Quasi-Random Matches: Evidence from Dual Labor Markets[*]

María Alexandra Castellanos[†] Henry Redondo [‡] Jan Stuhler [§]

– Preliminary draft –

January 3, 2026

Abstract

A fast-growing literature studies how sorting into specific jobs, firms, or locations affects workers. However, a key challenge 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 contract durations 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 vs. permanent – affect workers' careers? We find that transitory variation in the opening of permanent contracts is highly predictive of individual contract upgrade probabilities. Securing a permanent position translates into higher employment and earnings growth in the short run. However, despite lasting gains in work experience, the earnings advantages disappear within five years. We argue that the negative effect of open-ended contracts on worker mobility may help to explain these findings.

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. Funding from MICIU/AEI (CEX2021-001181-M and PID2020-117525RB-I00) is gratefully acknowledged.

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

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

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

1 Introduction

A fast-growing literature studies how sorting into particular jobs, firms, or locations affects workers. For example, there has been much interest in the observation that pay premia vary across firms (Abowd et al. 1999), 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 to 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 effects 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 (OE) contracts – 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 recent labor reform in 2022), the country provides an interesting context. Moreover, we can exploit rich, matched employer-employee data from Social Security

[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).

records that track 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% for each year of experience in FTCs, they grow by 3.6% per year in OECs. These patterns are highly non-linear, and the gap is much greater for experienced than for 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 different workers experiencing different wage growth *irrespective* of 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 who 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 matching 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 (Roca and Puga, 2017; Card et al., 2023). We discuss several reasons why this might be the case. One factor is that the switch to open-ended contracts often occurs within firms and is based on more information than workers switching to other firms. As Bentolila et al. (2023) demonstrates using evidence from dual vocational training, firms frequently use temporary contracts as a screening mechanism, meaning that mobility between contract types depends on multiple factors beyond the worker's ability. Moreover, switching into an OEC within a firm can be a form of promotion, and promotions, of course, depend on the worker's recent performance. 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 into better contracts and experience 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 with 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 in a province is highly predictive of whether workers whose temporary contracts just expired find 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 and firm 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 irrespective of 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 to the fixed effects approach, which exploits another (their scale).

Apart from this methodological contribution, we add to the active literature on dual labor markets (Bentolila et al. 2020; Boeri and Garibaldi 2024). 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 of 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).

Using our instrumental variable strategy, we find that workers securing a permanent contract experience a large gain in earnings in the short run. These earnings 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 once 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 (Booth et al., 2002): some workers who initially did not find a permanent position become increasingly likely to find such a position as time goes by, and once they do, their employment relationships and therefore earnings stabilize. 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 treated workers accumulate substantially more work experience: five years after entering a permanent contract, treated workers have accumulated 13 more months of 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. Therefore, 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 to maintain their stable positions.

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 descriptive results from a Mincerian approach, Sections 5 and 6 discuss the selection problem and our identification strategy, respectively, and Section 7 analyzes the effect of contract upgrade in workers' career trajectory by evaluating a series of labor market outcomes. Section 9 provides additional robustness checks, and Section 10 concludes.

2 Institutional Background

Following the democratic transition, Spain's institutions underwent significant changes, including major labor market reforms. The Workers' Statute (Estatuto de los Trabajadores), enacted in 1980, established open-ended contracts as the default employment arrangement and limited temporary contracts to exceptional cases, such as seasonal work or short-term replacements. The Statute also introduced severance regulations, setting compensation for temporary contracts at 8 days of wages per year of service, compared to up to 45 days for permanent ones. Despite these provisions, the use of fixed-term contracts remained tightly restricted.[2]

The dual structure of the Spanish labor market arose in 1984 with the enactment of *Law 32/1984*, which liberalized the use of temporary contracts to stimulate job creation in response to the high unemployment. Fixed-term contracts, previously limited to seasonal activities, were opened to general use and quickly expanded. Employers could now choose between permanent and temporary positions, but the former carried substantially higher severance costs. Because the reform left the conditions of permanent contracts unchanged, temporary contracts became especially attractive to firms (Bentolila and Dolado, 1994; García-Pérez and Muñoz-Bullón, 2011; Aguirregabiria and Alonso-Borrego, 2014).

As a response, a 1994 reform restricted temporary contracts to seasonal activities and eased dismissal conditions for permanent employees. However, in practice, employers continued to hire temporary workers for non-seasonal roles (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 introduced a new type of permanent contract with reduced severance pay – down to 33 days per year of seniority in some cases – and provided fiscal incentives to encourage the conversion of temporary to open-ended contracts for certain demographic groups.[3] The 2001 reform extended these subsidies to additional groups (García-Pérez and Muñoz-Bullón, 2011; García-Pérez et al., 2019), with similar incentives introduced in further reforms in 2006 and 2010.

It was not until 2012 that severance payments for permanent employees were significantly reduced across all types of open-ended contracts. At the same time, compensation at the termination of temporary contracts was increased, narrowing the gap between the dismissal costs for permanent and temporary workers.[4] The reform also eliminated in-

[2]During the early years of the transition (1975–1982), policymakers sought to prioritize stable employment. Over time, however, growing labor market pressures made it increasingly difficult to sustain a system based exclusively on permanent hiring, which led to a rise in temporary employment and, in practice, the spread of illegal temporary contracts (Galacho, 2006).

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

[4]The 2012 reform increased the compensation for terminating a temporary contract from 8 to 12 days

terim wages during judicial processes and introduced a new type of open-ended contract for firms with fewer than 50 employees, entailing no severance pay during an extended one-year probationary period. After this period, workers were entitled to the same severance payments as 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 protection gap between ordinary permanent and temporary contracts (Dolado, 2017). Despite these reforms, the share of fixed-term contracts remained above 20%. Additionally, the decline in temporary employment during this period was influenced by the loss of temporary jobs caused by the Great Recession.

Concerns about the lack of job stability for workers on temporary contracts, and their potential adverse consequences, have motivated numerous labor market reforms over the past three decades. Before 2021, these reforms targeted severance costs, contract duration, and penalties for contract rollovers. However, they did not substantially alter the duality created by the high flexibility of temporary contracts. This persistent segmentation provides the context for the analysis in this paper. By contrast, the most recent reform of December 2021 appears to have significantly reduced the use of fixed-term contracts (Conde-Ruiz et al., 2023). Fully effective since March 2022, the reform abolished project-based contracts and replaced them with a single temporary contract for structural needs, the *circumstances of production* contract. With a maximum duration of six months, extendable to one year by collective bargaining agreements, related to peaks such as seasonal demand (e.g., Christmas or agricultural harvests).[5] The reform further promoted the use of *intermittent-permanent* contracts, a highly flexible modality that has expanded considerably since 2021. Whether these changes will ultimately reduce labor market segmentation remains an open question.[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

of wages per year worked, while reducing the compensation for *unfair dismissals* of permanent contracts 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]The reform also allowed the use of this contract to cover employees on temporary leave. In addition, workers are now entitled to *permanent* status if they remain continuously employed for more than 18 months within a 24-month window, even across different positions within the same firm or group of firms

[6]Conde-Ruiz et al. (2023) distinguishes between the *contractual* and the *empirical* temporary employment rates in Spain, arguing that the 2021 reform primarily affected the former.

records. The sample allows tracking the full working history of individuals back to 1967 and the monthly earnings since 1980. The unit of observation in the MCVL is any change in the individual's labor market or contract status, including changes in occupation or contract conditions within the same firm. 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 dates, we can compute the exact days an employee was employed (including paid vacations). Whenever there is an overlap of spells, we preserve the job characteristics of the main job, i.e., within each year and month, the job with the highest income. 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, rather than those acquired by the worker. Still, they are closely related to the required formal education and skills to execute a particular job.

At the establishment level, we observe the province where the firm is located and its employment size since 2006. 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 employed in the private or public sector.

The MCVL provides earnings information from two sources: Social Security and tax records. Because the Social Security contribution base is both top- and bottom-coded,[8]

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

[8]The upper and lower bounds vary by contribution group and are updated annually.

we rely on monthly real earnings from tax records whenever available,[9] which are not censored. Combining multiple survey waves allows us to reconstruct tax records over time, as they do not include retrospective histories before 2006. For earlier years, we use Social Security information instead. Since the Autonomous Communities of Navarre and the Basque Country collect income taxes independently, only Social Security records are available for workers in those regions. Finally, because we observe the exact duration of each employment spell, we can compute daily wages, which we use when explicitly noted.

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 only from 1998 onwards. To mitigate the potential impact of the COVID-19 pandemic on job creation, we limit observations up to February 2020. We focus on native workers aged 18 to 49 to mitigate the potential impact of early retirement on labor market outcomes and to address the limitations of incomplete 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 self-employed workers, as they do not hold open-ended contracts and therefore 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, as the sample of workers is very small in these areas. Thus, we work with data from 50 provinces.

4 Descriptive Evidence

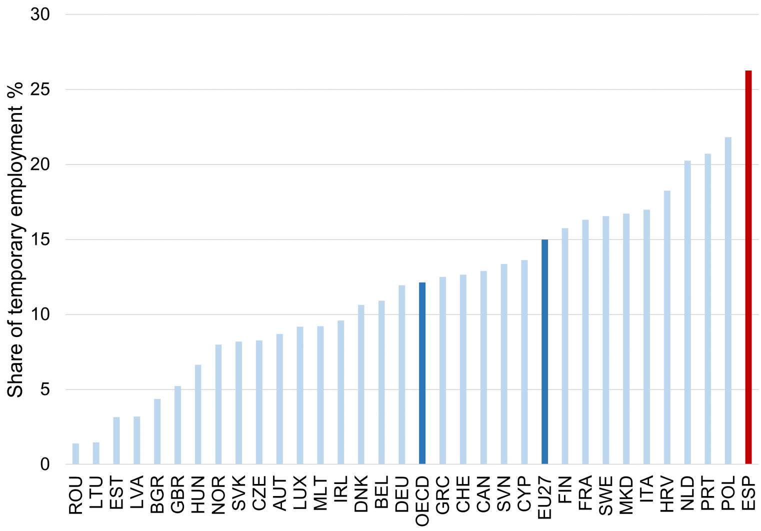
Over the past decade, roughly one-third of Spanish workers employed in any given year held a fixed-term contract. Although the incidence of temporary employment declined after the Great Recession (see Appendix Figure C.1.1), Spain continued to record the

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

[10]Public sector workers are typically required to pass specific exams and meet special requirements to secure a permanent position. This process differs significantly from the promotion path of private sector workers.

highest share of temporary workers among European and OECD countries until very recently (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 noted, the dualism in the Spanish labor market suggests that many fixed-term contracts, rather than serving as stepping-stones, lead to "dead-ends" (Bentolila et al., 2020). Although this problem is more pronounced in low-skilled occupations, it may also be 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. Regarding 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, accounting for nearly one-third of temporary contracts in 2016.

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

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

where exp_{it}^{FTC}, exp_{it}^{OEC} and exp_{it} denote the experience that worker i accumulated until period t in fixed-term, open-ended or any contract type, respectively, X_{it} is a vector of

[11] The 2022 labor reform substantially reduced temporary employment rates in Spain (Appendix Figure C.1.3). However, its effects on job duration and short-term transitions appear to have been more limited (Conde-Ruiz et al., 2023).

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.

time-varying individual and job characteristics, σ_r and ψ_t are province and year-month fixed-effects, and ε_{ict} is the error term (see Appendix B for details).

In the early years, experience accumulated under either open-ended or fixed-term contracts yields comparable wage returns. Over time, however, wage growth is slower for workers who remain in fixed-term contracts (Appendix Table B.1.1). For instance, after ten years of experience, an additional year on a fixed-term contract raises earnings by about 3.0%. In contrast, an additional year on an open-ended contract increases earnings by 4.5%. This specification accounts for differences in the value of accumulated experience across contract types but does not address the potential sorting of workers into each type. Prior studies have addressed this concern by including worker fixed effects. While this approach narrows the estimated gap between fixed-term and open-ended returns, the overall pattern remains intact (Appendix Figure B.1.1).

The finding of lower wage growth under fixed-term contracts in Mincer-type regressions is consistent with Garcia-Louzao et al. (2023), who show that this gap cannot be explained by unobserved firm heterogeneity or match quality.[12] We next demonstrate, however, that our descriptive estimates from Mincerian specifications with individual fixed effects should not be interpreted causally. Instead, they reflect selection: more able workers are both (i) more likely to obtain open-ended contracts and (ii) experience faster wage growth regardless of contract type, a form of selection that is not addressed by the fixed-effects

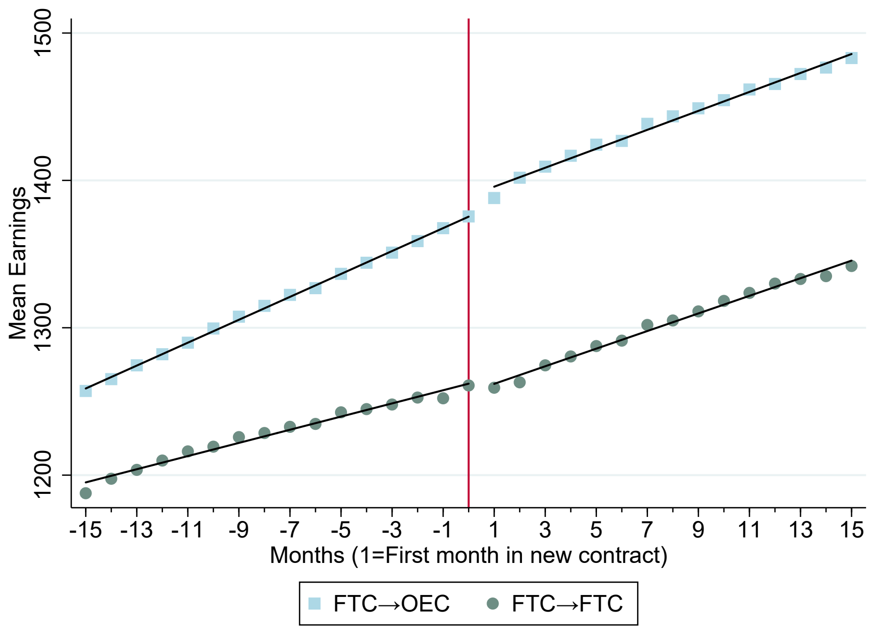
[12] Garcia-Louzao et al. (2023) also implement alternative strategies to address selection, using instruments based on regional variation in subsidies for hiring workers under open-ended contracts. In this paper, we exploit a different source of variation, drawing on precise high-frequency data from Spanish administrative records.

approach.

5 Selection into permanent positions

Mincer-type regressions appear consistent with a key concern about temporary contracts: in countries with high dualism, such contracts might offer fewer on-the-job-training opportunities and therefore result in less skill accumulation (Cabrales et al., 2017) and slower wage growth. However, including worker-fixed effects in such models only captures part of the endogeneity problem arising from contract sorting.

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 for 15 months before and after they switch to a new contract. For months in which a worker is not employed for the entire duration, we extrapolate monthly earnings based on their computed daily earnings.

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. Implementing a similar "event study" design 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 presents 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] We observe that workers who eventually transition to a permanent position are on a different earnings trajectory even *before* the transition occurs – while they are still on a fixed-term contract. Moreover, they do not experience higher earnings growth after switching to a permanent contract, neither compared to their previous trajectory nor to workers who remain in temporary contracts. These patterns suggest a very different interpretation of the Mincer regression results. Rather than permanent contracts offering higher returns to experience, it appears that workers with higher returns are more likely to secure such contracts. They also suggest that selection into *contract type* is a harder problem than selection into *firms* or *regions*, which typically "passes" the event study test.[14]

We adopt a more formal event-study design that controls for age effects and other potential confounders to further study these differences in earning trends. 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.[15] We categorize workers based on their future type of contract, distinguishing workers transitioning from a fixed-term to an open-ended contract (FTC→OEC, $C_i = 1$) and workers transitioning to another fixed-term contract (FTC→FTC, $C_i = 0$). Our baseline specification considers a balanced panel of workers for whom we observe fifteen periods (months) before and after the event.[16] 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:

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

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 a fixed effect 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

[13] Figure C.2.1 in the Appendix provides the corresponding evidence for median earnings and further distinguishes between transitions to open-ended contracts (OEC) that occur within the same firm and those associated with a move to a different firm.

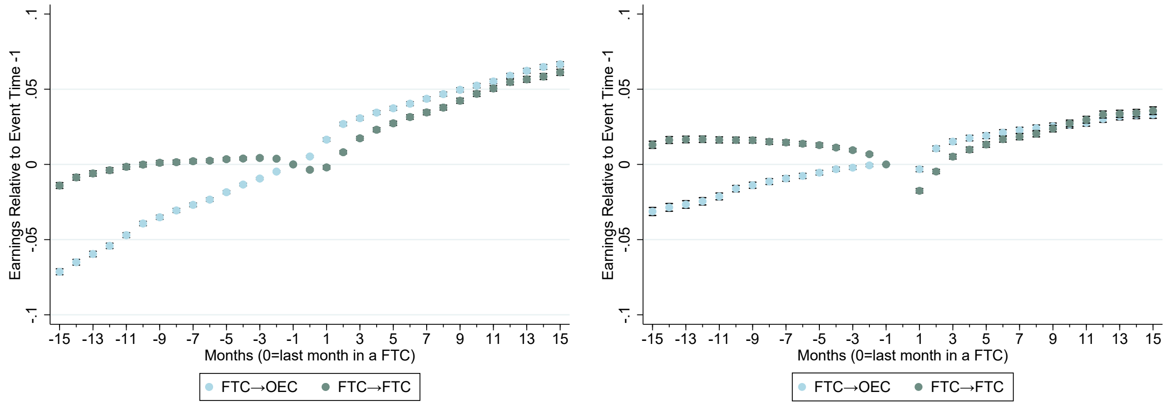
[14] For example, Card et al. (2023) conducts a similar exercise to study the causal effect of places on earnings, showing that workers' trajectories only differ *after* changing locations.

[15] A worker may experience multiple contract transitions throughout their career. Consequently, those transitioning between fixed-term or open-ended contracts can appear multiple times in the sample. On average, each worker is associated with 2.4 events. Figure C.2.3 in the appendix presents an equivalent exercise that restricts the sample to each worker's first event.

[16] We here 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.

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: Evolution of earnings before and after switching to a new contract (Event study)



(a) Baseline specification

(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 (FTC). These coefficients are derived from a balanced sample of workers observed between January 1998 and February 2020. The analysis tracks workers' log earnings for 15 months before and after the contract change. For months in which a worker is not employed for the entire duration, we extrapolate monthly earnings based on their computed daily earnings. 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.

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 might expect that workers face differential earnings in event period 1, as temporary contracts may be subject either to earnings penalties or premia (Albanese and Gallo 2020; Kahn 2016). Indeed, 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 compared to those who switch to another fixed-term contract. As shown in Appendix Figure C.2.2, this premia is much more pronounced for workers who switch to an OEC contract in a new firm rather than those who upgrade within the firm.

Figure 3 confirms that earnings evolve differently *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 growth (conditional on time and age effects) observed for workers who remain on fixed-term contracts. In contrast, earnings evolve similarly after workers enter permanent 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.

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 exogenous in a dynamic sense. One factor that might explain why selection is more problematic in our setting is that upgrades to open-ended contracts often occur within firms; therefore, they are based on more information than the matching of workers to new firms. Consistent with this hypothesis, we show in Appendix Figure C.2.2 that workers who secure an open-ended contract within the same firm experience markedly stronger earnings growth before the transition compared to those who remain on fixed-term contracts or those who obtain open-ended positions at other firms. Another argument is that the outcomes in this context are more clearly ordered. Given the choice, it is reasonable to assume that most workers would prefer an open-ended contract over a fixed-term one. In contrast, this preference is less evident in worker-firm or regional matching, where workers may have idiosyncratic preferences for specific regions (e.g., because their friends live there) or specific firms (Card et al., 2023).

6 Identification

We propose an instrumental variable strategy to deal with the endogeneity of contract upgrades into permanent positions. 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 thus exploit that workers face greater chances of finding a permanent position if there is a spike in permanent openings in the labor market just when their contract expires.[17]

[17] As noted by Blanchard and Katz (1992), transitory fluctuations in employment tend to reflect labor demand rather than supply shifts. A spike in permanent openings affects contract upgrade probabilities in direct and indirect ways. Directly, as workers have greater chances of securing permanent positions within or outside their current firm, more permanent openings become available. Indirectly, as 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 an individual's fixed-term contract is set to expire with the number of new contracts at the regional level starting the next month. The number of new contracts is a useful measure of the labor demand that an individual faces, even if their job search began in the preceding months.[18] Moreover, we argue that facing more job openings precisely when a contract is about to end is as good as random for the worker, conditional on time (year) and seasonal (month) fixed effects. We provide evidence to support this claim below.

Simplified example.— Consider two workers on identical 12-month fixed-term contracts in the retail sector of the province of Almería, whose contracts end one month apart. Worker A's contract ends in July 2007, while Worker B's ends in August 2007. In August 2001, Almería experiences a sharp, province-wide rise in the number of new open-ended contracts (OECs) (one-standard-deviation rise)—for instance, because large chains finalize their seasonal staffing plans and convert more positions to permanent status. Given this timing, Worker B faces a labor market with more newly created OEC slots than Worker A and is therefore more likely to be upgraded to a permanent contract by her (or another) firm.

A cross-sectional variant of this logic compares workers whose fixed-term contracts end in August 2007 but in different provinces. Suppose Almería experiences a surge in new OECs that month, while neighboring Murcia does not. A worker finishing in August in Almería is then more likely to transition to an OEC than an otherwise similar worker in Murcia, simply because more permanent positions are being created locally. Figure C.5.1 in the Appendix illustrates this source of variation for Almería and Murcia, highlighting the high-frequency fluctuations in open-ended contract creation that we exploit.

Our approach is conceptually related to previous work on the effect of labor market conditions on worker careers, such as research on the effects of entering the labor market during a recession ("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 recent research on compositional changes in labor demand (Arellano-Bover, 2024). However, while these studies consider yearly fluctuations in labor market conditions affecting entire cohorts, we exploit high-frequency administrative data to isolate individual-level shocks in labor demand while *controlling* for overall business cycle conditions.

Specifically, we estimate the following first-stage equation for the period 1998 to Febru-

[18] Workers may search for new positions *before* their current contract expires. Similarly, firms may post job openings some time before filling them. Regardless of the timing of this job search process, most workers will only start a new contract after their current one ends. Our instrument will, therefore, be relevant despite the (unobserved) dynamics of the underlying job search process, as we confirm below.

ary 2020:

$$p_{it+1} = \sum_{k=-24}^{24} \alpha_k OEC_{-i,r,t+k} + \sum_{k=-24}^{24} \gamma_k FTC_{-i,r,t+k} + \mathbf{X}'_{it}\theta + \mu_r + \psi_t + \phi_s + \epsilon_{it}, \quad (3)$$

where t refers to the exact month the worker's fixed-term position ends. Thus, p_{it+1} indicates whether the worker starts an open-ended contract in $t+1$, the month after their fixed-term contract ends.[19] The variable $OEC_{-i,r,t+k}$ is constructed as the log sum of all new open-ended positions in period $t+k$ in worker i's initial province of residence r, excluding worker i herself (leave-one-out). Therefore, we allow contract upgrades to depend on the leads and lags of the log number of new open-ended contracts, always excluding individual i. Figure C.4.2 in the appendix supports the linear specification of equation (3) by plotting the underlying conditional expectation following the methodology by Cattaneo et al. (2024).

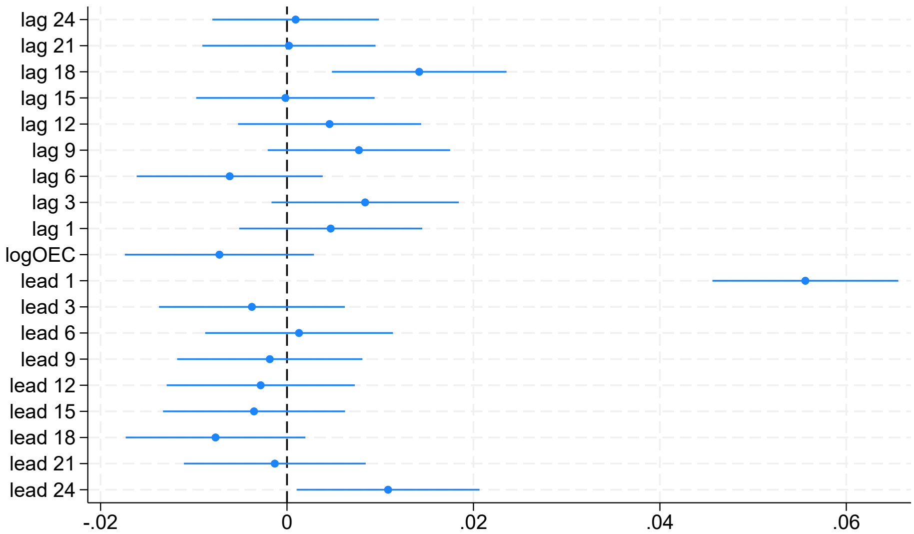
As motivated above, the first lead, $OEC_{-i,r,t+1}$, is our instrumental variable. At the individual level, X_{it} accounts for gender, overall experience, and experience squared at baseline, as well as interactions of age categories with educational attainment. As the vector X_{it} includes year, month, province-at-baseline (i.e. the last month employed in a fixed-term contract), and sector-at-baseline fixed effects, this instrument captures regional fluctuations in the supply of new open-ended contracts that are transitory and, as we argue, as good as random from the perspective of the worker ("instrument independence"). We provide evidence supporting this assumption below. To further partial-out business cycle and seasonal variations in job openings, we also include the corresponding set of leads and lags for the log number of new fixed-term contracts, $FTC_{-i,r,t+k}$. Appendix Table D.1.1 provides descriptive statistics for our estimation sample and instrument.

Under our identification assumptions, we expect the effect of this first lead, captured by the coefficient α_1, to be the strongest predictor of an individual's likelihood of switching to a permanent position. The coefficients on other leads and lags (α_k for $k \neq 1$) should be smaller in magnitude, though they might be non-zero, as they capture general business cycle conditions that could affect contract upgrade probabilities. Including a full set of leads and lags in the number of new permanent contracts serves, therefore, two purposes. First, to illustrate that transitory fluctuations matter if they occur precisely when a worker's previous contract expires, i.e., to show that the first lead has strong predictive power even conditional on the other leads and lags (instrument relevance). Second, these other leads and lags control for general business cycle conditions, which could violate the instrument exclusion restriction.

[19] $p_{i,t+1}$ takes the value of 1 if the worker is employed in an open-ended contract in the calendar month after the end of the fixed-term contract and 0 if the worker is either unemployed or employed in another fixed-term contract.

6.1 Instrument Relevance

Figure 4: Effect of new open-ended contracts in the region on permanent contract status



Notes: Estimates of equation (3), considering 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 province on the probability of switching to an open-ended contract in $t + 1$. The sample mean of the switching to an open-ended contract dummy equals 0.377. 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 in the province.

The estimates from equation (3) 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 number of new open-ended contracts in the month when the worker's previous contract ends is the strongest predictor of their likelihood of securing a permanent position immediately afterward. The instrument does a good job at predicting the probability of transitioning into an open-ended contract, with an excluded instrument, the F-statistic in the first stage is 130.48, suggesting that weak-instrument bias is not a concern in our setting. Moreover, the lack of strong correlations with the other leads and lags indicates that the instrument captures the effect of transitory shocks on job market matches unrelated to general business cycle trends. The observation that the coefficient is positive further confirms that transitory fluctuations in employment reflect shifts in labor demand rather than labor supply (consistent with citepblanchard1992regional.

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 level or by the workers' baseline industry instead. As shown in Appendix Figure C.5.2, we find similar patterns in these alternative specifications. The instrument is, therefore, relevant regardless of whether we measure it at the national, regional (baseline), or industry level. The instrument also remains relevant when excluding months with potentially high job-seasonality from the data (see Appendix C.6). Moreover, it likely satisfies the instrument monotonicity condition, as the opening of more permanent positions is unlikely to reduce any worker's chance of a contract upgrade.

We argue that the instrument 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 workers, 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.

6.2 Instrument Exogeneity

Instrument Independence. We argue that the number of new permanent positions when a worker's contract is about to expire is, from the worker's perspective, as good as random and, therefore, exogenous. Although it cannot be directly tested, we support the validity of the independence assumption by evaluating whether the instrument correlates with observable individual or firm characteristics. As shown in Appendix B.2, conditional on year, month, and province fixed effects, the instrument is broadly uncorrelated with worker characteristics (age, experience, and educational attainment), sector, or firm characteristics (firm age and size). The absence of such correlations with observable characteristics suggests that the instrument is less likely to correlate with unobserved worker characteristics. Moreover, our research design accounts for time-constant unobserved heterogeneity, and we systematically test for pre-trends that could result from time-varying unobserved heterogeneity.

An additional concern is that our instrument may be correlated with the quality of OECs. Particularly, certain firms might offer many OECs in a given month, but those might differ in quality from those in other months. To address this, we provide additional evidence showing that the instrument is not correlated with key job characteristics, such as the average tenure of new OECs, the average required skill level, and the average starting earnings. Table B.2.1 in the appendix presents the results on the composition of OECs using our instrumental variable. The findings show no significant relationship across any examined outcomes, suggesting that our instrument affects only the quantity of OECs, not their composition.

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 a 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 could influence the termination date for some workers. For example, firms might repeatedly renew fixed-term contracts while waiting for economic conditions to improve, potentially creating a feedback effect from labor demand (i.e., the number of new permanent contract openings) on labor supply (i.e., the number of workers with recently expired temporary contracts). However, such feedback effects would not necessarily be problematic. They could weaken the first stage by diluting the effects of demand shocks between more workers, but we already established that the first stage is strong (see Figure 4). Instead, feedback effects between supply and demand would pose a problem only if they were *selective*, affecting some types of workers more than others. For example, if more talented workers were more likely to terminate their employment contract prematurely to move to a permanent position, this could create a correlation between worker ability and our instrument, which would then contaminate our estimates of the impact of contract status on worker careers.

Four aspects of our empirical strategy mitigate these concerns. First, at the time of our analysis, legal limitations on the consecutive renewal of temporary contracts restricted such arrangements to a maximum of two years.[20] Second, we apply specific sample restrictions to focus on workers who are more likely to have terminated their contract at the initially intended date. As a large proportion of contracts are stipulated to last one year, we restrict our sample to workers whose contracts effectively ended after 0.8-1.2 years.[21] This choice also excludes extremely brief work contracts, common in our context (Bentolila et al. 2020). Third, we find no meaningful correlations between our instrument and observable worker characteristics, such as education (see Appendix B.2). Fourth, if selection were present, it would likely be reflected in the pre-treatment trend in wages (as in Figure 2), yet our instrumental variable estimates show no such pre-trends (as shown below).

[20]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.

[21]Appendix Figure C.1.2 confirms that many contracts effectively ended after one year. Additionally, the results in Section C.11 of the Appendix present findings using different tenure restrictions. Specifically, by focusing on contracts nearing the legal maximum duration, the results remain consistent and very similar.

7 Results

We exploit regional fluctuations in the availability of new contracts to generate exogenous variation in workers' likelihood of transitioning from fixed-term to permanent positions. Using this variation, we first present reduced-form estimates of how contract-upgrade opportunities affect labor market outcomes in both the short and long run. Interpreting these results under the exclusion restriction, we then implement a 2SLS strategy to estimate the causal effect of contract type on workers' career trajectories.

Focusing on workers observed in the final month of a fixed-term contract between 1998 and 2017 (N=219,704)[22] We track their outcomes for up to 60 months before and after the contract endpoint. To estimate the causal effect of obtaining a permanent contract, we exploit transitory fluctuations in the availability of open-ended contracts at the time a worker's fixed-term contract expires and use this variation as an instrument for permanent contract status in the following IV model:

$$y_{it+h} = \beta_1 p_{i,t+1} + \sum_{k\neq 1, k=-24}^{24} \beta_k logOEC_{-i,r,t+k} + \sum_{k=-24}^{24} \gamma_k logFTC_{-i,r,t+k} + X'_{it}\theta + \epsilon_{it}, \quad (4)$$

where $p_{i,t+1}$ is a treatment indicator that equals one if a worker i transitions to an open-ended contract in $t+1$, after their fixed-term position expires. Workers who do not switch into a permanent position at $t+1$ may either start a new fixed-term contract or exit into non-employment. The dependent variable y_{it+h} denotes the worker's i outcome in period $t+h$, with $h = 1, \ldots, 60$. This setup allows us to 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.

We include the same control variables as in the first stage (see equation 3), including 24 leads and lags of the log number of new open-ended contracts ($\log OEC_{-i,r,t+k}$) and new fixed-term contracts ($\log FTC_{-i,r,t+k}$), as well as year, month, province, and sector fixed effects. X_{it} represents a vector of individual controls measured at baseline, including gender, experience, experience squared, and interactions between age categories and educational attainment. Our instrument is the first lead of the leave-one-out log of new open-ended contracts in the worker's baseline province r, $\log OEC_{-i,r,t+1}$. Appendix Table D.1.1 reports descriptive statistics for the estimation sample.

Our baseline specification already includes extensive controls for business-cycle variation at both the national and provincial levels. We further tighten this control by in-

[22]Table D.1.1 in the Appendix provides descriptive statistics for the estimation sample. We impose no restrictions on the number of events per worker, so some individuals appear multiple times. The sample comprises 160,705 unique workers, with an average of 1.37 observations per person.

cluding the aggregate leave-one-out average of the outcome variable, $\overline{Y}_{-i,r,t+h}$, where we construct $\overline{Y}_{-i,r,t+h}$ using the full sample of workers aged 18 to 49 years old, regardless of the timing of their contract expiration date.[23] As such, there is no strong 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.[24] This additional control helps further account for economic conditions, ensuring that our instrument only captures transitory variation in the availability of open-ended positions that are uncorrelated with broader business-cycle trends. Results from this specification are presented in Appendix Figures C.8.1 and C.8.2.

We begin our analysis by examining the effects of our instrument on earnings and employment outcomes. For earnings, we track both current and cumulative earnings relative to those received in the last month of the expiring fixed-term contract. For employment, we study employment status, the probability of holding an open-ended contract, cumulative labor market experience, and cumulative experience in open-ended positions. We also explore mobility responses, including transitions across sectors or regions and the number of firm switches following contract expiration.

We first report reduced-form estimates from Equation (4), using the first lead of the leave-one-out log of new OEC as the instrument. This specification facilitates interpretation under weaker assumptions. The corresponding coefficients are plotted in Figure 5. We then turn to our IV estimates, reporting both short- and long-term effects of transitioning to an open-ended contract. Appendix C.8 shows that these results are robust to alternative control sets and the specification in Equation (4). To mitigate the influence of outliers, we trim observations whose earnings are above the 99th percentile in each period, as these likely reflect atypical wage realizations that could distort mean estimates.

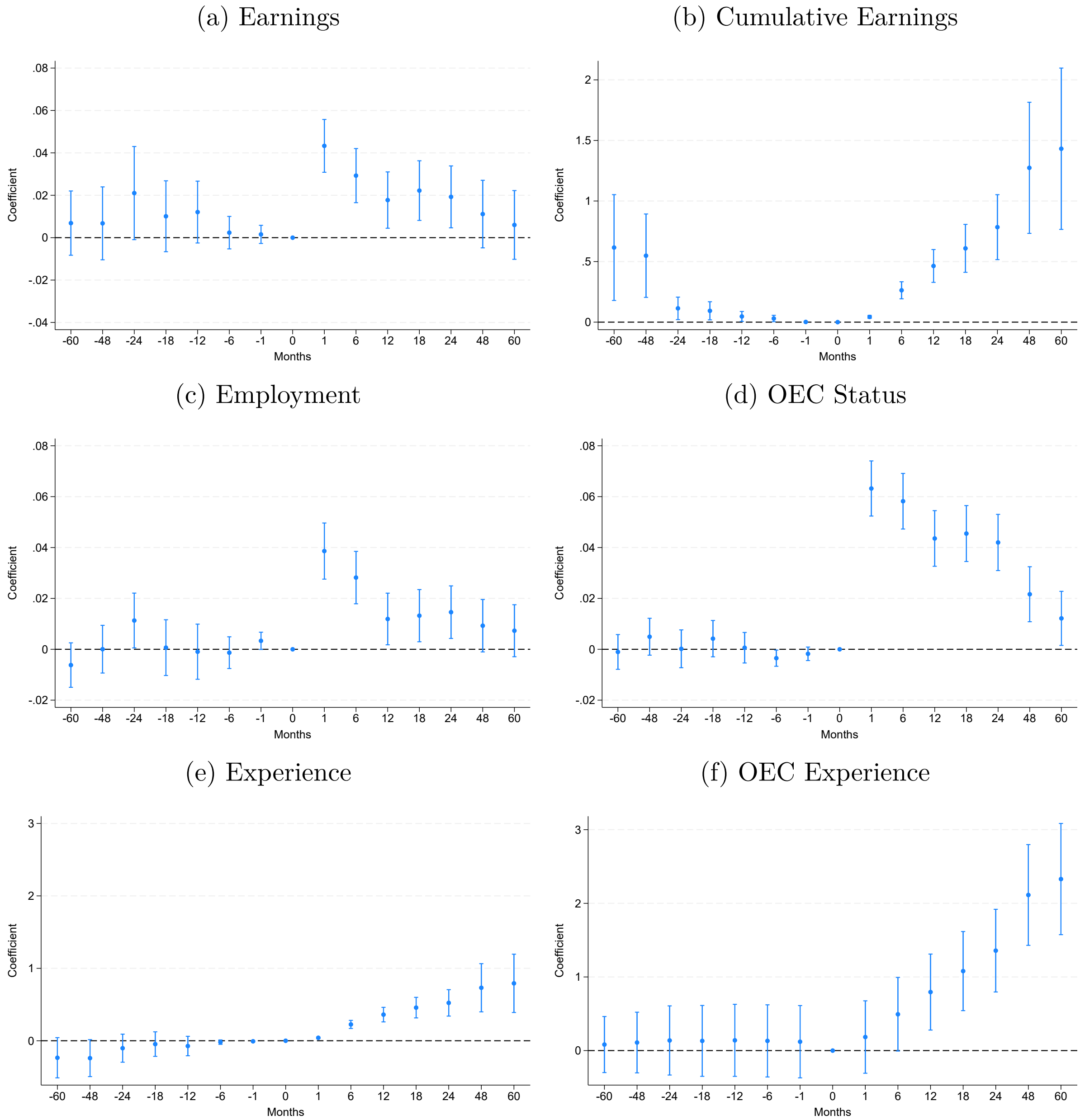
7.1 Earnings

Panel (a) of Figure 5 presents the long-term effects of contract upgrade opportunities on workers' earnings. We estimate the reduced form of equation (4) separately for each event period h and then plot the corresponding coefficients of our instrument $logOEC_{-i,r,t+1}$. Earnings are measured as the ratio between monthly earnings at $t+h$ and their earnings

[23]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 $t+h$. In contrast, $\overline{Y}_{-i,r,t+h}$ 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).

[24]Appendix Table D.1.2 presents descriptive statistics for the "all workers" sample. For comparison, this sample comprises an average of 311,150 monthly workers, whereas the estimating sample includes only 1,200 monthly workers (cf. Table D.1.1).

Figure 5: Effect of New Open-Ended Contracts on Worker Outcomes (Reduced Form)



Notes: The sample consists of workers in the last month of a fixed-term position in the 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 $logOEC$ and 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.

in the baseline month t,[25] 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 ending the fixed-term position. Similarly, Panel (b) explores the effect on workers' cumulative earnings, computed as the sum of monthly earnings from period t to

[25]We measure earnings at a monthly frequency. For workers not employed for the entire month, we impute monthly earnings by extrapolating observed daily earnings to the total number of days in that month.

$t + h$, also normalized by the monthly earnings in period t.

Panel (a) shows a sharp increase in earnings in the event period $h = 1$, i.e., one month after exposure to improved open-ended opportunities. A one-standard-deviation increase in the log number of permanent contracts raises exposed workers' earnings by 6.44% (1.348×0.0477) of their initial monthly earnings. The effect gradually fades, though the earnings impact remains positive for up to two years after exposure. Five years later, the point estimate is close to zero and no longer statistically significant.

Part of this decline reflects that some workers who were not *lucky* at $h = 1$ and remained in fixed-term contracts eventually secure a permanent position. These later upgrades are accompanied by earnings gains, narrowing the gap between early and late upgraders. However, as discussed below, this catch-up process explains only part of the overall decline. In the following sections, we examine additional mechanisms contributing to the erosion of the earnings premium.

Although the positive effect on *current* earnings diminishes over time, these temporary gains generate a lasting difference in cumulative earnings. As shown in Panel (b) of Figure 5, a one-standard-deviation increase in the number of permanent contracts at event time $h = 1$ raises cumulative earnings significantly after $h = 12$ months.

Table 2 reports our IV estimates for earnings, employment, and mobility in the short and long run.[26] Panel A shows short-term effects (12 months after the fixed-term contract ends), while Panel B examines long-term outcomes (five years after expiration). In the short term, upgrading to a permanent contract leads to sizable gains in both monthly and cumulative earnings, consistent with the reduced-form evidence but now with a more precise causal interpretation. The IV coefficient of 0.203 implies that obtaining a permanent contract raises monthly earnings by 20.3% relative to baseline levels after one year (Column 1). Put differently, a one–standard deviation increase in the predicted probability of upgrading translates into a 9.9% earnings gain (0.488×0.203). Over the same horizon, cumulative earnings rise by the equivalent of nearly 3.7 months of baseline pay (Column 2).

However, the earnings effects dissipate over time; five years after the fixed-term contract ends, the point estimates are close to zero and statistically insignificant. Nonetheless, workers in permanent contracts retain a substantial cumulative advantage, equivalent to 1.3 years of baseline earnings over the five-year horizon, driven mainly by the initial boost in earnings.

[26] Appendix Table D.2.1 presents the corresponding OLS estimates for Equation 4.

Table 2: Effect of Permanent Contracts on Worker Careers

	Panel A: Short-term effects (12 months)					
	Earnings (1)	Cum. Earnings (2)	Employment (3)	Experience (4)	Change Region (5)	Change Sector (6)
$p_{i,t+1}$	0.305***	7.972***	0.205**	6.164***	-0.051	-0.287***
	(0.114)	(1.113)	(0.086)	(0.770)	(0.064)	(0.086)
Obs.	197,299	197,299	197,299	197,005	197,299	197,299
R2	0.101	0.192	0.128	0.997	0.056	0.168
Mean Dep.	-0.184	8.538	0.743	85.016	0.091	0.289
	Panel B: Long-term effects (60 months)					
	Earnings (1)	Cum. Earnings (2)	Employment (3)	Experience (4)	Change Region (5)	Change Sector (6)
$p_{i,t+1}$	0.103	24.607***	0.125	13.266***	-0.006	-0.287***
	(0.142)	(5.675)	(0.089)	(3.249)	(0.081)	(0.096)
Obs.	197,299	197,299	197,299	190,846	197,299	197,299
R2	0.227	0.186	0.258	0.944	0.045	0.136
Mean Dep	-0.249	42.108	0.590	119.415	0.169	0.491

Notes: The table reports IV estimated coefficients based on equation (4). 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. The F-statistic excluded instrument is equal to 106.01. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

7.2 Employment

Panels (c) to (f) in Figure 5 show the effects of contract upgrade opportunities on employment trajectories. Panel (c) examines their effect on the likelihood of being employed at period $t + h$. Similarly, Panel (d) analyzes the probability that workers are employed under an open-ended contract at period $t + h$. Finally, Panels (e) and (f) focus on cumulative experience, with Panel (e) examining total experience, measured by the total number of days worked since the termination of the fixed-term contract, and Panel (f) considering experience in open-ended contracts (both expressed in months). Appendix Figure C.9.3 presents the corresponding results for part-time employment, showing no statistically significant effects either before or after the contract ends.

Panel (c) shows that a temporary increase in the number of open-ended contracts raises short-run employment probabilities. In the event period $h = 1$, a one–standard deviation increase in the log of new permanent contracts increases the probability of being employed by 5.4 percentage points. This effect size is similar to the corresponding effect on earnings (Panel (a)). Panel (d) reveals an even larger effect on the likelihood of holding a permanent contract, with an increase of 8.5 percentage points, equivalent to 21% of the mean (0.4063).

As for wages, these employment effects diminish over time. After $h = 60$ months, the impact on employment has nearly vanished, while the probability of holding a permanent contract is only slightly elevated. A "lucky draw" in upgrade opportunities provides thus only a temporary boost, with no long-term consequences on employment and minimal influence on contract status. However, while the employment effects are temporary, they have a lasting impact on work experience. As shown in Panels (e) and (f), workers exposed to favorable contract-upgrade opportunities accumulate more work experience, particularly in open-ended contracts.

Table 2 shows that the large earnings effects reflect that the instrument not only shifts workers into permanent contracts but also raises their probability of immediate reemployment. After 12 months, their employment rate is 17 pp. higher (Column 3), about 23% of the mean, and they have accumulated six additional months of work experience relative to workers who did not secure a permanent contract at $h = 1$ (Column 4).

The positive impact on employment is more persistent than that on earnings growth. Workers who upgraded at $h = 1$ are still 8.9 pp. more likely to be employed after five years, although this difference is not statistically significant. By that horizon, they have accumulated 11 additional months of total work experience. Appendix Table **??** shows that the long-term advantage is even larger for open-ended–contract-specific experience, amounting to nearly 31 extra months over five years.

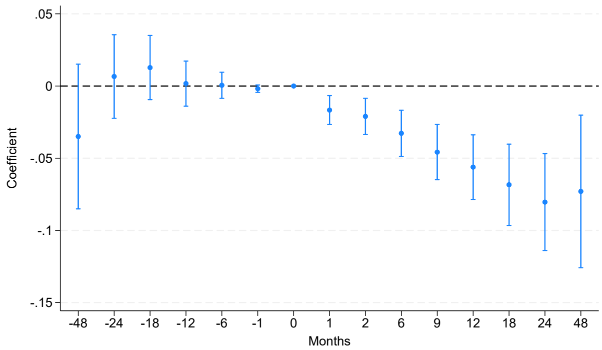
As already noted, the overall pattern in employment – with sharp initial gains that then fade over time – resembles the corresponding effects on earnings. However, the employment effect diminishes more gradually than the effect on earnings: while the earnings effects are close to zero after four years, the employment effects remain positive. Moreover, the zero long-run impact on earnings is difficult to square with the permanent gains in work experience documented in Column (4). With positive returns to experience, these permanent gains in experience should result in long-term earnings advantages. Next, we study whether the impact of upgrade opportunities on worker mobility might explain the null long-run effect on earnings.

7.3 Sectoral and Regional Mobility

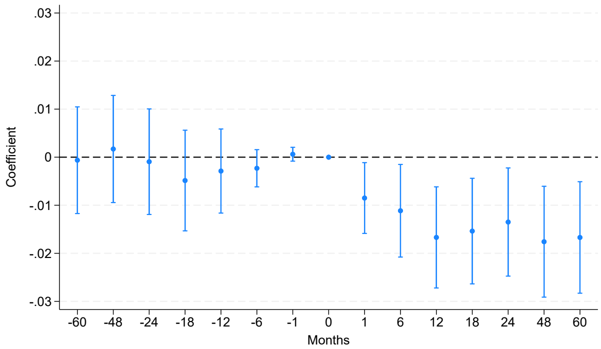
A key advantage of permanent over temporary contracts is greater job security, which is also reflected in workers' mobility decisions. Figure 6 examines this margin by assessing how the opportunity to upgrade to a permanent contract affects the number of firm switches, the likelihood of moving to a different sector, and to a different region. We first present reduced-form estimates, followed by the IV results in Columns (5) and (6) of Table 2. The number of firm switches is measured before ($h < 0$) and after ($h > 0$) the expiration of the fixed-term contract ($h = 0$). For sectoral and regional mobility, we use

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

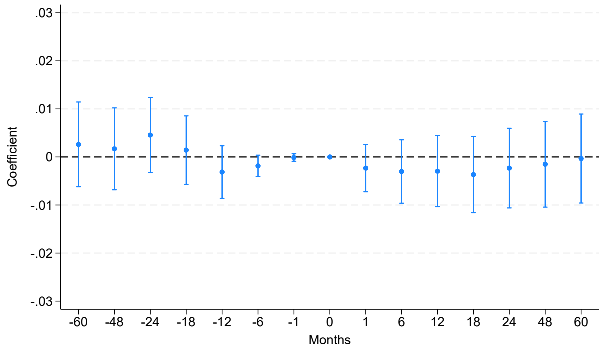
(a) Number of firm moves



(b) Probability to change sector



(c) Probability to change province



Notes: Baseline sample restrictions and empirical specification are described in Figure 5 notes. In Panel (a), the outcome is the number of firm switches between period t and $t+h$. In Panels (b) and (c), the outcome is an indicator that equals one if a worker has ever changed sector or province between period t and $t+h$. Missings are coded as zero.

an indicator equal to one if the worker changes sector or province at any point between t and $t+h$.

Panel (a) of Figure 6 shows that a temporary increase in the number of open-ended contracts leads to a sizable reduction in firm mobility. Not surprisingly, workers on permanent contracts are more likely to stay with their current firm than those in temporary positions, whose contracts cannot be extended indefinitely (see Section 2). Greater stability in employment relationships could positively impact earnings growth, as it may encourage firms to provide training or other investments to their workers (Cabrales et al., 2017; Albert et al., 2005). However, firm mobility could also positively affect earnings growth if workers climb the "firm ladder" (Burdett and Mortensen, 1998) or search for positions that better align with their skills or interests.

An increase in the number of permanent contracts also reduces the probability that workers switch to a different sector (Panel (b)) or relocate to another province (Panel (c)). Specifically, a one S.D. increase in the log number of new open-ended contracts is associated with a 1.11 p.p. reduction in the probability of sectoral change, representing 9.4% of the unconditional mean. The estimates for sectoral mobility remain highly significant over the longer term, whereas those for regional mobility are less pronounced. These results suggest that the security derived from permanent employment has important implications for workers' choices, potentially deterring them from pursuing alternative career paths. These findings provide a potential explanation for the null long-term response in earnings we documented earlier. While job stability is generally desirable for most workers, it may come at the cost of career flexibility and progression.

Columns (5) and (6) of Table 2 show large and persistent negative effects on regional and sectoral mobility. Securing a permanent contract reduces the probability of moving to a different province by 3.9 pp. in the first year and by 1.2 pp. after five years. The effects on sectoral mobility are even larger: the probability of switching sectors falls by 26.4 pp. in the first year and by 26.9 pp. after five years, both statistically significant. As discussed earlier, the sharp decline in mobility following an upgrade to a permanent position may help explain the short-lived earnings effects. While workers with permanent contracts gain stability and accumulate more work experience, they also become substantially less mobile than those who remained in fixed-term contracts at $h = 1$. We provide further evidence of the link between mobility and earnings in the next section.

Our findings, therefore, offer a cautious perspective on the frequently argued notion that temporary contracts hinder career development and earnings growth. In line with the work by Busch et al. (2025), their findings highlight that career mobility plays a key role at the individual level, while relocation can help reduce mismatches between firms and workers. However, contract type shapes the incentives for mobility. Workers on permanent contracts exhibit stronger job attachment, which can allow some low-productivity matches to persist. This may help explain why workers who have not yet transitioned to a permanent position eventually catch up to those who were only marginally upgraded.

Using a causal research design that exploits exogenous variation in the availability of permanent contracts, we find no evidence that transitioning workers from temporary to permanent contracts has any positive long-term effects on their earnings. The higher earnings growth observed among workers in permanent positions instead reflects selection, as discussed in Section 5. Of course, this does not imply that reducing labor market temporality – a goal of recent labor reforms – may not offer other important benefits.

Here, however, we do not claim that limited mobility is the only reason why workers who are not immediately upgraded may eventually catch up to those who obtain an open-ended contract earlier. Another complementary mechanism could be related to changes in effort once job security is achieved. Specifically, workers may exert higher effort when they expect that strong performance could lead to a permanent position, but once that position is secured, their effort may revert toward a lower, more average level. This "effort

Table 3: Effects of Permanent Contracts on Worker Careers (quartiles)

	Panel A: Short term effects (12 months)				
	Earnings Q_4	Earnings Q_3	Earnings Q_2	Earnings Q_1	Employment
	(1)	(2)	(3)	(4)	(5)
$p_{i,t+1}$	-0.177*	0.317***	0.187**	-0.328***	-0.205**
	(0.091)	(0.099)	(0.095)	(0.096)	(0.086)
Obs.	146,592	146,592	146,592	146,592	197,299
R2	0.062	-0.002	0.097	0.084	0.128

	Panel B: Long term effects (60 months)				
	Earnings Q_4	Earnings Q_3	Earnings Q_2	Earnings Q_1	Employment
	(1)	(2)	(3)	(4)	(5)
$p_{i,t+1}$	-0.138	0.135	0.083	-0.080	-0.125
	(0.110)	(0.119)	(0.121)	(0.118)	(0.089)
Obs.	116,440	116,440	116,440	116,440	197,299
R2	0.085	0.014	0.038	0.090	0.258

Notes: IV estimated coefficients based on equation 4. The dependent variable in each specification is an indicator for the worker being in a given quintile of the earnings growth distribution at period $t + h$. Columns (1)–(4) restrict the sample to workers employed in the corresponding month (12 or 60). Sample restrictions and controls are the same as in the reduced form exercise described in Figure 5 notes. Robust standard errors are in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

adjustment" channel is consistent with moral hazard or shirking behavior arising from the reduced incentives to signal productivity once the risk of contract termination diminishes.

7.4 Understanding the Earnings effect

Evidence from the previous section indicates that upgrading to a permanent contract yields an immediate earnings increase; however, the gap between early and later upgraders narrows over time. Mean effects, however, can mask substantial heterogeneity in the earnings response. We therefore examine the distribution of earnings consequences to assess whether the initial advantage is concentrated in particular parts of the distribution. We next re-estimate the IV specification from the previous section, replacing the continuous earnings-growth outcome with indicators for belonging to a given quintile of the earnings-growth distribution at $h = 12$ and $h = 60$. This approach highlights heterogeneity across the distribution, rather than focusing on mean effects.

Table 3 reports the IV estimates across earnings-growth quintiles. The average effect masks substantial heterogeneity: upgrading shifts mass from the bottom to the middle of the distribution. Twelve months after the termination of a fixed-term contract, workers who are upgraded to a permanent position are 16.3 pp less likely to fall into the bottom quintile and 18.1 pp less likely to be in the second quintile. Instead, their probability

Table 4: Effects of Permanent Contracts on Worker Careers (Between-Within Decomposition)

	(1) $logOEC_{-i,r,t+1}$	(2) $logOEC_{-i,r,t+6}$	(3) $logOEC_{-i,r,t+12}$	(4) $logOEC_{-i,r,t+18}$	(5) $logOEC_{-i,r,t+24}$	(6) $logOEC_{-i,r,t+48}$	(7) $logOEC_{-i,r,t+60}$
Earnings (Baseline)	0.745***	0.503***	0.305***	0.382***	0.331***	0.191	0.103
	(0.100)	(0.108)	(0.114)	(0.122)	(0.127)	(0.139)	(0.142)
Earnings (Between)	0.048	-0.023	0.122*	0.271***	0.101	0.123	0.099
	(0.043)	(0.055)	(0.067)	(0.090)	(0.084)	(0.122)	(0.129)
Earnings (Within)	0.032***	0.041	-0.021	-0.116*	-0.021	-0.091	-0.121
	(0.009)	(0.029)	(0.039)	(0.069)	(0.064)	(0.110)	(0.120)
Employment	0.664***	0.485***	0.205**	0.227***	0.251***	0.159*	0.125
	(0.083)	(0.085)	(0.086)	(0.088)	(0.089)	(0.090)	(0.089)

Notes: IV estimates from equation 4. The dependent variable in each row measures earnings growth since period 0, decomposed into within-firm growth, between-firm growth, and non-employment. Sample restrictions and controls are the same as in the reduced form exercise described in Figure 5 notes. Robust standard errors clusted at the individual level are in parentheses. $^{*}p < 0.10, ^{**} p < 0.05, ^{***} p < 0.01$.

of being in the third and fourth quintiles increases by 13.1 and 31.9 percentage points, respectively. Effects in the top quintile are small and statistically insignificant. Overall, promotion to a permanent contract shifts workers upward in the earnings-growth distribution, but not into its upper tail.

Heterogeneity in long-run consequences is even more pronounced. While the average earnings premium fades five years after the fixed-term contract ends, there is no significant mean difference between those upgraded at t+1 and those not. Panel B indicates a continued shift away from the bottom of the earnings-growth distribution, albeit with imprecise estimates. The third-quintile coefficient is about 23.7% of the short-run effect. By contrast, the fourth-quintile effect persists: upgraded workers remain roughly 15.5% more likely to be in the fourth quintile five years after expiration.

A remaining question from the previous section is how to reconcile the attenuation of average earnings growth effects with the significant differences in experience accumulation associated with earlier upgrades to permanent positions. One possible explanation is that workers who secure a permanent contract face reduced firm mobility due to greater job security, which may lessen their incentives to search for better matches. This suggests a potential trade-off between job security and the gains from advantageous firm-to-firm mobility.

To further investigate this mechanism, we decompose earnings growth into between-firm growth, within-firm growth, and zero earnings associated with non-employment. We regress earnings growth since period 0 and attribute the variation to firm mobility and within-firm changes over the 60 months following the end of the fixed-term contract. Additional details are provided in Appendix Section B.3.

Table 4 reports the decomposition results. Column (1) shows that a one–standard–deviation increase in the probability of being promoted to a permanent contract is associated with a 35.28 percent increase in earnings. As suggested earlier, this effect is driven primarily by differences in non-employment, with 8.9 percent and 6.6 percent attributable to between-firm and within-firm earnings growth, respectively. Consistent with previous results, the initial earnings advantage dissipates over time, mirroring the fade-out in employment effects. As proposed, workers who transition to a permanent contract exhibit higher earnings growth both within their current firm—reflecting internal promotions—and by moving to other firms. Specifically, a one–standard–deviation increase in the upgrading probability raises within-firm earnings by 2.34 percent in the short run. However, this effect declines over time and becomes negative after five years, although the long-run difference is not statistically significant.

Thus, workers who transition to a permanent contract initially experience positive within-firm earnings growth, but this premium dissipates quickly. In other words, while moving from a fixed-term to an open-ended contract yields an immediate earnings boost, those upgraded later eventually catch up (see Figure 5). Although between-firm earnings growth is also positively associated with upgrading, workers who remain on fixed-term contracts can still secure good matches. In contrast, earlier upgrades primarily benefit from greater job stability.

7.5 Heterogeneity

We now assess whether the effect of upgrading to a permanent contract varies with worker or firm characteristics. We analyze how the contract upgrade affects workers' earnings and employment, both in the short and long term.

Figure 7 presents heterogeneity by worker characteristics, while Figure 8 shows heterogeneity in the IV coefficients by firm characteristics, using worker and firm characteristics measured at period 0. Specifically, we examine treatment effect heterogeneity by education, gender, age, and baseline earnings (above vs. below the median). For firms, we distinguish sectors with a high vs. low share of fixed-term contracts, and further split by firm age and size. Table **??** in the Appendix reports the corresponding first-stage estimates, along with the short- and long-term effects for each subgroup.

Figure 7 reports 2SLS estimates from equation (4), estimated separately by subgroup and outcome. Short-term earnings effects are larger for men, workers with less than secondary education, and those with initially low earnings. Less-educated, male, and younger workers also display the largest differences in estimated effects on employment and cumulative earnings. The baseline earnings effect is particularly notable: workers starting with low earnings gain more in employment, whereas those with high initial earnings experience smaller employment effects but significant earnings gains. This pattern suggests that low-earning workers are more responsive to the employment margin, likely due to their higher risk of non-employment, while high-earning workers capture greater wage growth once employed.

Figure 8 shows heterogeneity by firm characteristics, with the largest earnings differences observed across sectors with varying initial shares of fixed-term contracts (FTCs). In low-FTC sectors, the earnings effect is 4.8% above the average estimate in Table 2, whereas in high-FTC sectors it is 65.2% below the average. The disparity is even more pronounced for cumulative earnings and employment: in high-FTC sectors, we find no significant employment effect, consistent with jobs in these sectors being relatively less secure.

The short-term differences by worker characteristics discussed earlier translate into larger disparities in cumulative earnings and work experience over the long run. Notably, we find substantial variation in the likelihood of ever changing sectors or provinces, with the sharpest contrast between workers with high and low initial earnings. Among high-earning workers, upgrading to a permanent position significantly reduces the probability of changing sector or province, whereas no significant effect is observed for low earners. This pattern highlights a trade-off between job mobility and earnings growth. For workers with initially high earnings, the scope for further gains through mobility is more limited, making job stability relatively more valuable.

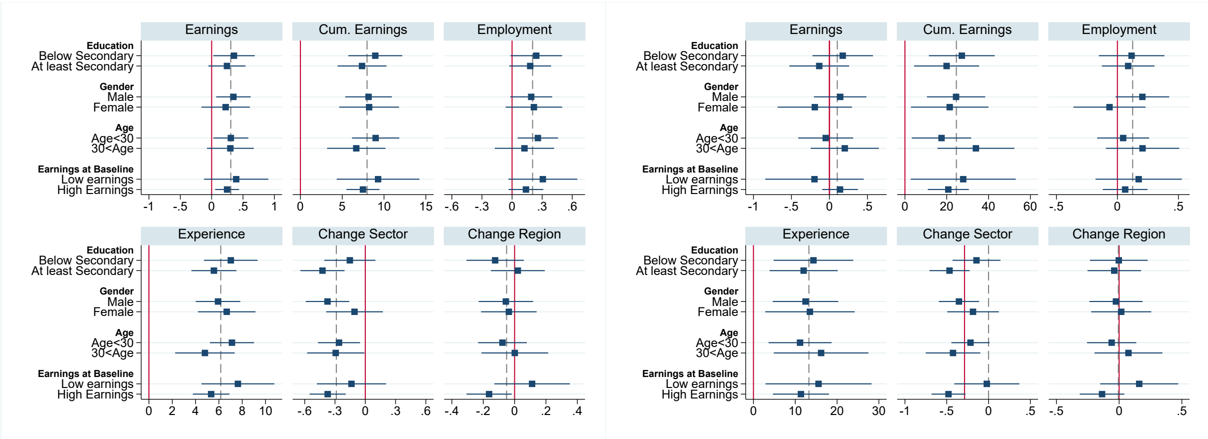
Examining firm heterogeneity, we find that in the long run the largest gains accrue to workers initially employed in young firms, who experience the greatest improvements in both employment and cumulative earnings 60 months after the end of their fixed-term contracts. In contrast, workers starting in large firms are less likely to change sector or region compared to those in small firms.

7.6 Compliers

A central feature of instrumental-variables (IV) designs is that they identify the Local Average Treatment Effect (LATE)—the causal effect for *compliers*, i.e., the individuals whose treatment status shifts in response to the instrument (Angrist et al., 1996). This interpretation is particularly relevant in settings where the instrument shifts treatment only for a subset of workers, such as those near institutional thresholds or contract-renewal margins. Although the LATE reflects the effect for only part of the population, this group is typically of direct policy interest because their behavior is marginal to the institutional variation generating identification.

Figure 7: Heterogeneity of the Effect of Permanent Contracts on Worker Careers (Worker characteristics)

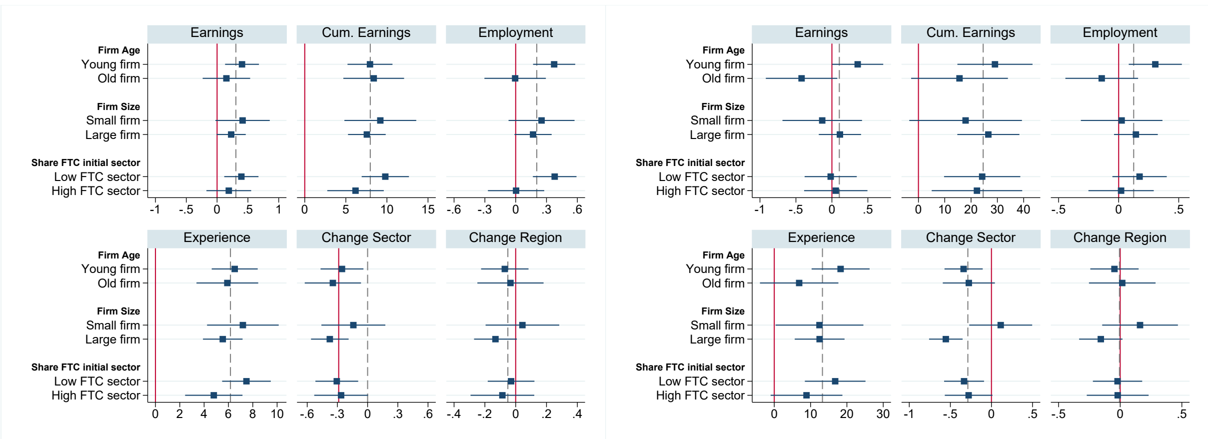
(a) 12 months (b) 60 months



Notes: IV estimated coefficients based on equation 4. Sample restrictions and controls are the same as in the reduced form exercise described in Figure 5 notes. The dashed line represents the coefficients presented in Table 2.

Figure 8: Heterogeneity of the Effect of Permanent Contracts on Worker Careers (Firm characteristics)

(a) 12 months (b) 60 months



Notes: IV estimated coefficients based on equation 4. Sample restrictions and controls are the same as in the reduced form exercise described in Figure 5 notes. The dashed line represents the coefficients presented in Table 2.

Heterogeneity in treatment effects implies that the composition of compliers determines whose causal effect the IV estimate recovers. As emphasized by Angrist and Pischke (2009) and Heckman et al. (2006), the LATE may differ from the population-average effect when responses vary across workers, making it essential to characterize compliers to assess external validity and to clarify the mechanisms underlying the estimated effects.

In this section, we characterize the complier group generated by the exogenous variation in the creation of new open-ended positions. This analysis clarifies the specific population underlying the IV estimates presented in Section 7 and provides insights into the types of workers whose outcomes drive the estimated effects.

Characterizing compliers with a continuous treatment is more nuanced than in the binary case. With a continuous treatment, compliers do not form a single group; instead, they correspond to marginal individuals whose treatment intensity changes with marginal shifts in the instrument. This follows the interpretation of IV as identifying marginal treatment effects (Heckman and Vytlacil, 2005). For ease of interpretation, in this section, we therefore work with a binary approximation of the instrument. Specifically, we residualize the number of new open-ended contracts using the set of individual, regional, and time controls described in Section 6.1, and define workers as treated if their residualized value is positive. This procedure partitions the sample into treated and non-treated groups in a manner that is consistent with the direction of the underlying continuous first-stage relationship.

For the empirical implementation, we rely on the statistical package introduced by Marbach and Hangartner 2020. As they emphasize, a key advantage of the decomposition into compliers, always-takers, and never-takers is that it rests on the same assumptions required for identifying the LATE—specifically, instrument independence and the monotonicity condition. The mean characteristics of compliers are obtained as the difference in average characteristics between individuals whose treatment status is shifted by the instrument. Standard errors are computed via bootstrap to account for sampling variability in this decomposition.

8 Two endogenous variables

In this section, we explore whether part of the earnings effect documented in Section 7 is driven by the employment consequences of holding an OE contract. Specifically, we extend the specification to control not only for OE status in period $t + 1$ but also for

Table 5: Average Characteristics of Always-Takers, Compliers, and Never-Takers

	Panel A: Worker characteristics			
	At least Secondary Education	Age<30	Above median Initial Earnings	Female
Complier	0.467	0.657	0.502	0.358
	(0.048)	(0.048)	(0.050)	(0.050)
Always-taker	0.485	0.548	0.544	0.444
	(0.002)	(0.002)	(0.003)	(0.002)
Never-taker	0.399	0.566	0.473	0.410
	(0.002)	(0.002)	(0.002)	(0.002)

	Panel B: Firm characteristics		
	Large firm	Young firm	High sectoral share FTC
Complier	0.591	0.311	0.424
	(0.050)	(0.052)	(0.050)
Always-taker	0.524	0.462	0.349
	(0.002)	(0.002)	(0.002)
Never-taker	0.485	0.474	0.557
	(0.002)	(0.002)	(0.002)

Notes: Descriptive statistics for complier and non-complier subpopulations based on individuals whose treatment status (the probability of being upgraded to an open-ended contract) is affected by the creation of new open-ended contracts. The instrument is constructed as the residualized count of new open-ended contracts, with individuals classified as one when this residualized count is positive. Standard errors are obtained by bootstrap.

Table 6: Effect of Permanent Contracts on Worker Careers (Endogenous Employment)

	First Stage		Second Stage			
	(1)	(2)	(3)	(4)	(5)	(6)
	$ContractOE_{t+1}$	$Employed_{t+1}$	Earnings +12		Earnings +60	
$p_{i,t+1}$			0.098	-0.060	0.057	-0.078
			(0.118)	(0.171)	(0.145)	(0.219)
$e_{i,t+1}$				0.546**		0.465
				(0.255)		(0.327)
$\log OEC_{-i,r,t+1}$	0.093***	0.027***				
	(0.009)	(0.009)				
$\log TNC_{-i,r,t+1}$	0.016	0.077***	0.040**		0.034	
	(0.014)	(0.015)	(0.019)		(0.023)	
Obs	197,433	197,433	197,433	197,433	197,433	197,433

Notes: The table reports the first-stage and IV estimates from Equation 4. Sample restrictions and controls are identical to those in the reduced-form specification described in the notes to Figure 5, except that the specification here additionally includes leads and lags of the leave-out measures for new OECs and total contracts. New OECs are measured at the month–autonomous-community level. Columns (1) and (2) present the first-stage results for OEC status and employment in period $t + 1$, respectively. Columns (3) and (4) report the second-stage estimates for changes in earnings 12 periods later, where the endogenous variables—OEC status and employment in $t + 1$—are instrumented using the leave-out measures of new OECs and total contracts. Columns (5) and (6) repeat the analysis using earnings 60 months later as the dependent variable. Columns (3) and (5) include only one endogenous variable (OEC status), whereas Columns (4) and (6) include both endogenous variables, OEC status and employed in $t + 1$. Robust standard errors, clustered at the individual level, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

employment status in $t + 1$, as shown in the following equation:

$$y_{it+h} = \beta_1 p_{i,t+1} + \gamma_1 emp_{t+1} + \sum_{k\neq 1,,k=-24}^{24} \beta_k \log OEC_{-i,r,t+k} + \sum_{k\neq 1,,k=-24}^{24} \gamma_k \log TNC_{-i,r,t+k} + X'_{it}\theta + \epsilon_{it}, \tag{5}$$

where the specification follows Equation 4, but additionally includes employment status in $t + 1$. Since employment status may itself be endogenous, we instrument for both endogenous variables using two instruments: the first lead of new OE contracts and the first lead of total contracts. The inclusion of total contracts as an instrument is justified by its correlation with employment status in $t + 1$ (relevance condition). Both the leave-out measures of new OE contracts and total contracts are constructed at the month–autonomous community level to ensure sufficient variation when instrumenting for two endogenous variables.

9 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 measures from 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 opening 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 uses data from Social Security records, accessible online via PX-Web.[27] 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 those of new open-ended contracts constructed from the MCVL.

Figure C.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 supports a strong correlation between them.

As an additional robustness check, Table D.4.1 uses our instrument as described in

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

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 creating 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 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 C.5.2 in the Appendix, we find similar patterns in these alternative specifications.[28] 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 C.6).

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

[28] Additionally, the different instrument alternatives show similar predictive power. The R^2 values for the national, province, sectoral, and sector-by-province instruments are 9.32, 9.34, 9.37, and 7.67, respectively.

specification in equation (??), 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 captured general business cycle conditions, we would also expect a positive impact on employment for these workers. Appendix Figure C.10.1 illustrates that there is no significant nor systematic employment response, ruling out this possibility.

Exogenous reason of dismissal and tenure restrictions

Section 6.2 detailed the sample restrictions used in our estimation, focusing on workers employed in the expiring fixed-term contract for at least 0.8 months to eliminate extremely short contracts. In this subsection, we extend our analysis with two additional exercises. First, we limit the sample to spells where the reason for dismissal is unrelated to firm-specific factors. Second, we demonstrate that the first-stage results remain robust when we apply alternative tenure restrictions.

First, we leverage an additional variable in the MCVL that records the reason for terminating an employer-employee spell. To refine our analysis, we further restrict the sample to spells where the dismissal reason is linked explicitly to the expiration of a fixed-term contract.[29] Then, we repeat the first stage, where we study the effect of our instrument on the probability of holding an open-ended contract. The corresponding figure is provided in Section C.7 of the appendix, while the results are presented in Figure C.7.1. This exercise confirms the relevance of our instrument in influencing the likelihood of obtaining an open-ended contract following the termination of a fixed-term contract.

Second, as described in Section 6.2, 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 C.11, we extend the reduced-form analysis by widening the tenure window to 0.4–2 years and alternative tenure restriction in that range. 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

[29] We use Code 54: Involuntary Dismissal and Code 93: End of Fixed-Term Contract.

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 C.8.1 and C.8.2. 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.

10 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 experienced 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 permanent and temporary contracts 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 workers 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 but highly predictive of their probability of securing a permanent position. This allows us to study the causal effect of entering a permanent contract for "compliers", i.e., workers 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 experienced 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. 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 treated workers accumulate substantially more work experience: 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 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., 2020; 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).

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.

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.

Angrist, J. D., G. W. Imbens, and D. B. Rubin (1996): "Identification of causal effects using instrumental variables," *Journal of the American statistical Association*, 91, 444–455.

Angrist, J. D. and J.-S. Pischke (2009): *Mostly harmless econometrics: An empiricist's companion*, Princeton university press.

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., A. Cabrales, and M. Jansen (2023): "Does Dual Vocational Education and Training Pay Off?" .

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 (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.

Boeri, T. and P. Garibaldi (2024): "Temporary Employment in Markets with Frictions," *Journal of Economic Literature*, 62, 1143–85.

Booth, A. L., M. Francesconi, and J. Frank (2002): "Temporary Jobs: Stepping Stones or Dead Ends?" *The economic journal*, 112, F189–F213.

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

Burdett, K. and D. Mortensen (1998): "Wage Differentials, Employer Size, and Unemployment," *International Economic Review*, 39, 257–73.

Busch, C., I. Galvez, E. González-Aguado, and L. Visschers (2025): "Dual Labor Markets, Unemployment, and Career Mobility," *Working Paper*.

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*.

Cattaneo, M. D., R. K. Crump, M. H. Farrell, and Y. Feng (2024): "On binscatter," *American Economic Review*, 114, 1488–1514.

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.

Heckman, J. J., S. Urzua, and E. Vytlacil (2006): "Understanding instrumental variables in models with essential heterogeneity," *The review of economics and statistics*, 88, 389–432.

Heckman, J. J. and E. Vytlacil (2005): "Structural equations, treatment effects, and econometric policy evaluation 1," *Econometrica*, 73, 669–738.

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.

Marbach, M. and D. Hangartner (2020): "Profiling compliers and noncompliers for instrumental-variable analysis," *Political Analysis*, 28, 435–444.

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 Sections

B Descriptive Evidence

B.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:

$$\ln w_{irt} = \beta_1 exp_{it}^{FTC} + \beta_2 (exp_{it}^{FTC} \times exp_{it}) + \beta_3 exp_{it}^{OEC} + \beta_4 (exp_{it}^{OEC} \times exp_{it}) + X'_{it}\mathbf{\Omega} + \sigma_r + \psi_t + \varepsilon_{irt}, \tag{6}$$

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

Instead of the typical quadratic form of homogeneous returns to experience, equation (6) 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 B.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.[30] 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.[31]

As we show in Section 5, our descriptive estimates here have, however, 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.

[30] Based on these results, Figure B.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%.

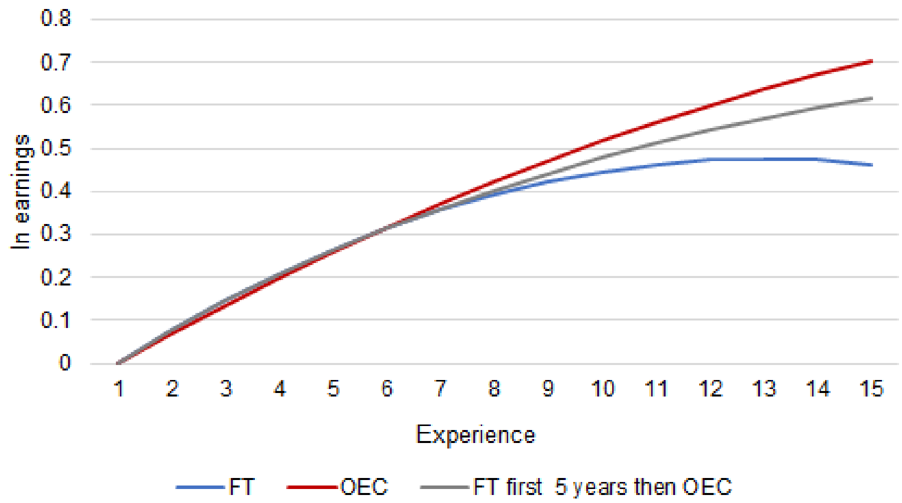
[31] One alternative that Garcia-Louzao et al. (2023) implement later on is to instrument experience and tenure using their deviations relative to the average computed within the contract and match the history of the worker. Additionally, they exploit supplementary instruments based on regional variations 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 B.1.1: Wage growth in fixed-term and open-ended contracts (Mincer regression)

	Dependent variable: ln earnings		
	(1)	(2)	(3)
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
Individual FEs	No	No	Yes

Notes: exp, exp_{FTC}, 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, province 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. $^{*}p < 0.05,^{**} p < 0.01,^{***} p < 0.001$

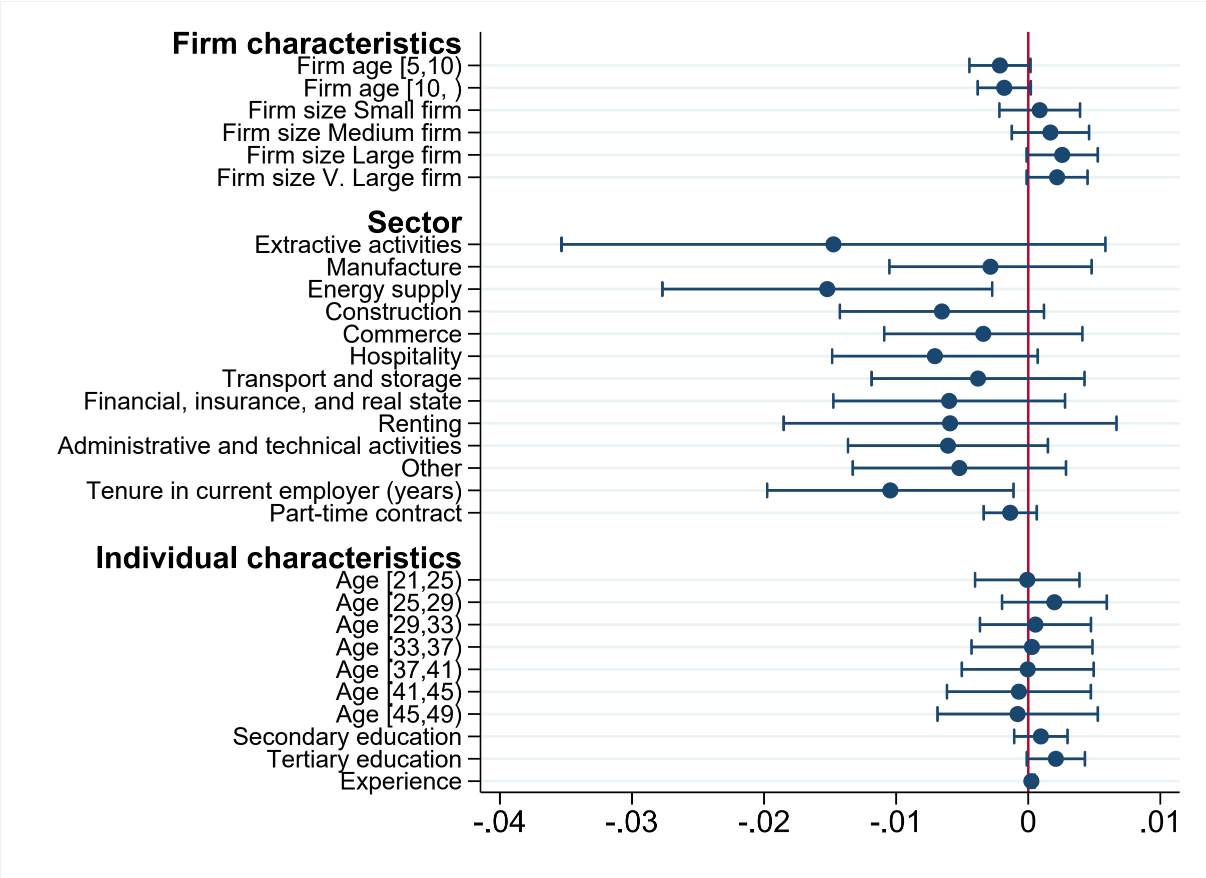
Figure B.1.1: Heterogeneous returns to experience by contract type



Notes: Fitted values based on the coefficient estimates from Column (3) in Table B.1.1.

B.2 Independence test

Figure B.2.1: Effect of individual, firm characteristics and sector on $logOEC_{t+1}$



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).

We argue that the number of new permanent positions available when a worker's contract is about to end is effectively random from the worker's perspective and thus exogenous. While we cannot test it directly, we assess the validity of the independence assumption by examining whether the instrument correlates with observable individual or firm characteristics.

In Figure B.2.1, we present the coefficients from a regression of the standardized version (mean zero and variance one) of the instrument $logOEC_{-i,r,t+1}$ on our usual set of control variables, including year, month, province, and sector fixed effects, individual-level characteristics, and leads and lags of the number of new permanent and new temporary contracts – mirroring the specification of our first stage. As an exception, to ease the interpretation of the coefficients on education, we do not interact education with age here. The different panels of the figure report the coefficient estimates for different groups of observable characteristics.

Panel (a) shows no significant relationship between the instrument and worker characteristics, such as age, experience, or education. Panel (b) explores the correlation between the instrument and the worker's sector at baseline. Although its correlation with the indicator for extractive activities is statistically significant, the effect is minimal: the number of new contracts is 3% of a standard deviation below the mean. Finally, Panel (c) examines the correlation with firm characteristics, specifically firm age and size. Again, the correlation between these dimensions and the number of new open-ended contracts is negligible. The results suggest that, conditional on our standard set of fixed effects, the degree of selection is quite limited.

An additional concern is that our instrument may be correlated with the quality of OECs. Certain firms might offer many OECs in a particular month, potentially differing in quality from those in other months. To address this, we provide additional evidence showing that the instrument is not correlated with key job characteristics, including the average duration of new OECs, the average required skill level, and average initial wages. Specifically, we calculate the average initial wages, average occupation skill level, and average tenure of new OECs at the province, year, and month level. We estimate these outcomes using the following equation:

$$y_{rt} = \beta logOEC_{-i,r,t+1} + \sum_{k\neq 1} \alpha_k logOEC_{-i,r,t+k} + \mu_r + \psi_t + \varepsilon_{rt}, \tag{7}$$

where y_{rt} represents the outcome variable measured at the province, year, and month level. The term $\log OEC_{-i,r,t+1}$ denotes the first lead of the log number of new open-ended contracts, while we additionally control for up to 24 leads and lags of this variable. Similarly, $\log FTC_{-i,r,t+k}$ represents the log number of new fixed-term contracts. Finally, X_{it} includes the same individual control variables as in our main specification.

Table B.2.1 presents the results on the composition of OECs using our instrumental variable. The findings indicate no significant relationship across any examined outcomes, suggesting that our instrument captures only variations in the quantity of OECs, not their composition.

B.3 Robustness: Between vs. Within earnings change

Panel (a) of Figure 5 shows that workers who experience a high number of new open-ended contracts at the time of contract expiration initially enjoy a rise in earnings. However, this growth gradually attenuates over time, and by 60 months after the shock, the earnings differences associated with the initial increase in new OECs are no longer statistically significant. To better understand the drivers of this initial earnings growth and its subse-

Table B.2.1: Effect of new open-ended contracts on the composition of new contracts

	(1) Earnings	(2) Occupation	(3) Tenure
$logOEC_{-i,r,t+1}$	-13.01	0.055	-0.048
	(14.06)	(0.039)	(0.073)
Constant	1337.9***	6.342***	8.904***
	(158.1)	(0.312)	(0.568)
Obs.	8,625	8,625	8,625
R2	0.219	0.274	0.440
Time FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes

Notes: The table reports the coefficients of the average starting earnings, average occupation skill level, and average tenure of new OECs at the province, year, and month level on the log number of new open-ended contracts in $t+1$. The additional controls are the same as in our baseline specification. Robust standard errors are in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

quent attenuation, we decompose the effect into its underlying components: between-firm earnings growth, within-firm earnings growth, and the impact of non-employment or unemployment.

Earnings growth between period t (the contract expiration date) and $t+h$ can be expressed as $\frac{e_{t+h}-e_t}{e_t}$. This measure captures positive changes in the case of the worker being employed or equals -1 when the worker has zero earnings in period $t+h$, indicating non-employment. In cases where the worker remains employed, the observed earnings growth can be further decomposed into within-firm earnings growth and between-firm earnings growth components. Specifically, we apply the following decomposition:

$$\begin{aligned}\frac{e_{t+h}-e_t}{e_t} &= \frac{e_{t+h}-e_{t+h-1}}{e_t}+\cdots+\frac{e_{t+1}-e_t}{e_t}\\ &= \left(\frac{e_{t+h}-e_{t+h-1}}{e_t}+\cdots+\frac{e_{t+h-k}-e_{t+h-k-1}}{e_t}+\frac{e_{t+h-k-2}-e_{t+h-k-3}}{e_t}+\cdots+\frac{e_{t+1}-e_t}{e_t}\right)+\\ &+\left(\frac{e_{t+h-k-1}-e_{t+h-k-2}}{e_t}\right)\\ &= \underbrace{\left(\frac{e_{t+h}-e_{t+h-k-1}}{e_t}+\frac{e_{t+h-k-2}-e_t}{e_t}\right)}_{Within}+\underbrace{\left(\frac{e_{t+h-k-1}-e_{t+h-k-2}}{e_t}\right)}_{Between}\end{aligned}$$

The decomposition above follows the period-by-period change in earnings from period t to $t+h$. For ease of exposition, we assume that the worker experiences one firm change between periods $t+h-k-2$ and $t+h-k-1$. Accordingly, the change in earnings between these two periods captures the between-firm earnings growth. We further assume that the worker remains continuously employed at the same firm throughout the remaining periods. However, in our exercise, we allow for several firm changes and non-employment periods between periods $t + h$ and t.

Based on the outcomes obtained from the previous decomposition, we estimate the reduced form specification of the $logOEC_{-i,t,t+1}$ on the within, between, and employment components of earnings. The results from this decomposition are presented in Table B.3.1.

Table B.3.1: Earnings decomposition (Reduced form)

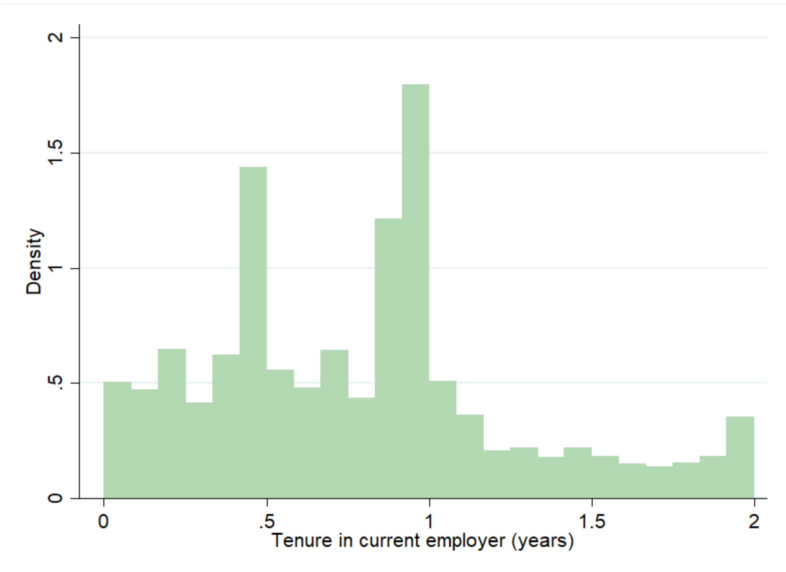
	(1) $logOEC_{-i,r,t+1}$	(2) $logOEC_{-i,r,t+6}$	(3) $logOEC_{-i,r,t+12}$	(4) $logOEC_{-i,r,t+18}$	(5) $logOEC_{-i,r,t+24}$	(6) $logOEC_{-i,r,t+48}$	(7) $logOEC_{-i,r,t+60}$
Earnings (Baseline)	0.053***	0.037***	0.014**	0.021***	0.010	0.005	-0.001
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)
Earnings (Between)	0.005*	0.000	0.004	0.009**	0.001	-0.002	-0.004
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)
Earnings (Within)	0.003***	0.004**	-0.001	-0.003	-0.002	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)
Employment	0.045***	0.033***	0.010**	0.015***	0.011**	0.008	0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)

Notes: Earnings decomposition.

C Supplementary Figures

C.1 Fixed-term contracts in Spain

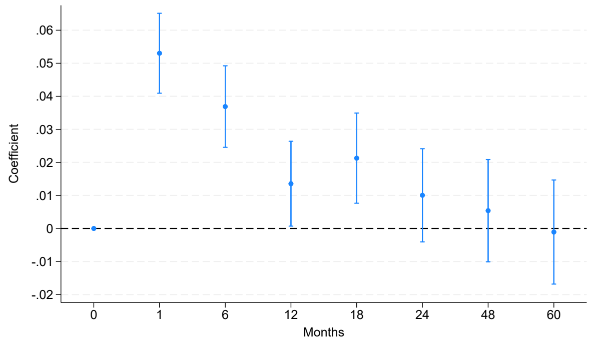
Figure C.1.2: Maximum tenure at the expiration of fixed-term contracts



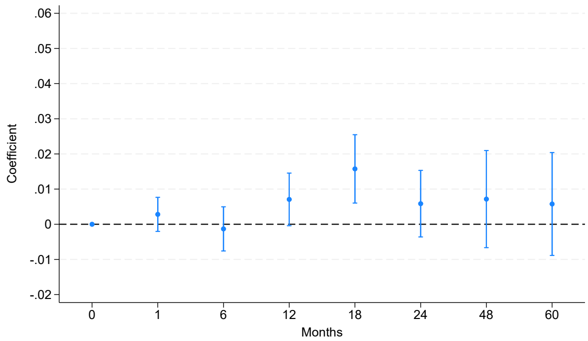
Notes: Distribution of maximum tenure in fixed-term contracts 1998-2021.

Figure B.3.1: Earnings decomposition: Reduced form

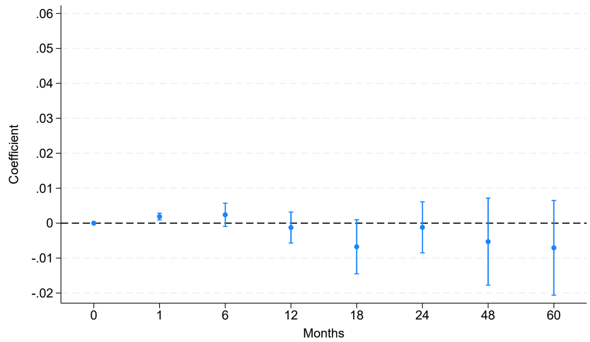
(a) Earnings (Baseline)



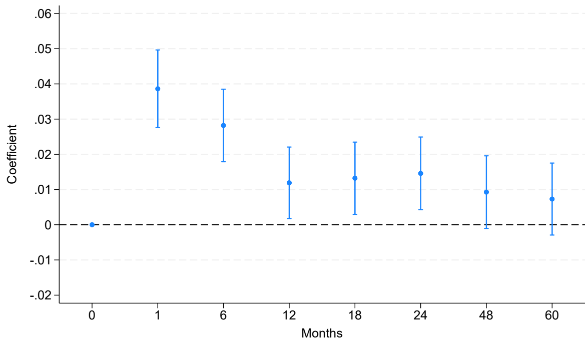
(b) Between effect



(c) Within effect



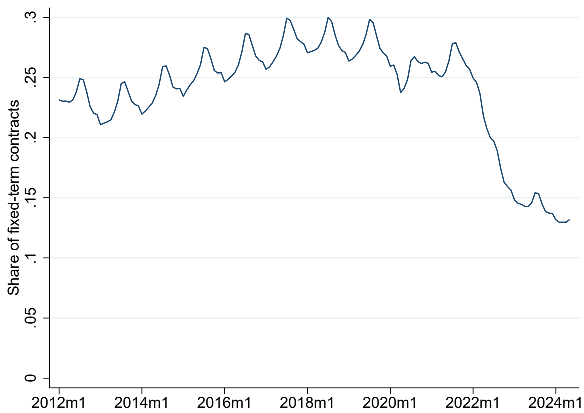
(d) Non-Employment effect



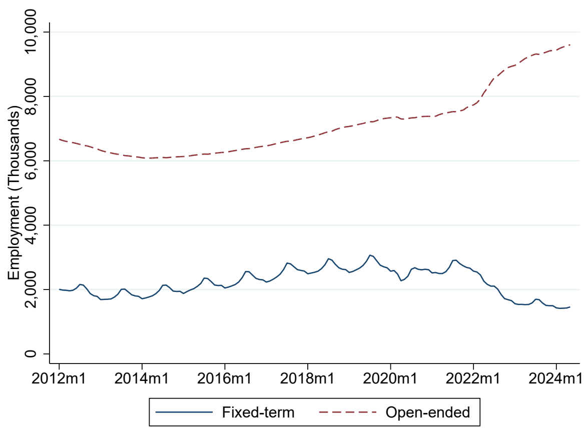
Notes: The sample consists of workers in the last month of a fixed-term position in event period $h = 0$, with at least 0.8 but less than 1.2 years of tenure. Period 1998-2017. The coefficients correspond to the effect of the first lead of the *log* number of new permanent contracts ($logOEC$) on each outcome. All regressions control for the leads and lags of $logOEC$ and 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.

Figure C.1.3: Descriptive statistics on fixed-term contracts from Social Security records

(a) Share FTC

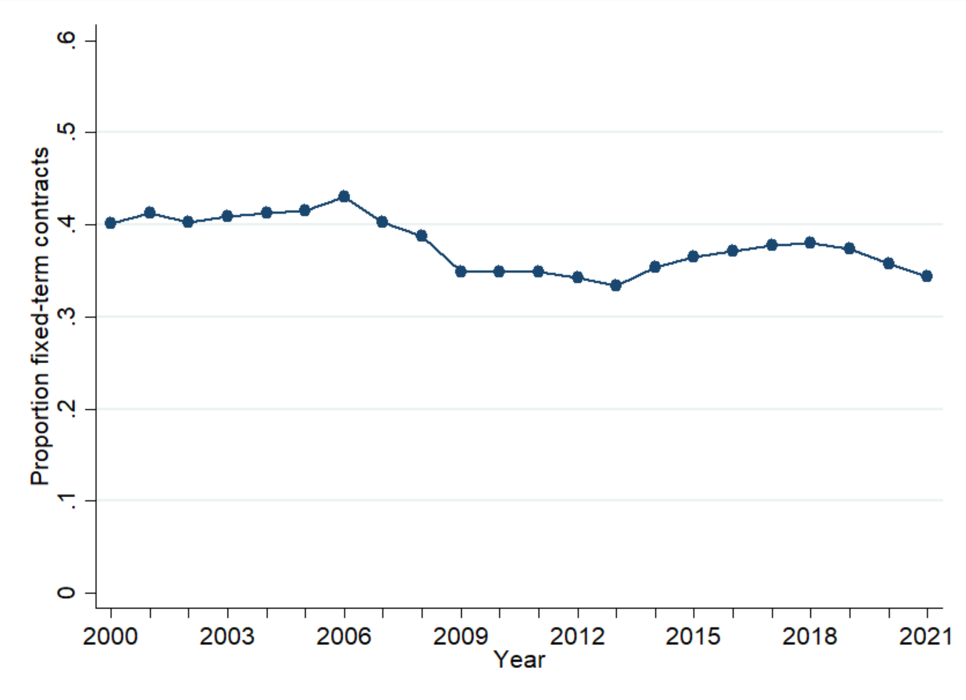


(b) Count of workers by contract



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

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

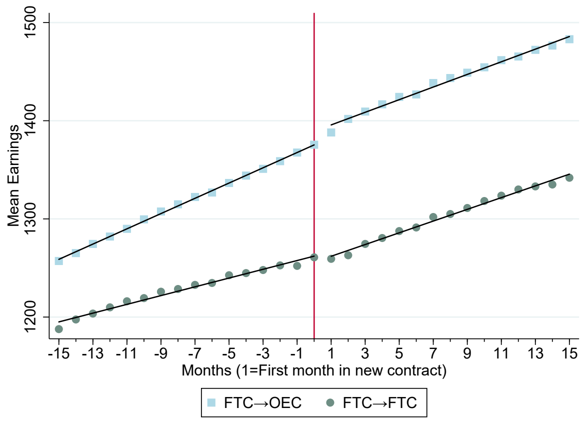


Source: MCVL

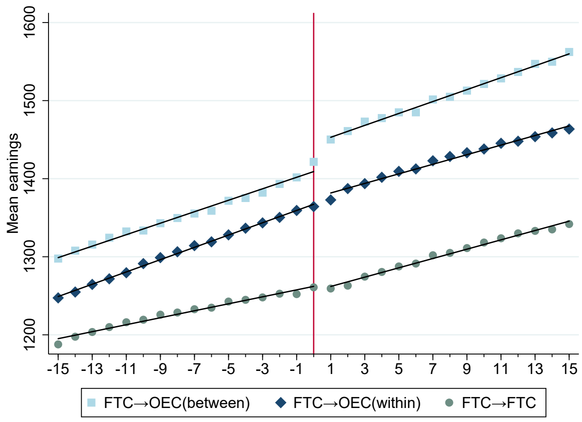
C.2 Selection: Additional Results

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

(a) Baseline

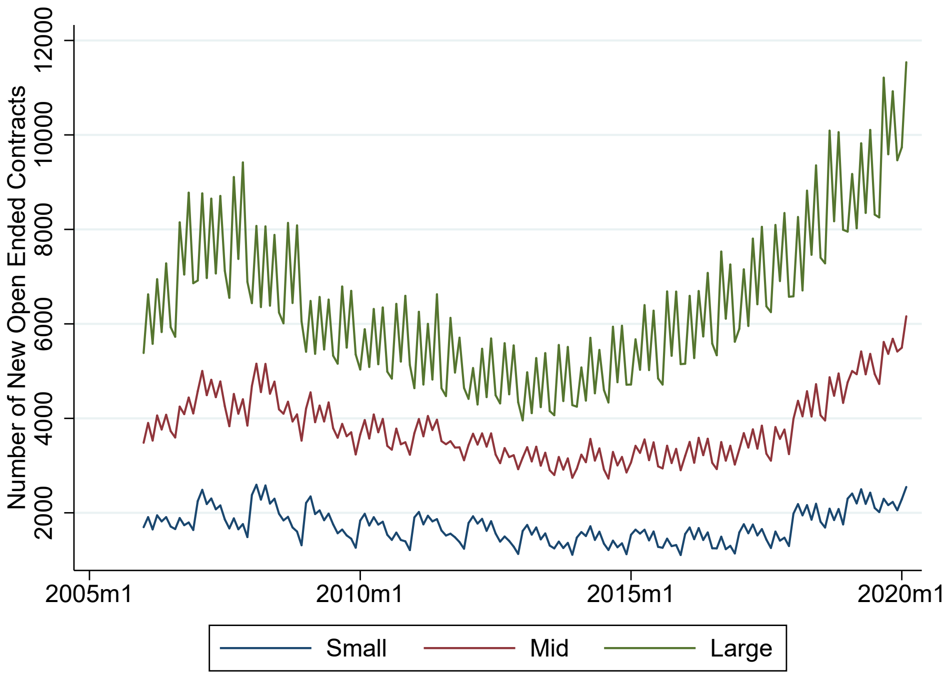


(b) Within/Between



Notes: The figure shows Spanish workers' mean 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: FTC → OEC (transitioning to an open-ended contract) or FTC → FTC (transitioning to another fixed-term contract). Panel (a) presents the mean 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 (FTC → OEC-Same Firm) and transitions to an open-ended contract within the same firm (FTC → OEC-New Firm).

Figure C.1.4: New open-ended contracts by firm size

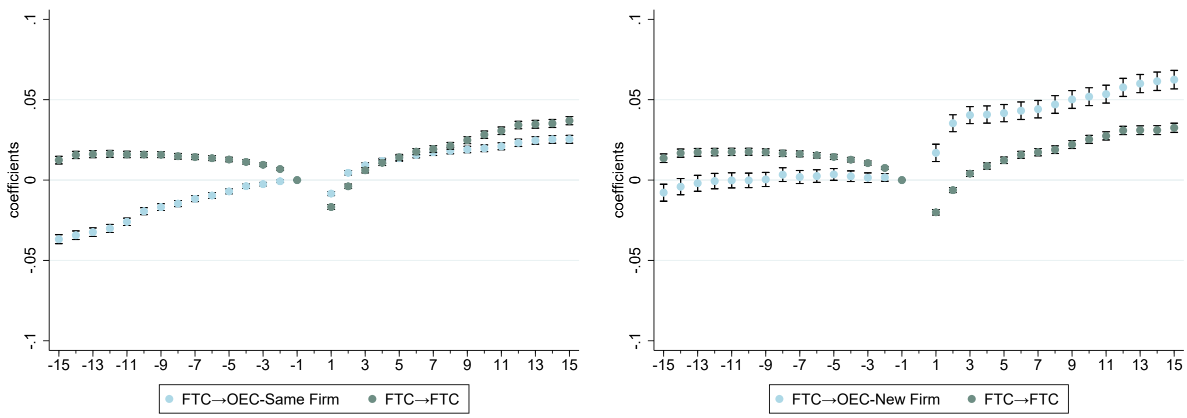


Notes: Count of new open-ended contracts by firm size, with firms categorized into terciles based on the annual firm size distribution.

Figure C.2.2: Evolution of earnings before and after switching within or to a new firm

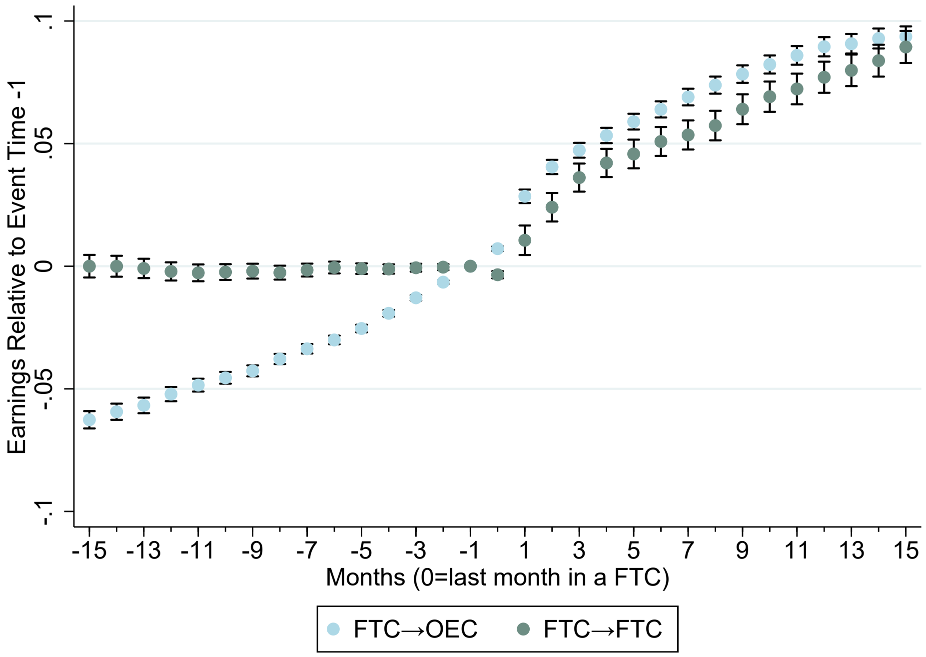
(a) FTC-OEC Within Firm

(b) FTC-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 the notes of Figure 3. Panel (a) presents fixed-term to fixed-term transitions (FTC-FTC) along with fixed-term to open-ended transitions (FTC-OEC) that occur *within* the same firm. Panel (b) presents FTC-FTC transitions and FTC-OEC transitions to a *new* firm. Controls include interactions of event time with educational attainment and sector.

Figure C.2.3: Evolution of earnings before and after switching within or to a new firm

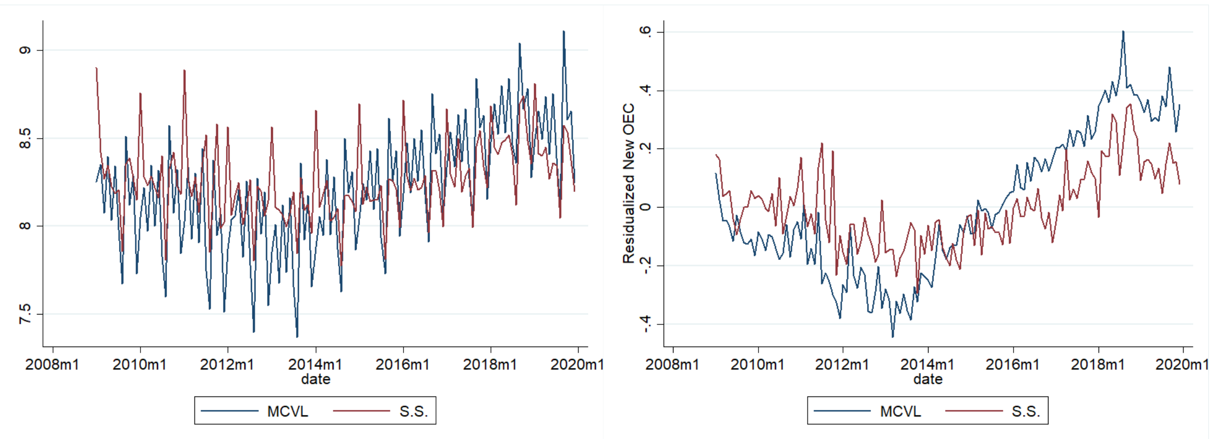


Notes: The figure shows event time coefficients estimated from equation 2 for workers transitioning to open-ended contracts or a new fixed-term contract. The exercise is described in the notes of Figure 3. The sample is restricted to the first contract transaction for a worker.

C.3 Social Security records

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

(a) Baseline | (b) Residualized

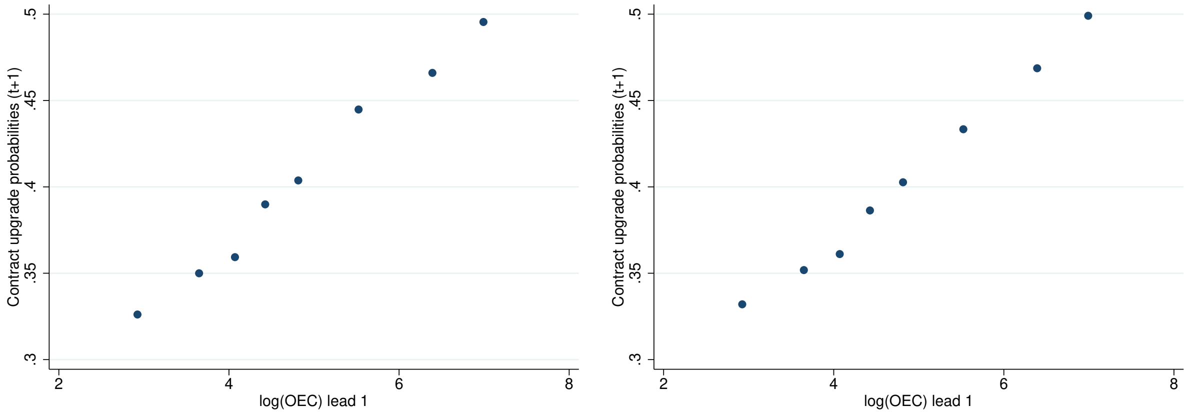


Notes: Sum of NewOEC obtained from Social Security records (OEC_{Total}) and from the MCVL. Panel (a) displays the monthly sum of New open-ended contracts from both data sources. Panel (b) residuals the sum of new Open-Ended contracts (OEC) by subtracting the variation explained by month-fixed effects.

C.4 Binscatter

Figure C.4.1: Binned scatter plots of the $OEC_{-i,r,t+1}$ on the probability of contract upgrade in $t+1$

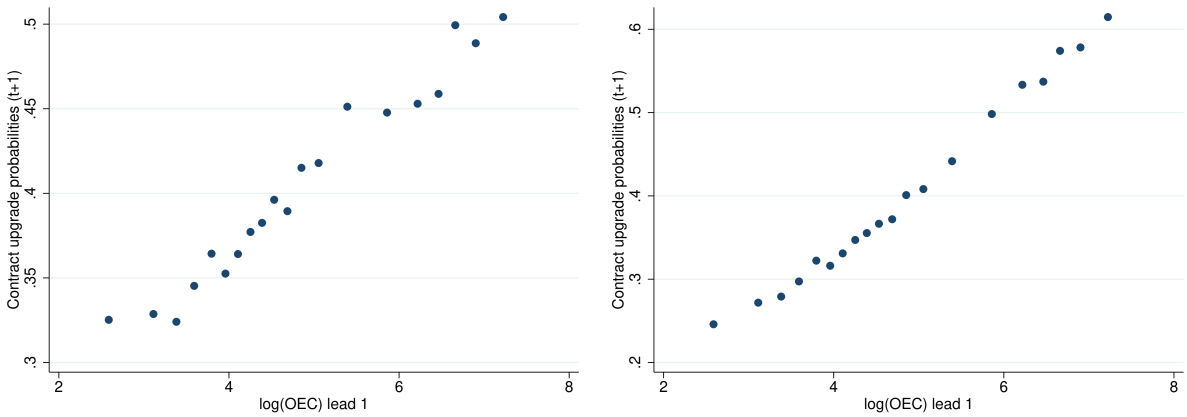
(a) Unconditional (b) FE Controls



Notes: This figure shows a binscatter plot with the dependent variable indicating whether a worker on an ending fixed-term contract transitions to an open-ended contract in the period $t+1$. The independent variable is the first lead of the logarithm of the number of new open-ended contracts in the same month-year and province as the worker's ending contract. The plot is a covariate-adjusted binscatter proposed by Cattaneo et al. (2024). Panel (a) presents the unconditional relationship, while Panel (b) controls for the sector, year, month, and province fixed effects. The number of bins is equal to 10 and chosen by the algorithm as the optimal number.

Figure C.4.2: Binned scatter plots of the $OEC_{-i,r,t+1}$ on the probability of contract upgrade in $t+1$

(a) Unconditional (b) FE Controls

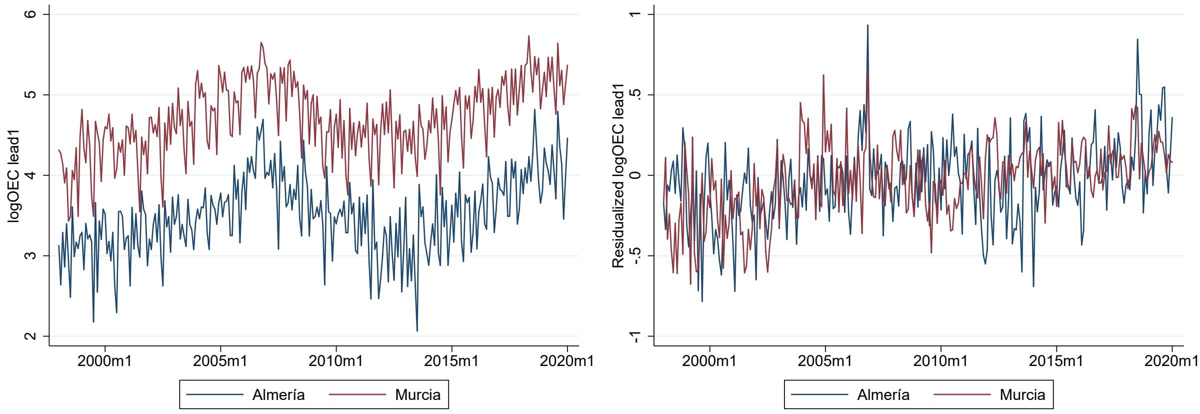


Notes: This figure shows a binscatter plot with the dependent variable indicating whether a worker on an ending fixed-term contract transitions to an open-ended contract in the period $t+1$. The independent variable is the first lead of the logarithm of the number of new open-ended contracts in the same month-year and province as the worker's ending contract. The plot is a covariate-adjusted binscatter proposed by Cattaneo et al. (2024). Panel (a) presents the unconditional relationship, while Panel (b) controls for the sector, year, month, and province fixed effects. The number of bins is set to 20.

C.5 First Stage

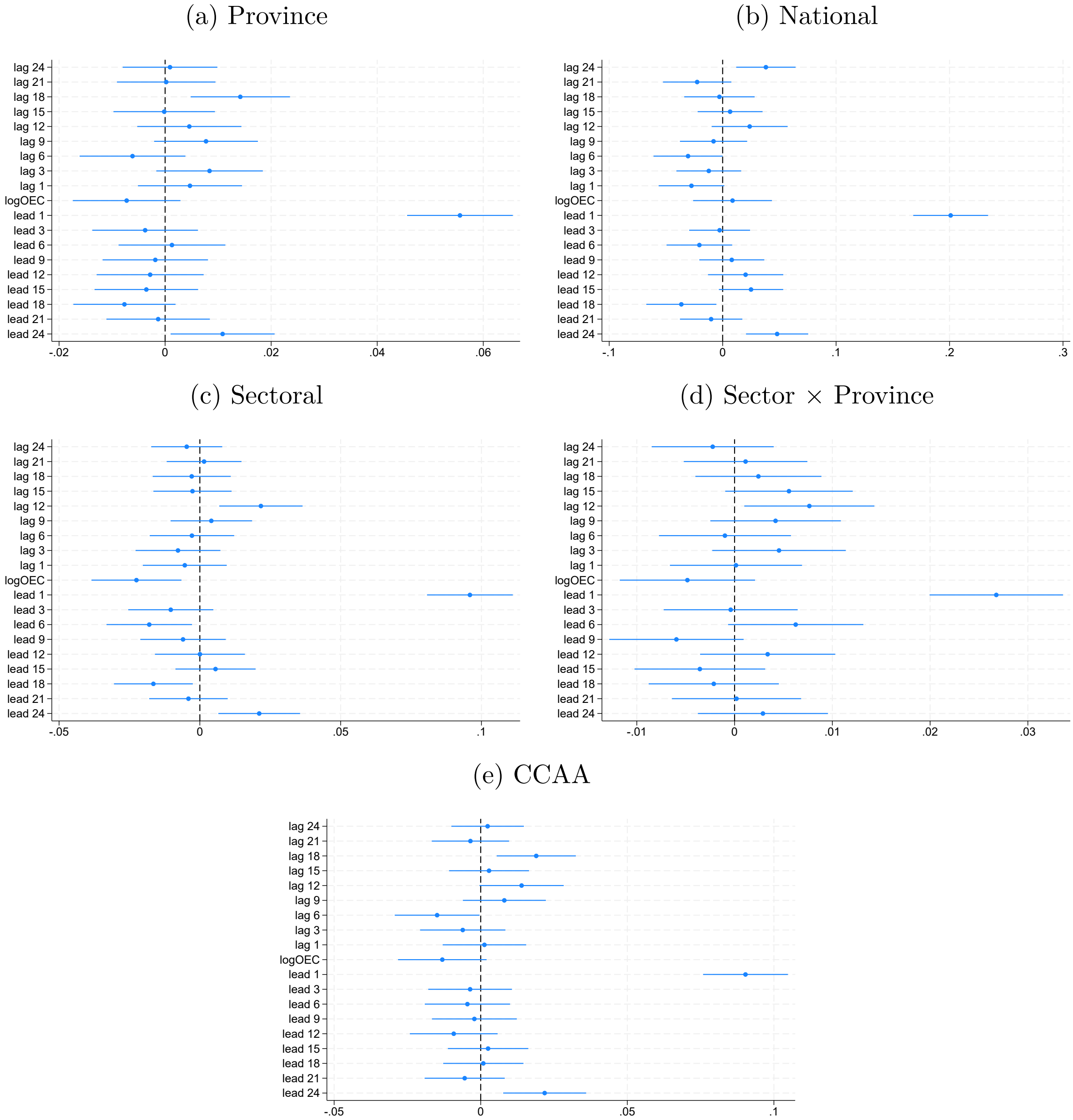
Figure C.5.1: LogOEC lead1: Province Instrument

(a) $logOEC_{lead1}$ (b) $logOEC_{lead1}$ (Residualized)



Notes: Panel (a) plots the log of new OECs by province and year–month from 1998 to 2020 for Almería and Murcia. Panel (b) presents the residualized log of new OECs after removing province, year, and month fixed effects.

Figure C.5.2: First stage: National, Province, Sectoral, and Sector × Province Instrument



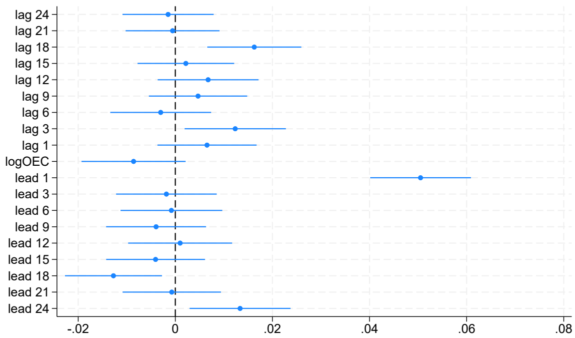
Notes: The sample consists of workers who were in the last month of a fixed-term contract in event period $h = 0$, with at least 0.8 but less than 1.2 years of tenure. The coefficients correspond to the effect of leads and lags of the *log* of new open-ended contracts on the probability of switching to an open-ended contract in $t + 1$. Panel (a) presents our baseline specification. Panel (b) employs variation in the opening of permanent positions at the national level. Panel (c) exploits the opening of permanent positions by sector. Panel (d) exploits province by sectoral variation. Panel (d) variation at the monthly and autonomous community level. 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 number of new fixed-term contracts.

C.6 Robustness: Job Seasonality

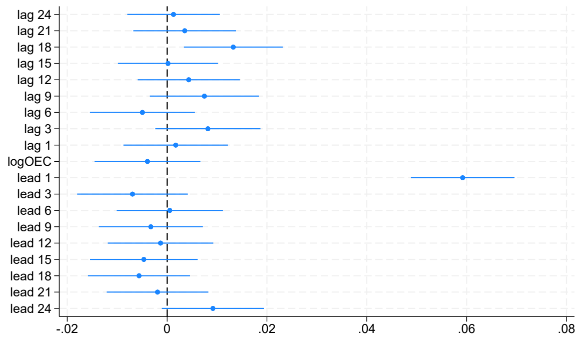
Figure C.6.1: Regional Instrument

Removing specific months

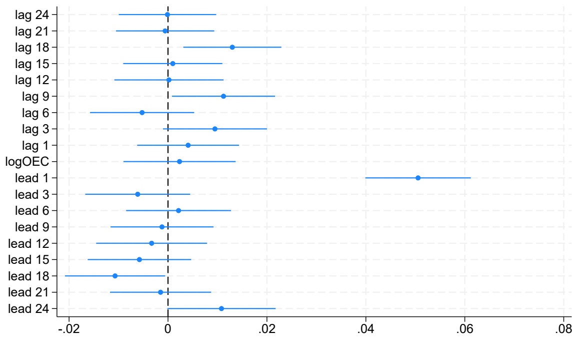
(a) No August



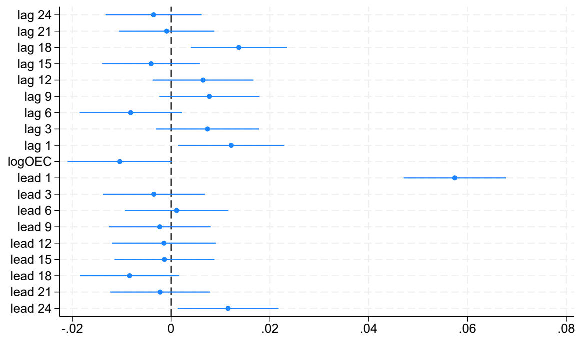
(b) No September



(c) No December



(d) No January



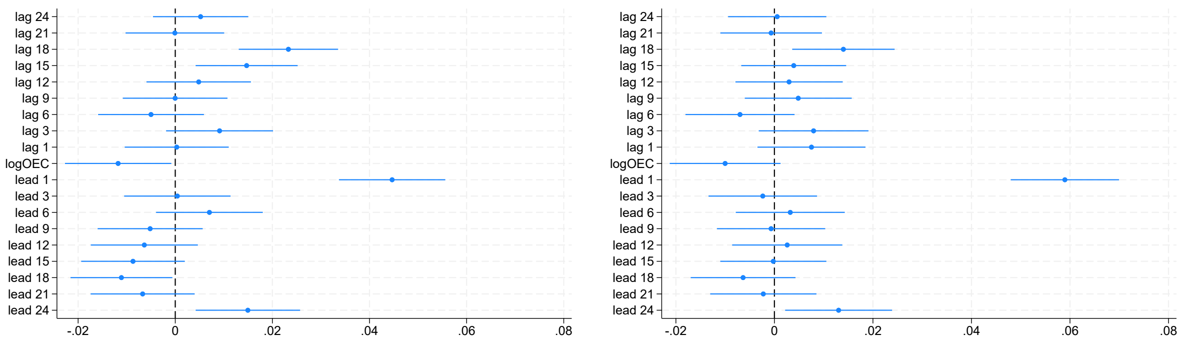
Notes: Baseline sample restrictions and empirical specification are described in Figure 4 notes. Panel (a) excludes observations from August. Panel (b) excludes observations from September. Panel (c) excludes observations from December. Panel (d) excludes observations from January.

C.7 Robustness: Reason of dismissal

Figure C.7.1: First Stage: Provincial Instrument for Exogenous Termination of Fixed-Term Contracts

(a) Ending fixed-term contract

(b) Ending fixed-term and within firm upgrades



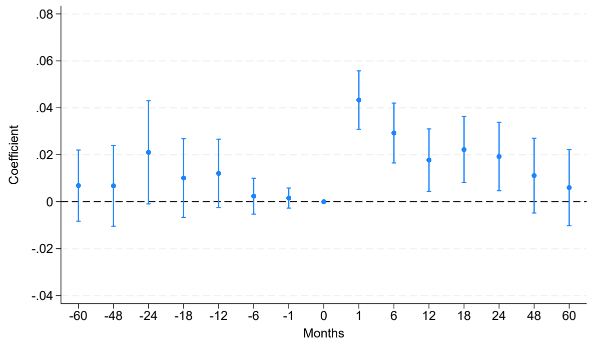
Notes: The baseline sample restrictions and empirical specification are detailed in the notes for Figure 4. Panel (a) includes workers dismissed due to the expiration of their contracts. Panel (b) expands the sample to include individuals promoted to open-ended contracts within the same firm.

C.8 Robustness: Alternative Reduced-Form Specifications

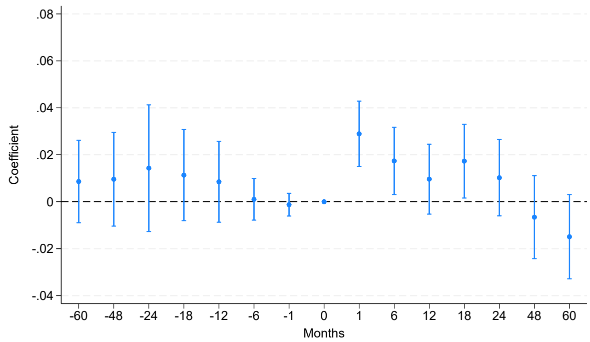
Figure C.8.1: Effect of OEC regional shock on earnings growth

Alternative Specifications

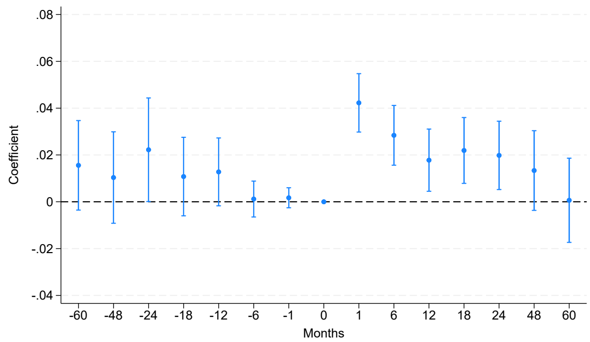
(a) Baseline



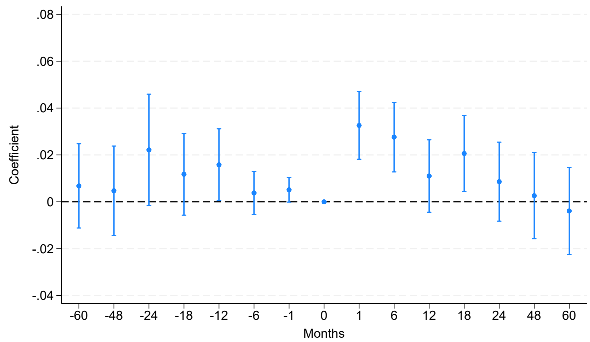
(b) Date FE



(c) Aggregate average earnings growth control



(d) Total new contracts control

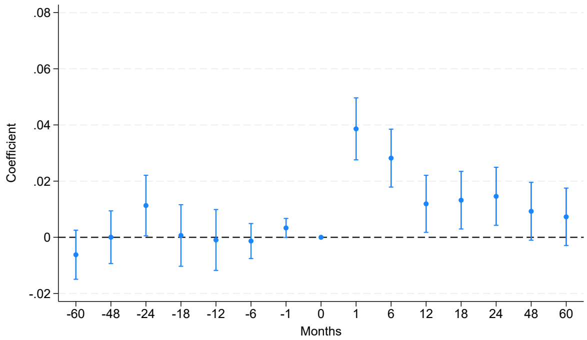


Notes: Impact of the first lead of the *log* of new open-ended in the province on individual earnings growth. Baseline sample restrictions and empirical specification are described in Figure 5 notes. Panel (a) presents the baseline specification from Figure 5 based on equation **??**. Panel (b) adds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

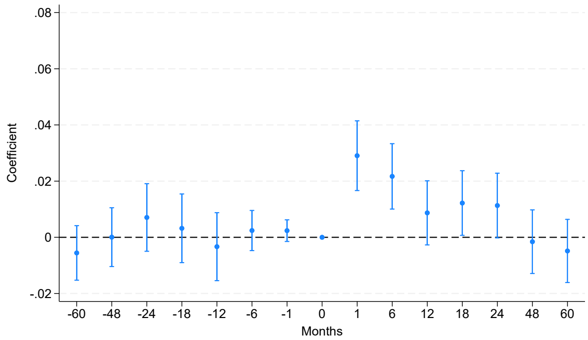
Figure C.8.2: Effect of OEC regional shock on employment

Alternative Specifications

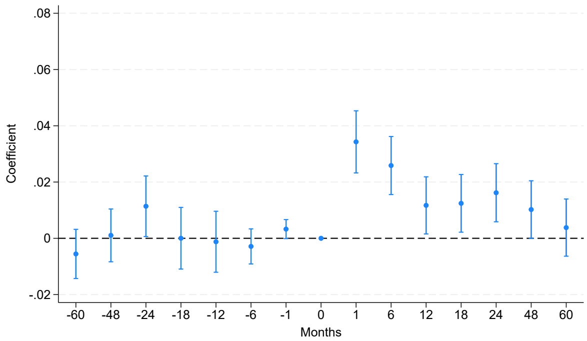
(a) Baseline



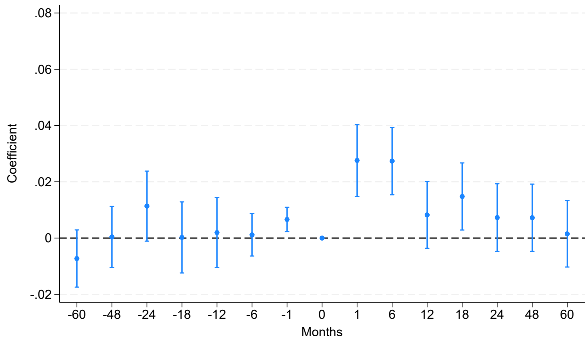
(b) Date FE



(c) Aggregate employment control



(d) Total new contracts control

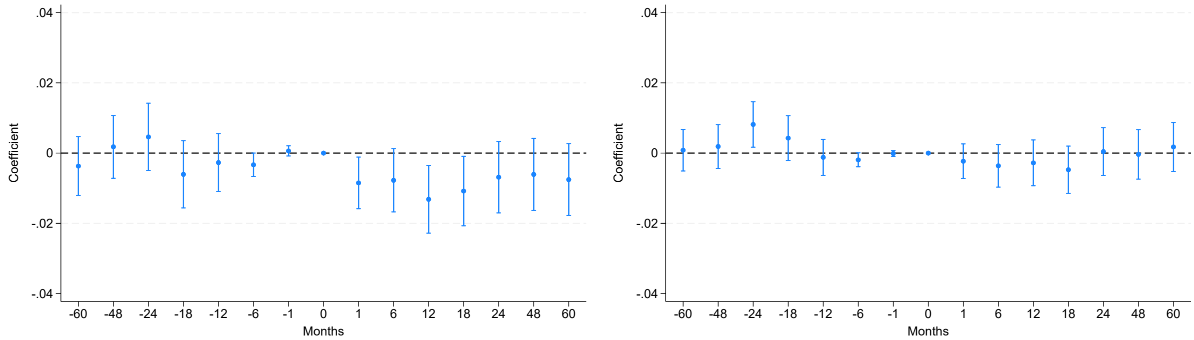


Notes: Impact of the first lead of the *log* of new open-ended in the province on individual employment probability. Baseline sample restrictions and empirical specification are described in Figure 5 notes. Panel (a) presents the baseline specification from Figure 5 based on equation **??**. Panel (b)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

C.9 Robustness: Additional outcomes

Figure C.9.1: Effect of new open-ended contracts in the region on workers' mobility (Compared to baseline sector/province)

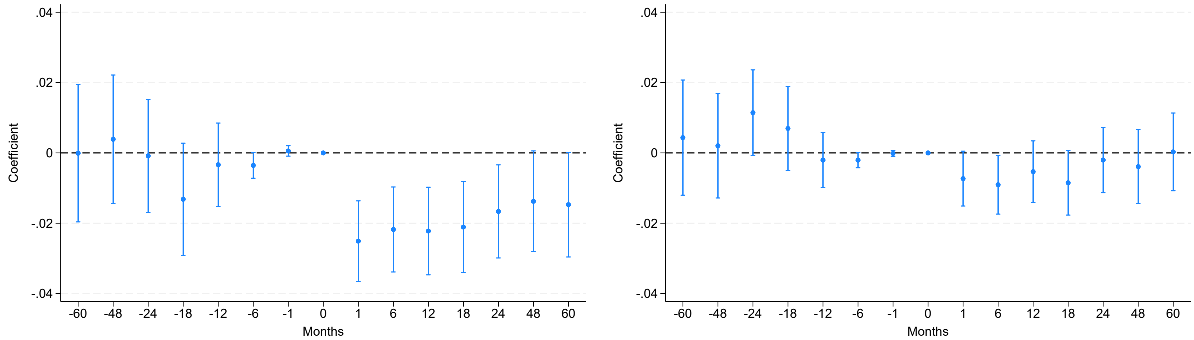
(a) Probability to change sector (b) Probability to change region



Notes: Baseline sample restrictions and empirical specification are described in Figure 5 notes. The outcome variable equals one if a worker is employed in a different sector or province at time $t + h$ compared to their baseline status in period t. Missing values are coded as zero.

Figure C.9.2: Effect of new open-ended contracts in the region on workers' mobility (Employed sample)

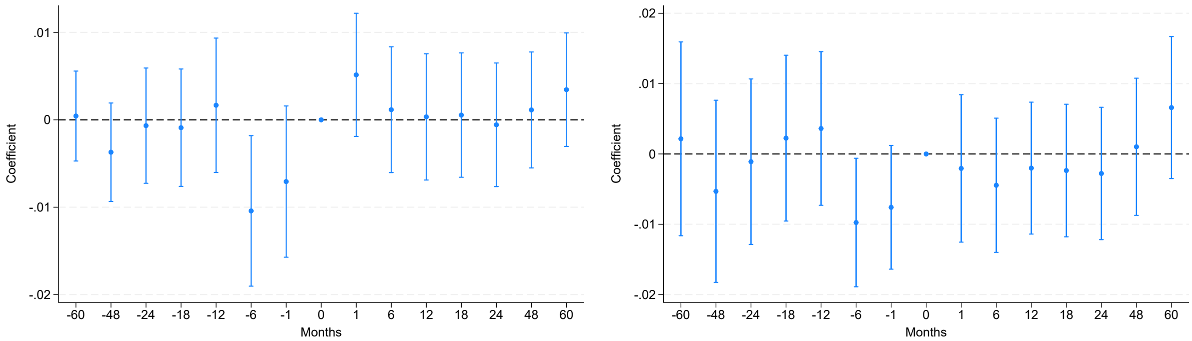
(a) Probability to change sector (b) Probability to change region



Notes: Baseline sample restrictions and empirical specification are described in Figure 5 notes. For each period, we restrict the estimation sample to workers who are employed in that period (unbalanced sample).

Figure C.9.3: Effect of new open-ended contracts in the region on workers' part-time work

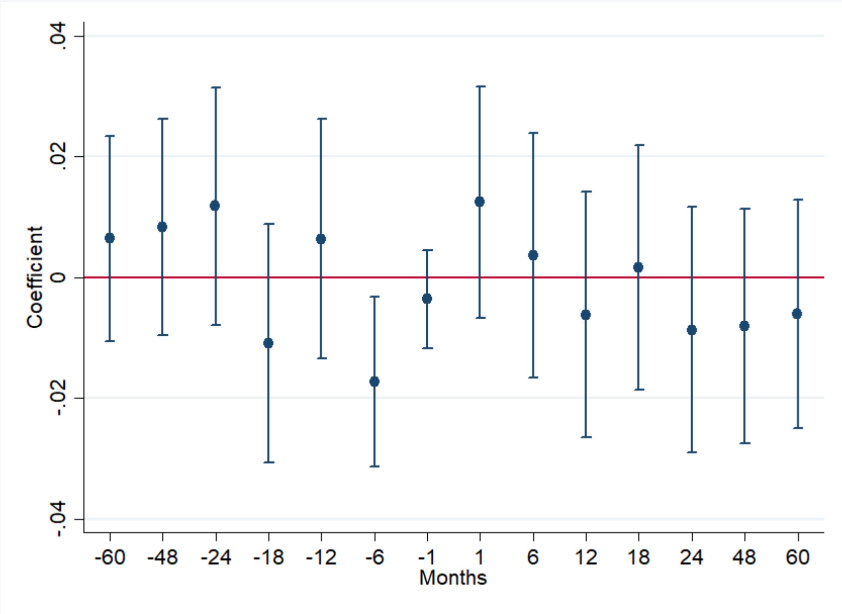
(a) Part-time status (b) Part-time status (employed)



Notes: Baseline sample restrictions and empirical specification are described in Figure 5 notes. Panel (a) shows the effect of our instrument on part-time status on each period. In Panel (b), we restrict the estimation sample to workers employed for each period.

C.10 Robustness: Exclusion Restriction

Figure C.10.1: Effect of OEC regional shock on employment: restriction to fixed-term employment

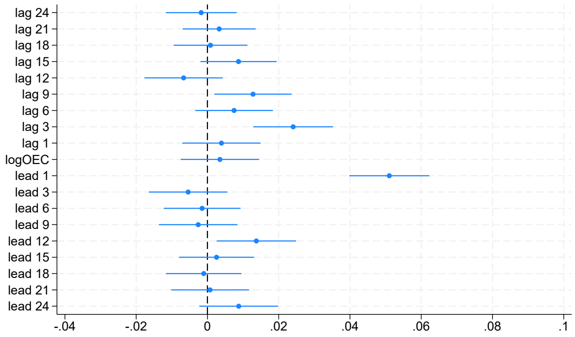


Notes: The sample consists of workers who were in the last month of a fixed-term contract in event period $h = 0$, with at least 0.8 but less than 1.2 years of tenure. We restrict the sample to those workers who *do not* start an open-ended position in period $t+h$. The coefficients correspond to the effect of the first lead of the *log* of new open-ended contracts in the province 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.

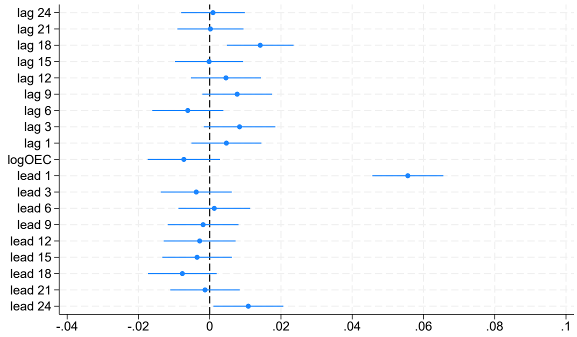
C.11 Alternative Tenure Restrictions

Figure C.11.1: First stage: Provincial Instrument. Alternative tenure restrictions

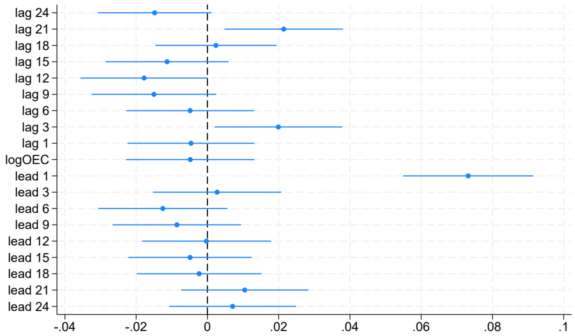
(a) Tenure 0.6-0.8



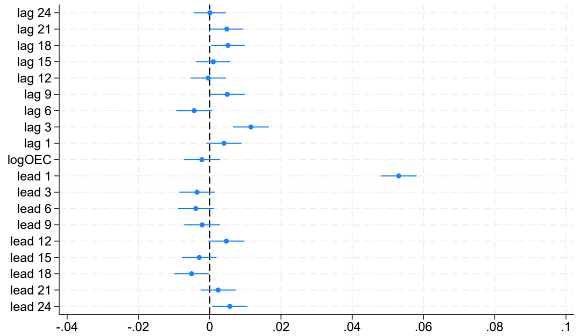
(b) Tenure 0.8-1.2



(c) Tenure 1.6-2



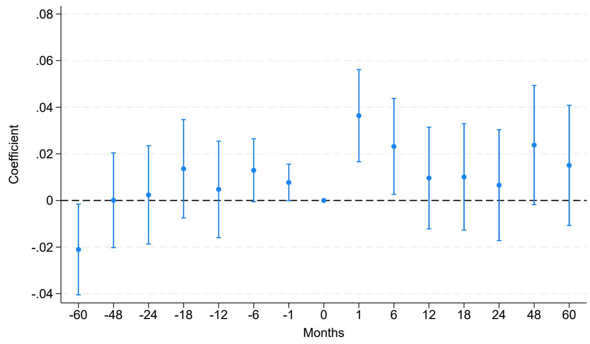
(d) Tenure 0.4-2



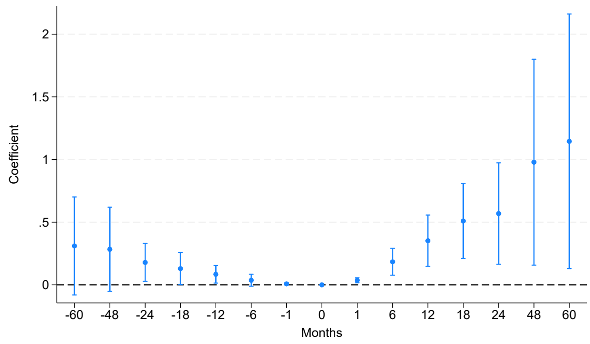
Notes: Notes: The baseline sample restrictions and empirical specification are detailed in the notes for Figure 4. Panel (a) restricts the sample to workers with tenure ranging from 0.6 to 0.8 years in the expiring fixed-term contract. Panels (b)-(d) restrict the sample to workers with tenure from 0.8-1.2, 1.6-2, and 0.4-2 years, respectively.

Figure C.11.2: Effect of OEC regional shock on earnings. Tenure 0.6-0.8 years

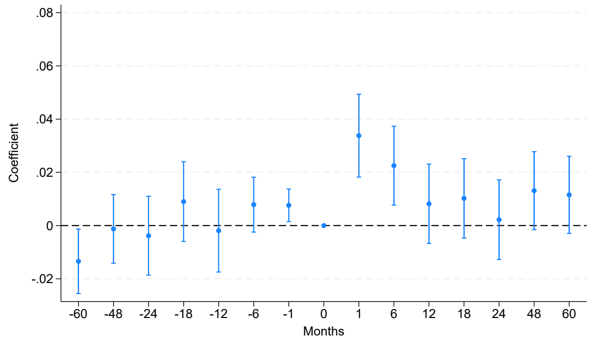
(a) Earnings



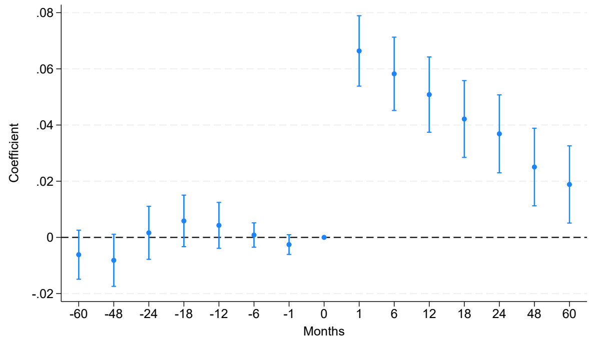
(b) Cumulative Earnings



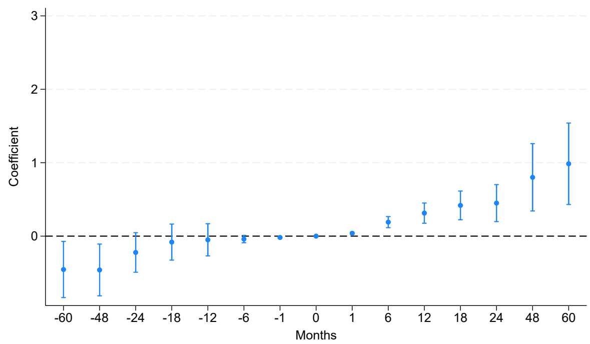
(c) Employment



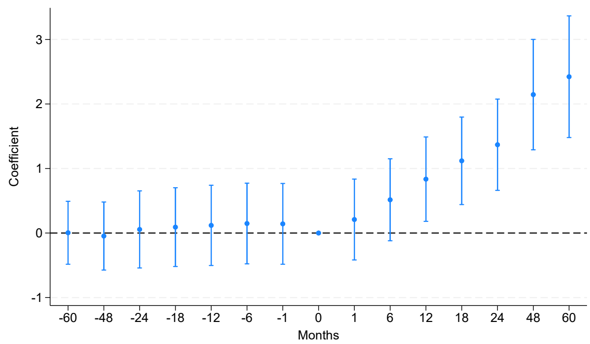
(d) OEC Status



(e) Experience



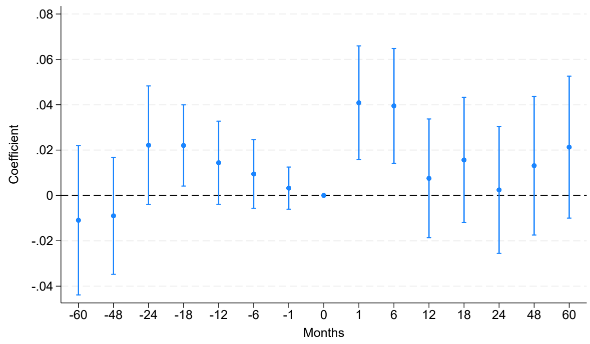
(f) OEC Experience



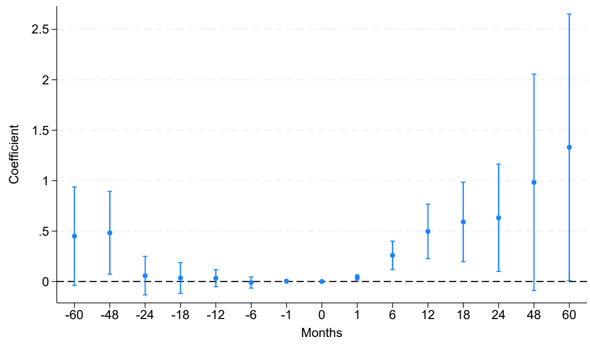
Notes: The sample consists of workers in the last month of a fixed-term position in event period $h = 0$, with at least 0.6 but less than 0.8 years of tenure. Period 1998-2017. The coefficients correspond to the effect of the first lead of the *log* number of new permanent contracts ($logOEC$) on each outcome. All regressions control for the leads and lags of $logOEC$ and 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.

Figure C.11.3: Effect of OEC regional shock on earnings. Tenure 1.6-2 years

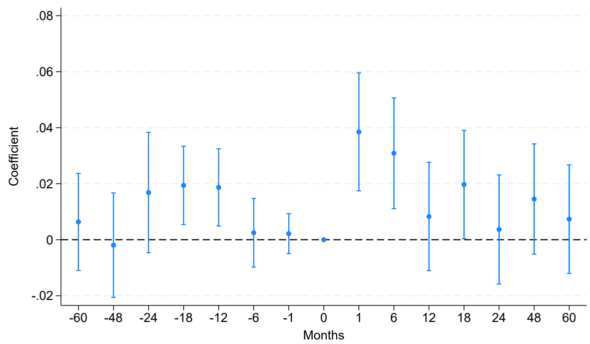
(a) Earnings



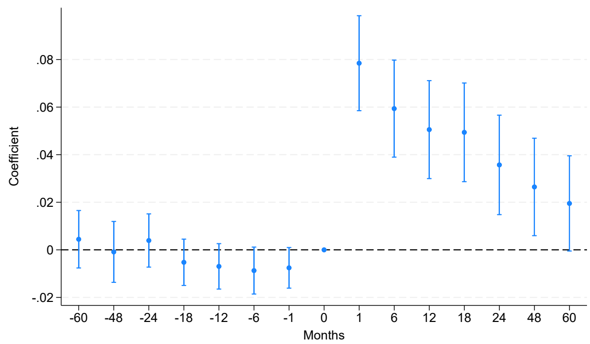
(b) Cumulative Earnings



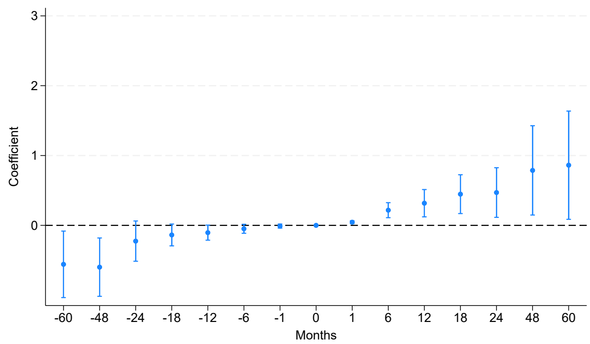
(c) Employment



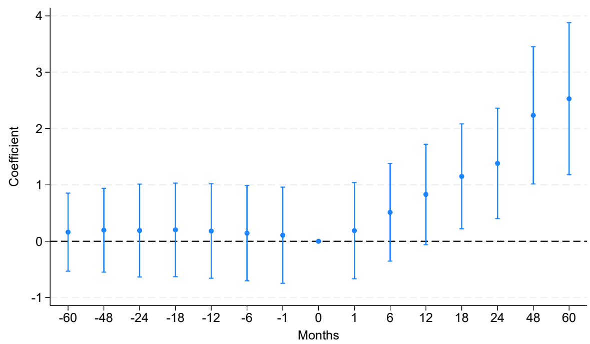
(d) OEC Status



(e) Experience



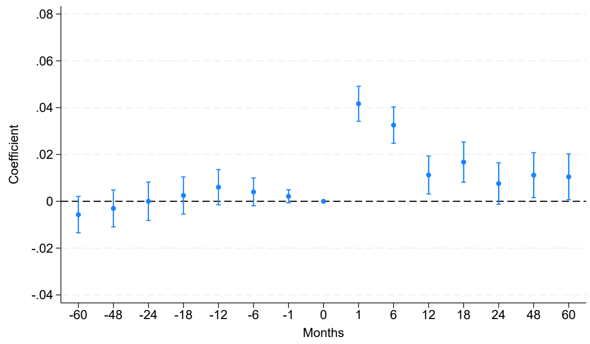
(f) OEC Experience



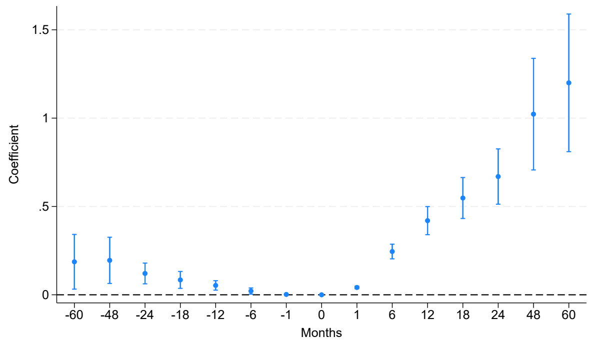
Notes: The sample consists of workers in the last month of a fixed-term position in event period $h = 0$, with at least 0.6 but less than 0.8 years of tenure. Period 1998-2017. The coefficients correspond to the effect of the first lead of the *log* number of new permanent contracts ($logOEC$) on each outcome. All regressions control for the leads and lags of $logOEC$ and 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.

Figure C.11.4: Effect of OEC regional shock on earnings. Tenure 0.4-2 years

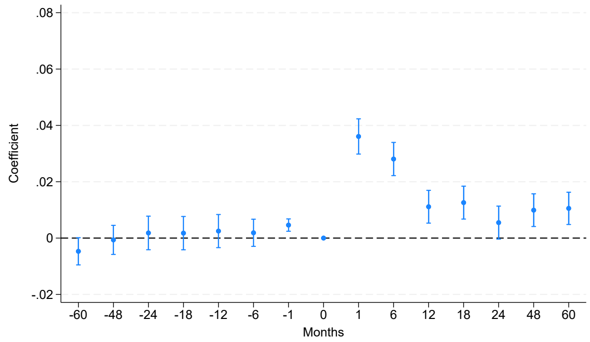
(a) Earnings



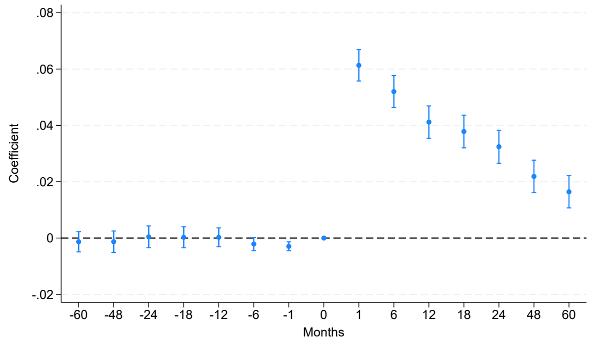
(b) Cumulative Earnings



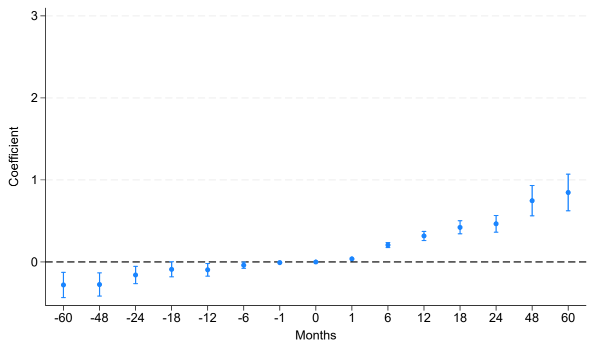
(c) Employment



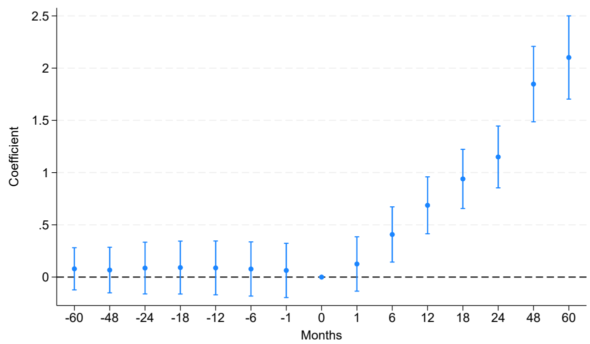
(d) OEC Status



(e) Experience



(f) OEC Experience



Notes: The sample consists of workers in the last month of a fixed-term position in event period $h = 0$, with at least 0.6 but less than 0.8 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 $logOEC$ and 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.

D Supplementary Tables

D.1 Descriptive Statistics

Table D.1.1: Descriptive statistics of the 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
Instrument ($logOEC_{-i,r,t+1}$)	4.80	1.35
Net Instrument ($logOEC_{-i,r,t+1}$)	0	0.19
Obs.	219,704	

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. The net instrument is the mean and standard deviation of the first lead of log new open-ended contracts net of all controls included in the first stage.

Table D.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.

D.2 IV Estimates: Additional Results

Table D.2.1: Effect of Permanent Contracts on Worker Careers (OLS)

	Panel A: Short-term effects (12 months)					
	Earnings	Cum. Earnings	Employment	Experience	Change Region	Change Sector
	(1)	(2)	(3)	(4)	(5)	(6)
$p_{i,t+1}$	0.258***	5.057***	0.253***	4.521***	-0.108***	-0.371***
	(0.002)	(0.023)	(0.002)	(0.016)	(0.001)	(0.002)
Obs.	197,302	197,302	197,302	197,008	197,302	197,302
R2	0.103	0.245	0.130	0.997	0.065	0.176
Mean Dep	-0.184	8.538	0.743	85.017	0.091	0.289
	Panel B: Long-term effects (60 months)					
	Earnings	Cum. Earnings	Employment	Experience	Change Region	Change Sector
	(1)	(2)	(3)	(4)	(5)	(6)
$p_{i,t+1}$	0.100***	15.626***	0.088***	11.210***	-0.123***	-0.353***
	(0.003)	(0.129)	(0.002)	(0.076)	(0.002)	(0.002)
Obs.	197,302	197,302	197,302	190,849	197,302	197,302
R2	0.227	0.206	0.259	0.944	0.066	0.140
Mean Dep	-0.249	42.108	0.590	119.416	0.169	0.491

Notes: The table reports OLS estimated coefficients based on equation 4. 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. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

D.3 Heterogeneity IV

Table D.3.1: Heterogeneity of the Treatment Effects: Firm characteristics (12 months)

	(1) First Starge	(2) Earnings	(3) Cum. Earnings	(4) Employment	(5) Experience	(6) Change Sector	(7) Change Region
Firm's age							
Young Firm	0.068***	0.405***	7.938***	0.375***	6.515***	-0.254**	-0.071
Obs. 98,664	(0.008)	(0.140)	(1.397)	(0.105)	(0.965)	(0.109)	(0.079)
Old Firm	0.047***	0.150	8.393***	-0.007	5.907***	-0.345**	-0.034
Obs. 99,068	(0.007)	(0.197)	(1.893)	(0.153)	(1.298)	(0.143)	(0.110)
Firm's size							
Small Firm	0.042***	0.413*	9.192***	0.250	7.180***	-0.142	0.044
Obs. 95,979	(0.007)	(0.227)	(2.256)	(0.166)	(1.519)	(0.164)	(0.123)
Large Firm	0.077***	0.229*	7.561***	0.169*	5.537***	-0.375***	-0.131*
Obs. 101,753	(0.008)	(0.120)	(1.165)	(0.092)	(0.827)	(0.095)	(0.071)
Share of FTC at initial sector							
Low FTC	0.061***	0.393***	9.814***	0.380***	7.489***	-0.306***	-0.030
Obs. 103,298	(0.008)	(0.143)	(1.488)	(0.110)	(1.034)	(0.110)	(0.078)
High FTC	0.055***	0.191	6.185***	0.004	4.802***	-0.263*	-0.086
Obs. 94,434	(0.007)	(0.185)	(1.749)	(0.139)	(1.200)	(0.136)	(0.105)

Notes: This table reports the first-stage and IV estimates of the effect of upgrading to an open-ended contract on worker characteristics. Column (1) presents the first-stage coefficients, while Columns (2)–(6) display the corresponding IV estimates for earnings and employment outcomes. Robust standard errors clustered at the individual level are reported in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Table D.3.2: Heterogeneity of the Treatment Effects: Firm characteristics (60 months)

	(1) First Stage	(2) Earnings	(3) Cum. Earnings	(4) Employment	(5) Experience	(6) Change Sector	(7) Change Region
Firm's age							
Young firm	0.068***	0.358**	29.096***	0.305***	18.263***	-0.339***	-0.047
Obs. 98,664	(0.008)	(0.181)	(7.294)	(0.113)	(4.069)	(0.119)	(0.100)
Old Firm	0.047***	-0.424*	15.571*	-0.142	6.828	-0.276*	0.017
Obs. 99,068	(0.007)	(0.255)	(9.422)	(0.155)	(5.525)	(0.162)	(0.138)
Firm's size							
Small Firm	0.042***	-0.140	17.830	0.023	12.406**	0.115	0.162
Obs. 95,979	(0.007)	(0.285)	(11.052)	(0.175)	(6.228)	(0.198)	(0.159)
Large Firm	0.077***	0.113	26.586***	0.144	12.510***	-0.554***	-0.157*
Obs. 101,753	(0.008)	(0.149)	(5.994)	(0.093)	(3.479)	(0.104)	(0.089)
Share of FTC at initial sector							
Low FTC	0.061***	-0.022	24.178***	0.174	16.774***	-0.334***	-0.022
Obs. 103,298	(0.008)	(0.187)	(7.492)	(0.117)	(4.298)	(0.126)	(0.103)
High FTC	0.055***	0.055	22.249**	0.021	8.892*	-0.277*	-0.020
Obs. 94,434	(0.007)	(0.224)	(8.757)	(0.138)	(5.028)	(0.148)	(0.128)

Notes: This table reports the first-stage and IV estimates of the effect of upgrading to an open-ended contract on worker characteristics. Column (1) presents the first-stage coefficients, while Columns (2)–(6) display the corresponding IV estimates for earnings and employment outcomes. Robust standard errors clustered at the individual level are reported in parentheses. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Table D.3.3: Heterogeneity of the Treatment Effects: Worker characteristics (12 months)

	(1) First Stage	(2) Earnings	(3) Cum. Earnings	(4) Employment	(5) Experience	(6) Change Sector	(7) Change Region
Education							
Below Secondary	0.050***	0.356**	8.955***	0.241*	7.022***	-0.153	-0.124
Obs. 111,713	(0.007)	(0.169)	(1.646)	(0.131)	(1.172)	(0.129)	(0.093)
At least Secondary	0.054***	0.244	7.368***	0.181*	5.574***	-0.424***	0.020
Obs. 47,253	(0.012)	(0.151)	(1.494)	(0.106)	(0.985)	(0.112)	(0.088)
Gender							
Male	0.059***	0.347**	8.148***	0.191*	5.925***	-0.375***	-0.056
Obs. 113,541	(0.007)	(0.140)	(1.423)	(0.106)	(0.974)	(0.110)	(0.088)
Female	0.055***	0.223	8.216***	0.218	6.673***	-0.107	-0.037
Obs. 84,191	(0.009)	(0.197)	(1.826)	(0.144)	(1.258)	(0.143)	(0.090)
Age							
Age < 30	0.065***	0.306**	8.999***	0.258**	7.120***	-0.260**	-0.077
Obs. 109,239	(0.007)	(0.143)	(1.443)	(0.103)	(0.967)	(0.106)	(0.079)
Age > 30	0.050***	0.298	6.679***	0.124	4.801***	-0.293**	0.001
Obs. 88,493	(0.008)	(0.190)	(1.780)	(0.151)	(1.302)	(0.145)	(0.109)
Earnings at baseline							
Low Earnings	0.041***	0.391	9.288***	0.307*	7.630***	-0.137	0.112
Obs. 94,628	(0.008)	(0.262)	(2.523)	(0.175)	(1.595)	(0.175)	(0.123)
High Earnings	0.078***	0.246**	7.482***	0.139	5.337***	-0.373***	-0.163**
Obs. 103,104	(0.008)	(0.097)	(1.006)	(0.088)	(0.802)	(0.091)	(0.074)

Notes: This table reports the first-stage and IV estimates of the effect of upgrading to an open-ended contract on worker characteristics. Column (1) presents the first-stage coefficients, while Columns (2)–(6) display the corresponding IV estimates for earnings and employment outcomes. Robust standard errors clustered at the individual level are reported in parentheses. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Table D.3.4: Heterogeneity of the Treatment Effects: Worker characteristics (60 months)

	(1) First Stage	(2) Earnings	(3) Cum. Earnings	(4) Employment	(5) Experience	(6) Change Sector	(7) Change Region
Education							
Below Secondary	0.050***	0.356**	8.955***	0.241*	7.022***	-0.153	-0.124
Obs. 111,713	(0.007)	(0.169)	(1.646)	(0.131)	(1.172)	(0.129)	(0.093)
At least Secondary	0.054***	0.244	7.368***	0.181*	5.574***	-0.424***	0.020
Obs. 47,253	(0.012)	(0.151)	(1.494)	(0.106)	(0.985)	(0.112)	(0.088)
Gender							
Male	0.059***	0.347**	8.148***	0.191*	5.925***	-0.375***	-0.056
Obs. 113,541	(0.007)	(0.140)	(1.423)	(0.106)	(0.974)	(0.110)	(0.088)
Female	0.055***	0.223	8.216***	0.218	6.673***	-0.107	-0.037
Obs. 84,191	(0.009)	(0.197)	(1.826)	(0.144)	(1.258)	(0.143)	(0.090)
Age							
Age < 30	0.065***	0.306**	8.999***	0.258**	7.120***	-0.260**	-0.077
Obs. 109,239	(0.007)	(0.143)	(1.443)	(0.103)	(0.967)	(0.106)	(0.079)
Age > 30	0.050***	0.298	6.679***	0.124	4.801***	-0.293**	0.001
Obs. 88,493	(0.008)	(0.190)	(1.780)	(0.151)	(1.302)	(0.145)	(0.109)
Earnings at baseline							
Low Earnings	0.041***	0.391	9.288***	0.307*	7.630***	-0.137	0.112
Obs. 94,628	(0.008)	(0.262)	(2.523)	(0.175)	(1.595)	(0.175)	(0.123)
High Earnings	0.078***	0.246**	7.482***	0.139	5.337***	-0.373***	-0.163**
Obs. 103,104	(0.008)	(0.097)	(1.006)	(0.088)	(0.802)	(0.091)	(0.074)

Notes: This table reports the first-stage and IV estimates of the effect of upgrading to an open-ended contract on worker characteristics. Column (1) presents the first-stage coefficients, while Columns (2)–(6) display the corresponding IV estimates for earnings and employment outcomes. Robust standard errors clustered at the individual level are reported in parentheses. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table D.4.1: 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. $^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$.

D.4 Social Security records