients of the logarithm of new open-ended Contracts by province from the MCVL on the logarithm of total OEC registered in the population records of the 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, respectively. $^{*}p < 0.05,^{**} p < 0.01,^{***} p < 0.001$.

## B.2 IV Estimates: Additional Results

Table B.2.1: Effect of switching to an OEC on OEC Experience

| | 12 months | | 60 months | |
|---|---|---|---|---|
| | OEC Status | Experience OEC | OEC Status | Experience OEC |
| | (1) | (2) | (3) | (4) |
| $p_{i,t+1}$ | 0.764*** | 11.254*** | 0.220*** | 35.151*** |
| | (0.054) | (0.437) | (0.067) | (3.108) |
| Obs. | 199,155 | 192,472 | 199,155 | 156,743 |
| R2 | 0.449 | 0.727 | 0.192 | 0.396 |

**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 described in Figure 5 notes. Robust standard errors in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

## B.3 Social Security records

# 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:

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

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, $\sigma_r$ is a province fixed effect, $\psi_t$ is a year-month fixed-effect, and $\varepsilon_{ict}$ 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.[23] 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.[24]

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.

[23]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%.

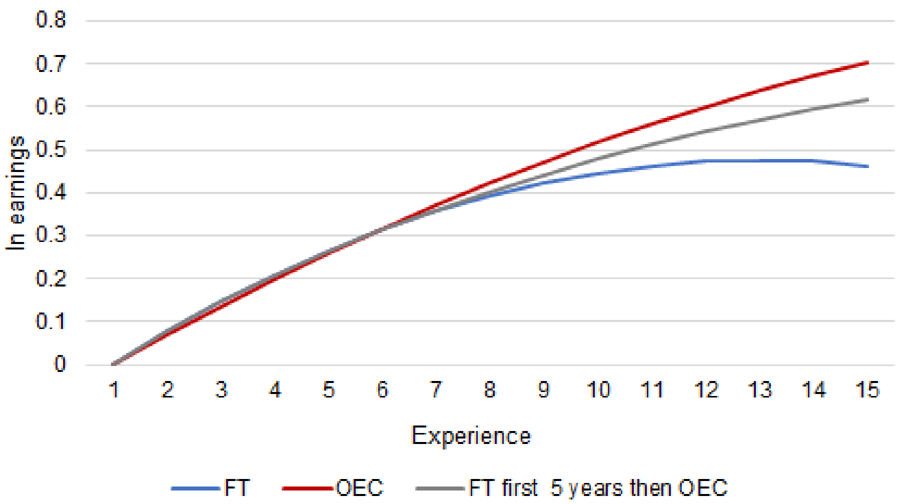
[24]One alternative that Garcia-Louzao et al. (2023) implement later on is to instrument experience and tenure using their deviations relative to the average computed within contract and match history of the worker. Additionally, they exploit supplementary instruments based on regional variations in 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 |

**Notes:** exp, $exp_{FT}$, and $exp_{OEC}$ refer to experience, experience in fixed-term, and experience in open-ended contracts, respectively. Controls include gender and occupation-skill group, interactions on educational attainment, sector, region and time fixed-effects, age, age squared, and interactions of tenure with an indicator for current fixed-term contract status. Clustered standard errors at the worker level. $^*p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$

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.