Quasi-Random Matches: Evidence from Dual Labor Markets *

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Abstract

A fast-growing literature studies how sorting into particular jobs, firms, or locations affects workers. The key challenge when studying such questions is the non-random sorting of workers into jobs. We propose a novel identification strategy that exploits the *timing* of worker-firm matching. We isolate quasi-random variation in matches by interacting high-frequency information on the duration of contracts on the labor supply and transitory fluctuations in job creation on the labor demand side. We apply this method to address a central question in *dual labor markets*: how do different contract types – fixed-term or open-ended contracts – affect workers' careers? We find that transitory variation in the opening of permanent contracts is highly predictive of individual promotion probabilities and has long-lasting effects on earnings, employment, and the accumulation of experience in permanent positions.

JEL Codes: J29, J31, J41, J60

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1 Introduction

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

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

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

Our application focuses on Spain. With the highest rate of temporary employment in Europe of nearly 25% (See Figure A.2.10) and as much as 90% of new contracts being fixed-term (until a major reform in 2022), the country provides an interesting context. Moreover, we can exploit rich, matched employer-employee data from Social Security

[1]In addition other relevant outcomes may be affected by labor market duality, such as: fertility (Auer and Danzer 2016; Lopes 2020; Nieto 2022); 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. (2021), we find that earnings growth is higher for workers with more experience in open-ended contracts: while earnings grow by 2.7 percent for each year of experience in FTs, they grow by 3.6 percent per year in OECs. These patterns are highly non-linear, and the gap is much greater for experienced than young, inexperienced workers. An intuitive interpretation of these findings is that fixed-term contracts slow skill acquisition and wage growth (i.e., differences in returns to experience). However, they could also be due to workers who secured an OEC early in their career experiencing higher wage growth *irrespectively* of current contract type (i.e., selection).

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

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

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

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

Conceptually, our strategy is similar to studies that analyze the effects of labor market conditions at the entry on worker careers – "graduating in a recession" – (Oreopoulos et al. 2012; Kahn 2010), in particular, recent work by Arellano-Bover (2020) on the selection of workers into different firm types. However, rather than exploiting yearly variation in 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-out) sum of new open-ended contracts is highly predictive for a worker to switch from a fixed-term into an open-ended contract. We then provide evidence to support the instrument independence assumption and exclusion restriction. Instrument independence would imply that facing more open-ended job openings (relative to trend) in the month a contract ends is as-good-as random for the worker. To support this assumption, we show that our instrument is indeed broadly uncorrelated with worker characteristics. However, the exclusion restriction is unlikely to hold without further adjustments. The number of new open-ended contracts (our instrument) does, of course, correlate with general business cycle conditions, so it is not obvious whether a worker enjoys higher wage growth because she started in an open-ended contract or because the economic conditions in this period were generally favorable, affecting wage growth conditional on the contract type. The objective, therefore, becomes to control for general economic trends while exploiting the exact timing of when an individual switched jobs, i.e., we exploit high-frequency variation in the types of contracts that are 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 for the calculation of taxes and social security contributions. Our approach, therefore, exploits a comparative advantage of administrative data (their high frequency), similarly as the fixed effects approach exploits another (their scale).

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

The paper is organized as follows: Section 2 provides a background of the institutional framework, Section 3 introduces the main data source, Section 4 provides a characterization of dualism in Spain and preliminary results of a mincerian approach, Sections 5 and 6 discuss the main sources of endogeneity and our identification strategy, respectively and Section 7 analyses the effect of upgrade promotion in workers' career trajectory by evaluating a series of labor market outcomes.

2 Institutional framework

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

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

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

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

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

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

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

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

3 Data

Our main data source combines the 2006-2017 waves of the Continuous Sample of Working Lives (*Muestra Continua de Vidas Laborales* or MCVL). The microdata from the MCVL constitutes a 4% non-stratified random sample of Spain's Social Security administrative records. The sample allows tracking the full working history of individuals back to 1967 and the monthly earnings since 1980. Once an individual with an ongoing relationship with Social Security is included in the sample, it remains in all future waves.[4] Furthermore, every year, those individuals that 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.

Several features make this rich dataset optimal for our analysis. A key advantage of the MCVL is its high-frequency records, reporting the exact start and end dates of each contract. This enables us to measure the labor market conditions that workers face at a very detailed (in our baseline analysis, monthly) level. Since we have information on each spell's entry and exit date, we are able to compute the exact days an employee worked. Whenever there is an overlap of spells, we preserve the job characteristics of the main job: i.e., the largest spell of the month. We are then able to build a reliable measure of tenure and work experience with a clear distinction between the experience accumulated in fixed-term and open-ended contracts.

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

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

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

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

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

3.1 Sample restrictions

Our study evaluates the 1998-2017 period. Although we can trace each worker's earnings trajectory back to 1980's, information on the type of contract is reliable only from 1998 onwards. We focus on workers aged 18-49. We restrict the analysis to workers registered in the general social security regime or the special regime for agrarian, sea workers, and mining. This excludes autonomous workers. Since they are not employees and therefore do not hold a contract, they are not part of our study.

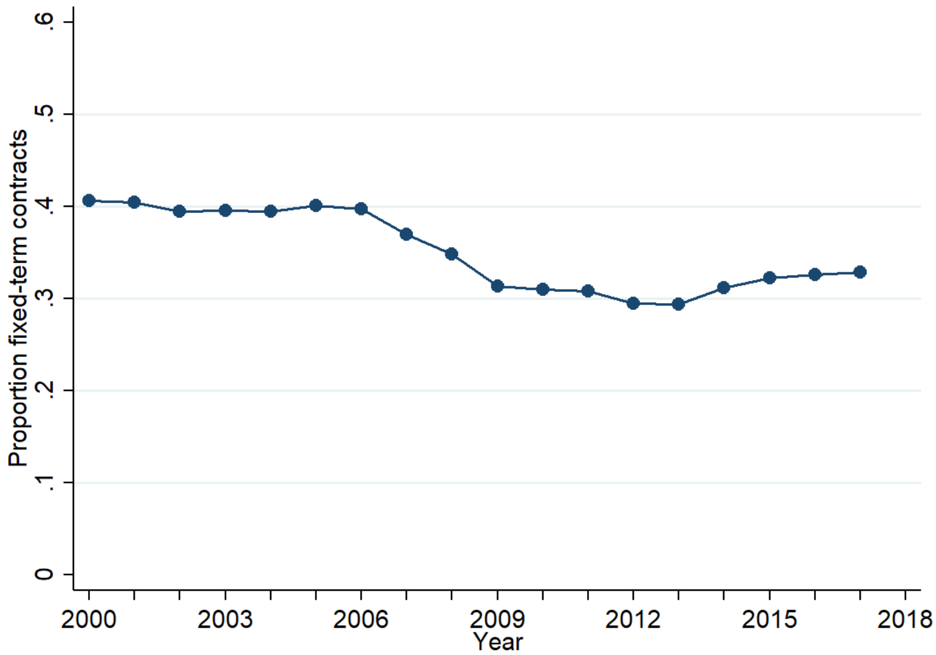
In our main specification, we only consider private sector workers, as the contract duration of public sector employees is highly regulated and centralized, as well as the promotion to permanent positions relies on a special process.[7] However, whenever this is the case, our measure of experience does take into account the time that a private employee previously worked in the public sector, either in a fixed or a permanent contract. Regionally, we exclude information from Ceuta and Melilla, for which the sample of workers is very small. Thus, we work with data from 50 provinces.

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

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

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

Figure 1: Proportion of workers in fixed-term contracts, by year



Notes: Proportion of workers under a fixed-term from 2000 to 2017.

4 Descriptive evidence

One-third of all Spanish employees are employed on a fixed-term basis, on average, over the last few decades. Despite a decline in the share of temporary workers in the aftermath of the Great Recession (Figure 1), their share is still very high compared to most European countries.[8] The reduction in the proportion of fixed-term contracts reflects, to a great extent, the decrease in hiring after the financial crisis. The construction sector, which concentrated a large share of temporary workers, was one of the hardest hit. Likewise, young workers' unemployment increased dramatically and remained high for many years, spiking from around 22.3% in 2004 to 44.5% in 2016. This situation also affected the age distribution of temporary workers. As shown in Table 1, the share of fixed-term contract workers under 24 years almost halved, from 20.7% in 2004 to 11.2% in 2016.

As discussed previously, the high dualism in the Spanish labor market implies that rather than working as stepping-stones, a large proportion of fixed-term contracts are dead-ends. While this problem is more severe for low-skilled occupations, it cannot be neglected at the top of the distribution. As shown in Table 1, the share of high-skilled occupations among temporary contracts has steadily increased. In terms of other workers and job characteristics, these contracts are equally spread among women and men. While most of these contracts correspond to full-time positions, the proportion of part-time jobs

[8]In 2019, more than 25% of Spanish workers were on a temporary contract. See Figure A.2.10.

under this modality has increased substantially, representing almost one-third of these jobs by 2016.

Table 1: Characteristics of workers in fixed-term contracts

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

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

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

$$\ln w_{irt} = exp_{it}^{FT}(\beta_1 + exp_{it}\beta_2) + exp_{it}^{OEC}(\beta_3 + exp_{it}\beta_4) + X_{it}'\mathbf{\Omega} + \sigma_r + \psi_t + \varepsilon_{irt}, \tag{1}$$

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

Instead of the typical quadratic form of homogeneous returns to experience, equation (1) 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.

Table 2: Wage growth in fixed-term and open-ended contracts

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

Standard errors in parentheses

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

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

The estimates are shown in Table 2. Disregarding the distinction between fixed-term and open-ended contracts, column (1) shows that one extra year of experience is associated with an increase in individual earnings of 2.5% 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 irrespectively 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 the same. 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.[9]

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

5 Selection into permanent positions

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

[9] Based on these results, Figure A.2.7 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%.

sorting.

We start examining whether workers with open-ended and fixed-term contracts follow parallel earnings paths before they are promoted using an event-study design. For each worker in the data, we denote the precise month in which the individual ends a temporary contract by $t = 0$, and index future and past months relative to that moment. We use the last complete month in the old contract ($t = -1$) as our base period. After the contract ends, we categorize workers based on their future type of contract, distinguishing workers transitioning from a FT to an open-ended contract (FT→OEC, $T_i = 1$) and workers transitioning to another FT contract (FT→FT, $T_i = 0$). Our baseline specification considers a balanced panel of workers whom we observe fifteen periods (months) before and after the event,[10] so the event time t runs from -15 to $+15$. We denote by y_{ist} the log earnings of individual i, in year-month s and at event time t, and estimate the following regression:

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

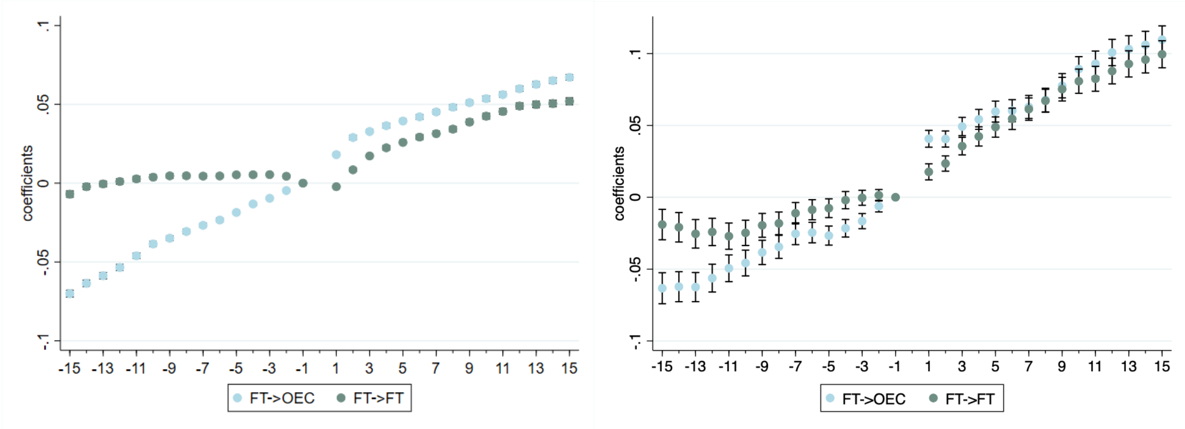
where we include a complete set of event time dummies (first term on the right-hand side), age dummies (second term), and year × month dummies (third term). As we omit the event time dummy at $t = -1$ from the estimation, the event time coefficients measure the impact of moving into a new contract relative to the earnings just before the termination of the previous fixed-term contract. By including a complete set of age dummies, we control non-parametrically for underlying life-cycle trends. We also control non-parametrically for time trends such as business cycle variation, including a full set of time dummies. Including age dummies in the comparison is important because workers in open-ended positions tend to be older than workers who remain in temporary positions.

Results are presented in Figure 2. Panel (a) controls for the full set of time and age dummies discussed above. Additionally, Panel (b) also accounts for interactions between event time and worker's education and sector, accounting for earnings growth explained by differences in observable characteristics. The estimates remain unchanged if we include worker fixed effects, as we consider a balanced sample of workers and the estimates represent the earnings growth of those workers compared to the base period (i.e., worker fixed effects are netted out already).

We would expect that workers face a differential earnings path after event period 0, as temporary contracts may be subject either to earnings penalties or premia (Albanese

[10]Periods may differ from months if workers have a non-employment spell within those fifteen months before or after. Due to sample restrictions, this is ruled out for the pre-period.

Figure 2: Earnings consequences from transitioning to OEC or FTC



(a) No controls

(b) Additional controls

Notes: The base category is t=-1. Panel (a) Controls for the full set of time and age dummies. Panel (b) includes additional interactions of event time with education and sector FE. Errors are clustered at the worker level. The coefficient from event period 0 is omitted from each graph, given that not all workers worked the whole last month.

and Gallo 2020; Kahn 2016), and because returns to experience depend on contract type. However, we observe that earnings evolve very differently even *before* workers start their new contract: those workers who subsequently switch into open-ended contracts enjoy *much* faster earnings growth than those who do not, even while both groups are still in fixed-term contracts. The finding of higher wage returns among workers with more open-ended work experience, therefore partially reflects this difference in worker selection. In fact, the difference in earnings growth between worker types is much more pronounced before any transitions to open-ended contracts take place.

6 Identification

In order to deal with the endogeneity of promotions into permanent positions, we propose an instrumental variable strategy. As an exogenous source of variation, we combine individual variation in the expiration date of a fixed-term contract and transitory fluctuations in the opening of new open-ended jobs over time and space. Workers face a positive shock if there is an abnormal increase in permanent openings in the labor market just before their contract expires. This affects promotion probabilities in two ways: in the most direct channel, workers face a tighter labor market with more opportunities of landing a permanent job outside their current firm as their availability is higher. Moreover, other workers might switch to a job in a new firm, creating vacancies that could be filled by promoting fixed-term workers whose contract is about to end.

Exploiting the high frequency of our data, we can precisely match the month when the individual contract is about to end with the job openings at the regional level that precise month. We argue that facing more job openings precisely in the month a contract is about to end is as good as random for the worker.

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

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

where p_{it+1} indicates whether the worker is promoted to an open-ended contract in $t+1$, the variable $logOEC_{-i,t+k}$ is constructed as the sum of all new open-ended positions in period t in the worker's initial province of residence, leaving out individual i herself. We, therefore, allow for promotions to depend on the total number of new open-ended contracts in period t and leads and lags of this variable, excluding individual's i promotion in the calculation. The first lead, $logOEC_{-i,t+1}$, is our instrumental variable. As we control for a full set of time fixed effects, it captures regional fluctuations in the supply of new open-ended contracts that are as good as random from the perspective of the worker.[11] The instrument independence assumption is therefore plausible. Under our identification assumptions, we would expect the effect of this first lead, captured by coefficient α_1, to be the strongest predictor of an individual's probability to switch into a permanent position. The coefficients on other leads and lags (α_k for $k \neq 1$) should be smaller in magnitude, but might be non-zero, as they capture general business cycle conditions that might not be fully captured by α_1.

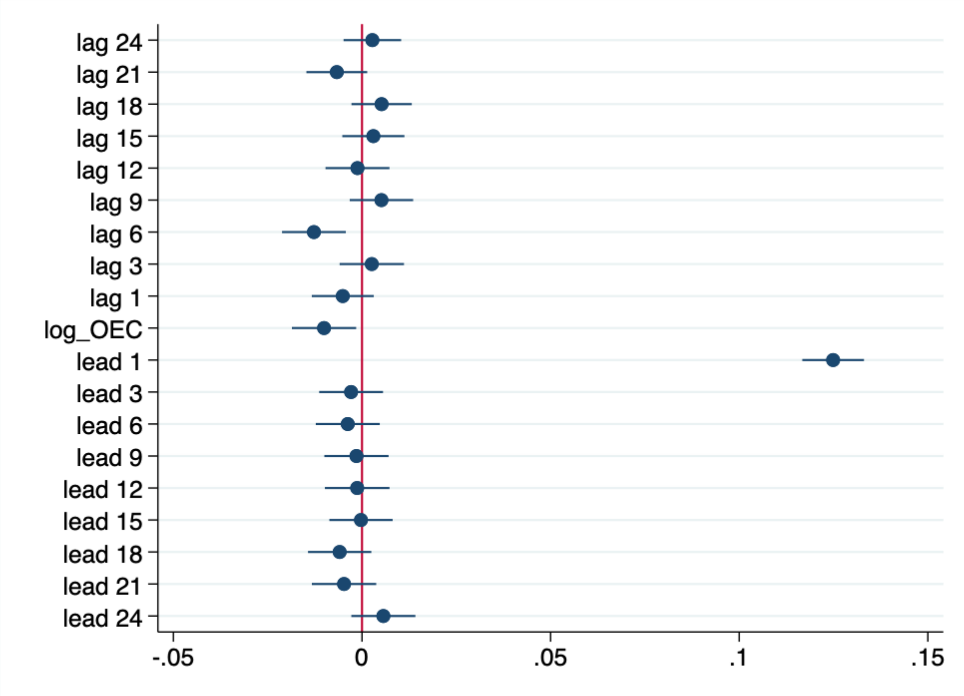
Specifically, the inclusion of leads and lags of the instrument serves two purposes. First, to illustrate that transitory fluctuations matter if they hit a worker in exactly the month in which her previous contract runs out, i.e. to show that the first lag has strong predictive power even conditional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

The results from this regression are presented in Figure 3.[12] As expected, the effect of the first lead of new permanent positions stands-out strongly. Consistent with our

[11] In Figure A.1.1 in the Appendix, we provide evidence that $logOEC_{-i,t+1}$ is uncorrelated with worker's characteristics once we account for time and region fixed effects.

[12] The regression estimates for the baseline and alternative specifications are reported in Tables 7, 8, 9, 10.

Figure 3: The effect of new open-ended contracts on promotion probabilities



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

identification strategy, we find that the openings of new open-ended contracts when the worker's contract expires are the strongest predictor of the probability of finding a permanent position immediately after. Moreover, the absence of strong correlations with the rest of the leads and lags indicates that the instrument is capturing the effect of transitory shocks on job market matches, as opposed to general business cycle conditions.

Figure 3 depicts the leads and lags in the number of new open positions on the *regional* level. We can apply the same logic to exploit instead new openings of permanent positions at the national and industry level, which might be more consequential for an individual's labor market chances. As shown in Figure A.1.4 in the Appendix, we find similar patterns in these alternative specifications.

The instrumental variable identifies 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 type 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.

7 Results: Reduced-form evidence

Labor market dualism may impact workers' trajectories in several dimensions. Previously, we showed that regional variations in the opening of permanent contracts affect promotion probabilities. In a reduced-form approach, this section examines how the improved upgrade to permanent position opportunities affects workers' labor market outcomes in the short and long-term. Restricting the sample to those workers holding contracts that are about to end, we estimate the following equation:

$$y_{it+h} = \sum_{k=-24}^{24} \alpha_k logOEC_{-i,t+k} + \sum_{k=-24}^{24} \gamma_k logTNC_{-i,t+k} + X_{it}\theta + \epsilon_{it}, \tag{4}$$

where y_{it+h} is the worker's i outcome in period $t+h$, with $h = -60, \ldots, 60$. Each outcome is studied up to 60 months before and after fixed-term contract expiration, allowing us to explore the long-term effects of contract type and to verify that workers had similar career trajectories in the pre-treatment period. We include 24 leads and lags of the *log* of new open-ended contracts ($logOEC$) relative to the last month of the worker's current fixed-term contract. In order to control for business cycle variation and job creation seasonality, we also include the same number of leads and lags of the *log* total number of new contracts denoted by ($logTNC$).

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

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

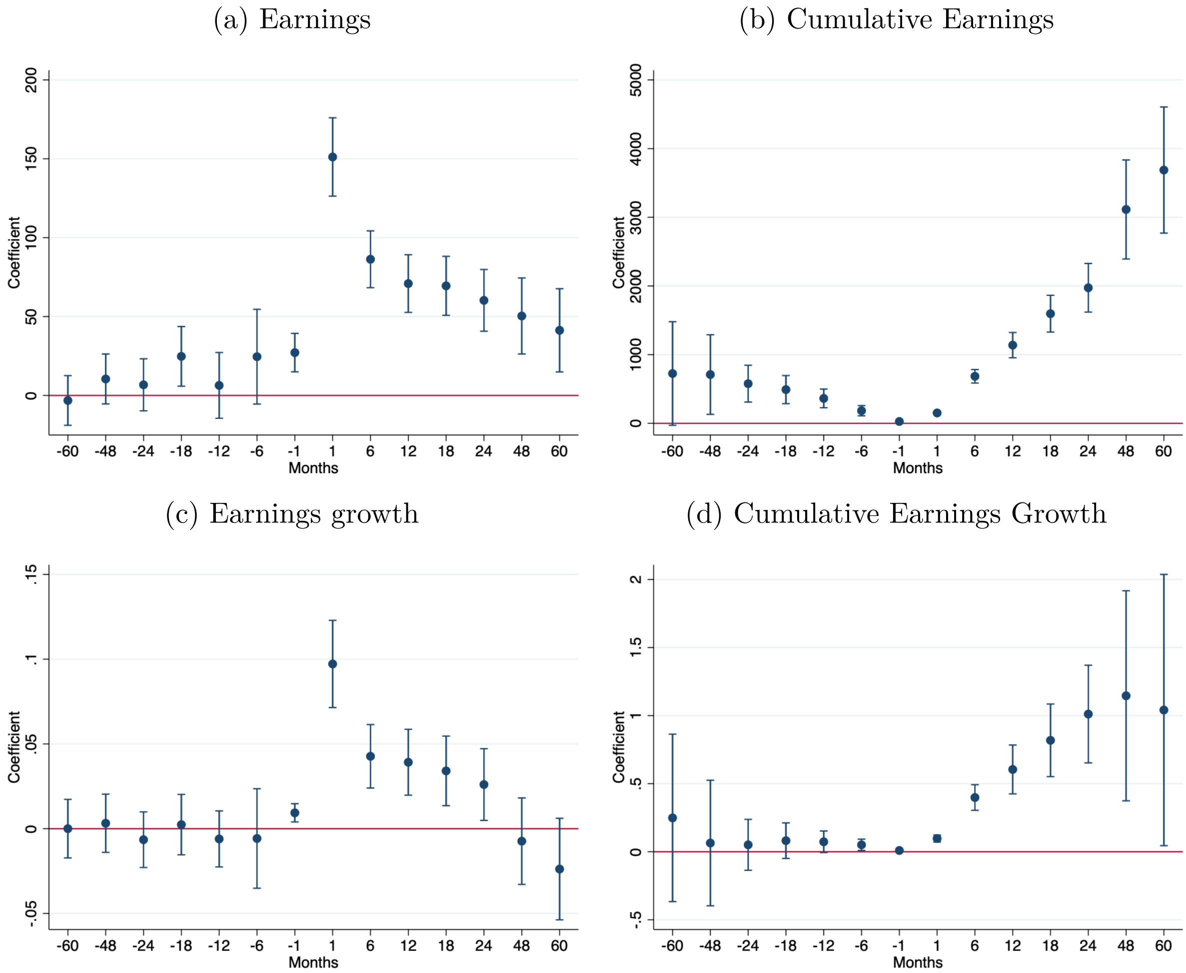
we construct $\overline{Y}_{-i,t}$, based on the full sample of workers, irrespective of the timing of their contract expiration date (i.e., there is no mechanical link between y_{it+h} measured for recently hired workers and $\overline{Y}_{-i,t+h}$ measured for all workers). This should further ensure that we keep economic conditions constant such that our instrument only captures atypical variation in open-ended positions availability, uncorrelated with business-cycle trends. Finally, we add individual and regional controls, including year, month, province, and sector fixed effects, overall experience, experience squared, gender, and interactions of age categories with education attainment.

We consider four earnings-related outcomes. First, we construct earnings by adding up the monthly labor income m for each year. Cumulative earnings are the sum of workers' earnings from the expiration of the fixed-term contract up to period t. Analogously, we construct earnings growth and cumulative earnings growth as the ratio between each variable at t and the monthly earnings at the baseline period 0: i.e., during the last month of the contract before expiring. Thus, the coefficients capture the effect on workers' outcomes compared to their last contract before switching to a new (fixed-term or open-ended) position. In terms of employment we evaluate: employment status, the probability of being employed in an open-ended contract, and cumulative experience in open-ended contracts measured in months. Additionally, we explore mobility responses.

7.1 Earnings

Figure 4 presents the long-term effects on workers' earnings of the transitory increase in open-ended vacancies *just* at the time of the worker's expiration date. We present the coefficient associated to the first lead of $logOEC_{-i,t+1}$, α_1, which we use as our source of exogenous variation. As shown in panel (a), we find a significant positive effect on workers' earnings, which is more pronounced in the first year after the contract change. While the effect is persistent over time, we observe smaller magnitudes as time goes by. This reduction is mechanic to some extent. A fraction of workers who were *unlucky* at t=0 and remained in a fixed-term contract, will eventually get promoted after a few years such that the gap with respect to those promoted at t=0 becomes smaller, explaining the observed effects. Workers who are more likely to be promoted also experience a significant increase in cumulative earnings (panel b), which captures both higher wages as well as more stable employment trajectories. Moreover, panel c) illustrates a positive effect on earnings growth concentrated over the first years, consistent with the same upgrade dynamics we mentioned before.

Figure 4: Effect of OEC regional shock on earnings



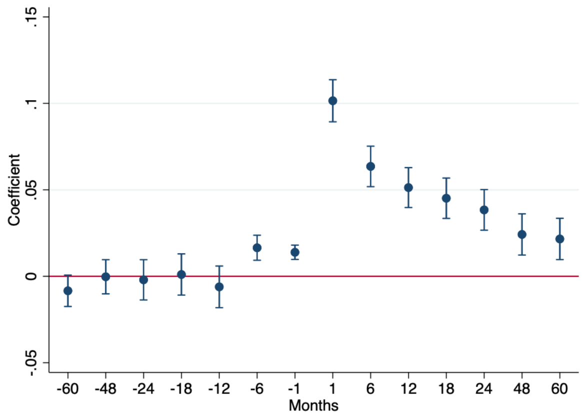
Notes: The sample is restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as the log of total new contracts. We also control for the mean of the outcomes at time t, for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

7.2 Employment and Mobility

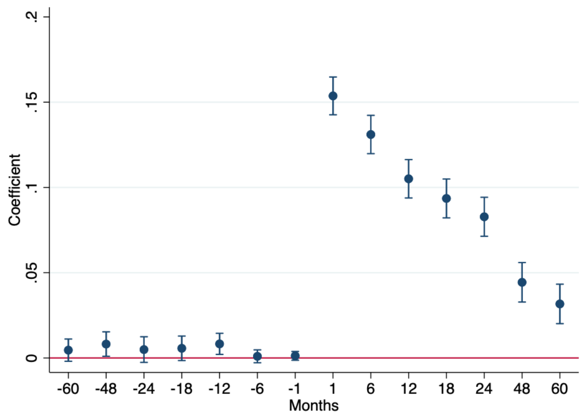
In terms of employment, our results suggest that upgrading to a permanent position places workers on a stable career path. As illustrated in Figure 5a, we find that the effect of better opportunities to switch to an open-ended contract translates into a higher employment probability even after 2 years of promotion. As expected, once workers start a job in a permanent contract, they are unlikely to return to a fixed-term position. Moreover, workers seem to be considerably less likely to change sectors and slightly less prone to move to another region, as depicted in Figure 6.

Figure 5: Effect of OEC regional shock on employment

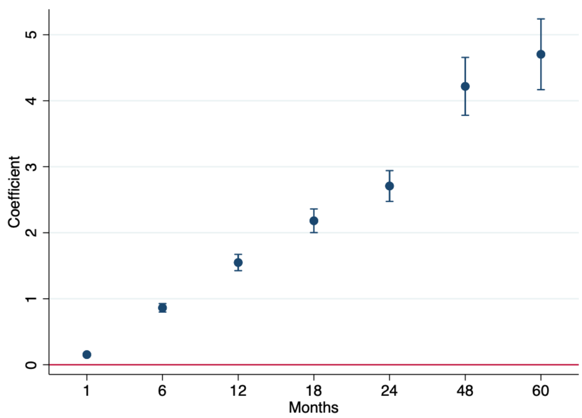
(a) Probability of employment



(b) OEC Status



(c) Cumulative months in OEC

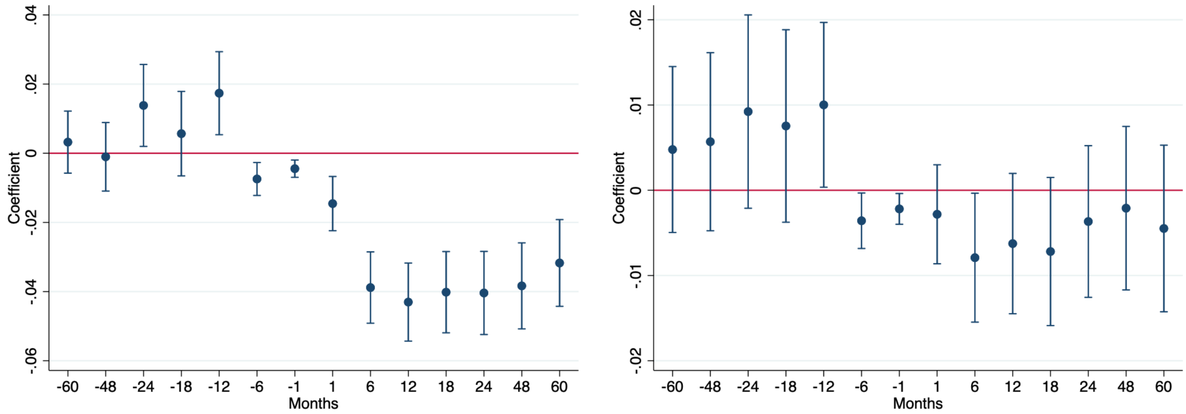


Notes: The sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as the log of total new contracts. We also control for the mean of the outcomes at time t, for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

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

(controlling for total new contracts)

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



Notes: The sample is restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline and who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as the log of total new contracts. We also control for the mean of the outcomes at time t, for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, leads, and lags of new fixed-term contracts.

8 Conclusion

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

By examining the Spanish context as a case study, we investigate how different types of contracts affect workers' careers. Consistent with recent evidence by Garcia-Louzao et al. (2021), workers who spent more time in fixed-term contracts experience lower earnings growth than workers who spent time in open-ended positions. Nevertheless, differences in earnings growth may reflect not only differences in returns between contract types but also heterogeneity among employees.

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

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

We find that individual promotion probabilities and experience accumulation in permanent positions are highly correlated to transitory variation in the opening of permanent contracts. Moreover, we uncover long-lasting effects on earnings, employment, and workers' mobility from being promoted to a permanent position.

The methodology we use is general, and not restricted to the dual labor market context. The key idea is to exploit two advantages of administrative registers, namely their high frequency, such that we know when exactly a worker's contract ends, and their large size, such that we can measure fluctuations in local labor market conditions. As most administrative registers share those same advantages, our method is widely applicable to address (dynamic) selection in the matching between workers and firms, jobs and contracts on the labor market.

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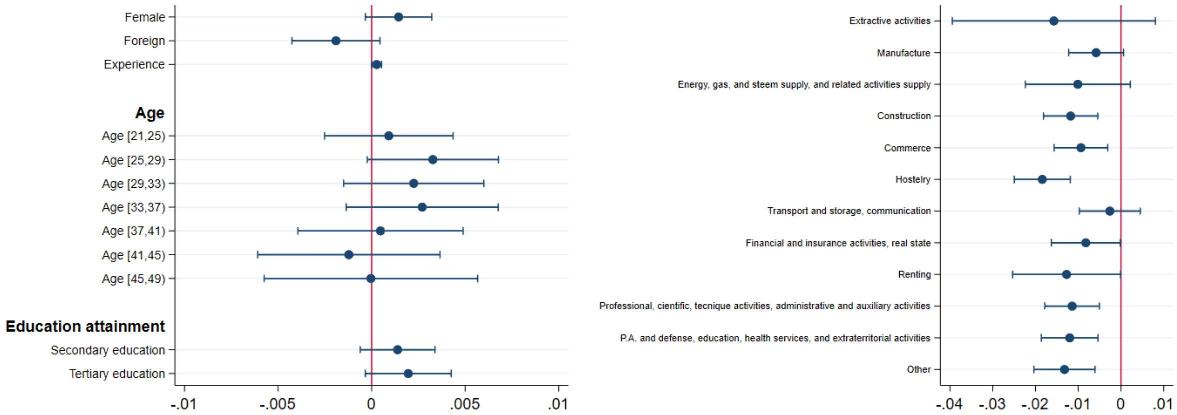
A Supplementary Figures

A.1 IV Results

Figure A.1.1: Exogeneity: Effect of individual characteristics and sector on $logOEC_{t+1}$



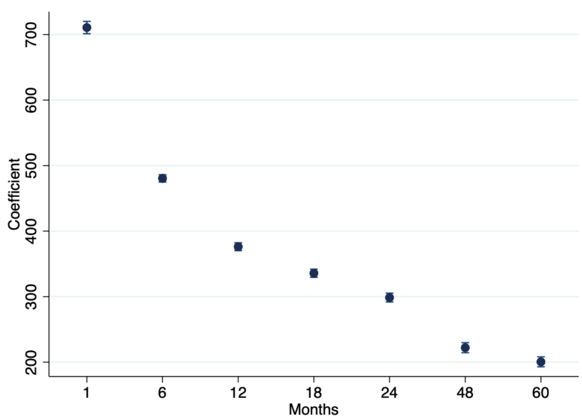
(a) Individual characteristics

(b) Sector

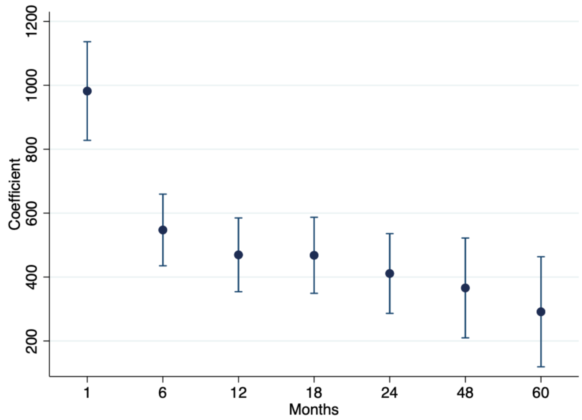
Notes: Additionally, we control for leads and lags of logOEC, year, month, and province fixed effects.

Figure A.1.2: Effect of transitioning into an OEC on earnings

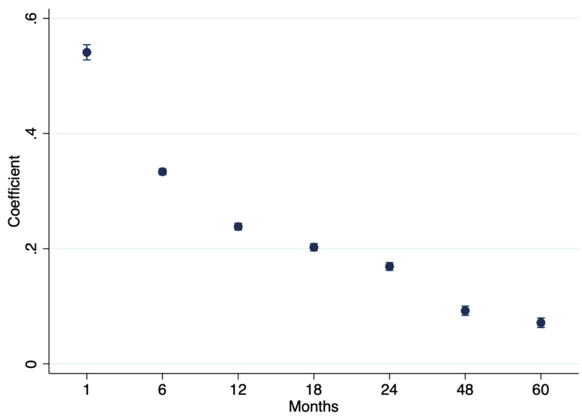
(a) Earnings - OLS



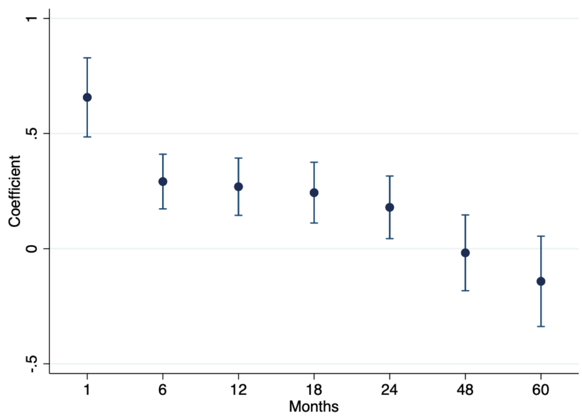
(b) Earnings - IV



(c) Earnings growth



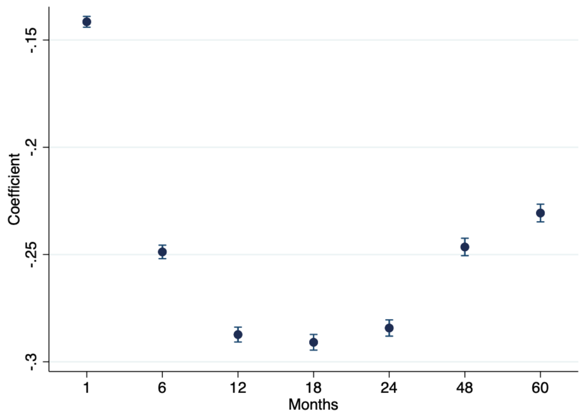
(d) Earnings growth IV



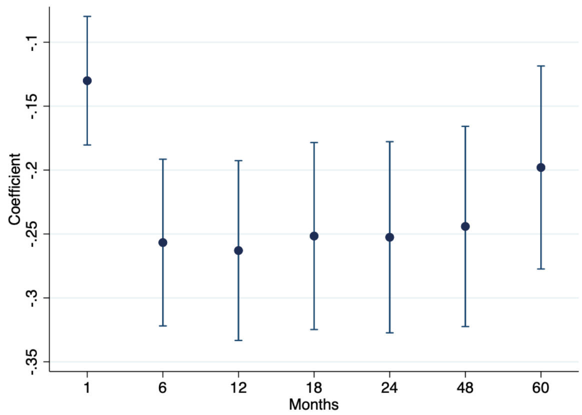
Notes: Sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as log of total new contracts. We also control for the mean of the outcomes at time t, for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

Figure A.1.3: Effect of transitioning into an OEC on mobility

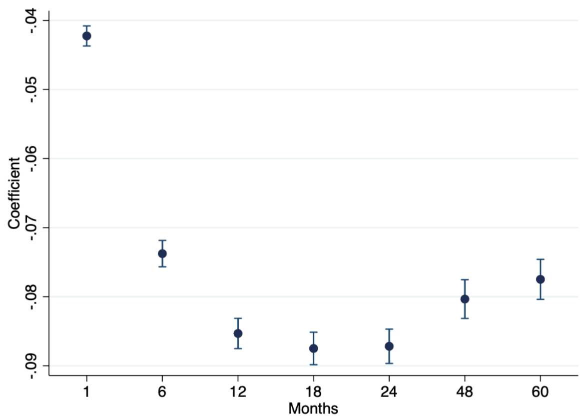
(a) Change Sector - OLS



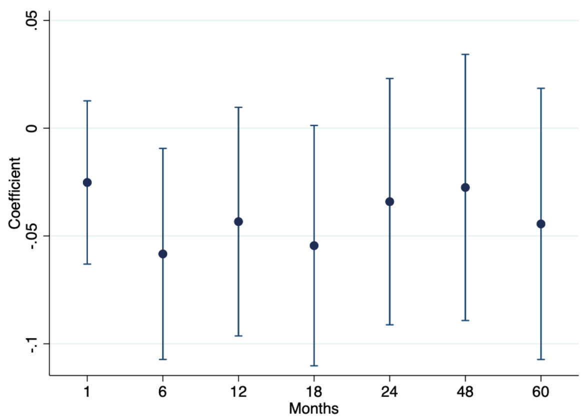
(b) Change Sector - IV



(c) Change Region - OLS

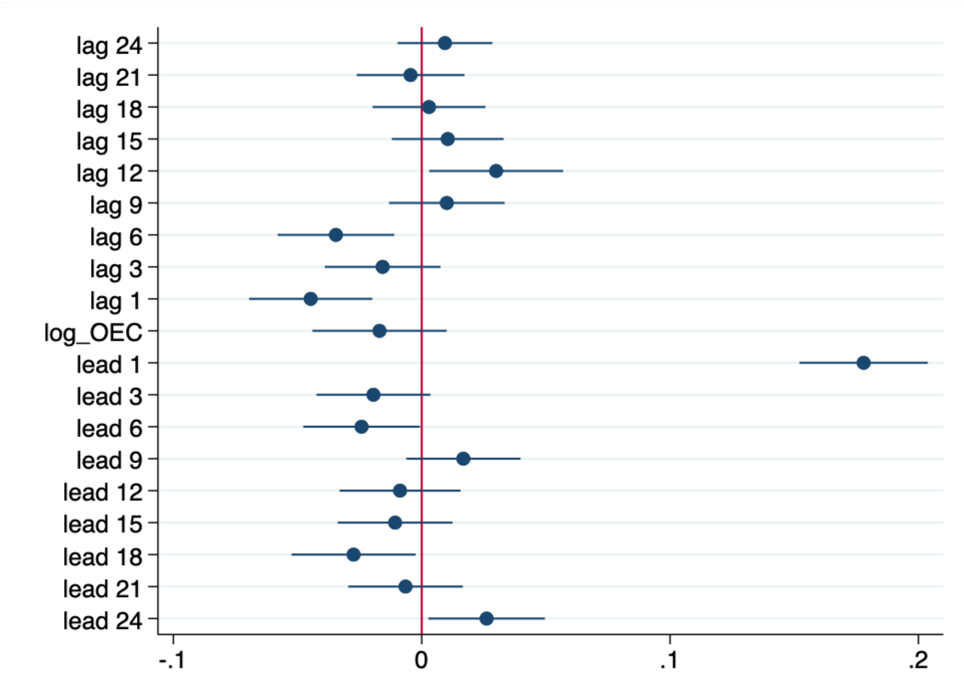


(d) Change Region - IV



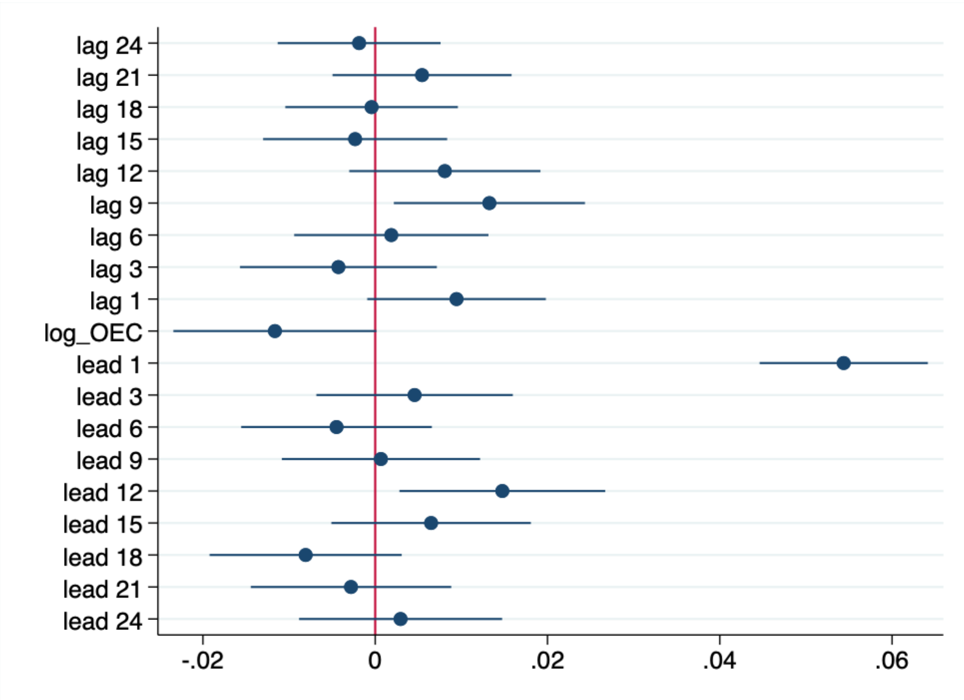
Notes: Sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as log of total new contracts. We also control for the mean of the outcomes at time t, for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

Figure A.1.4: National instrument: Promotion probabilities



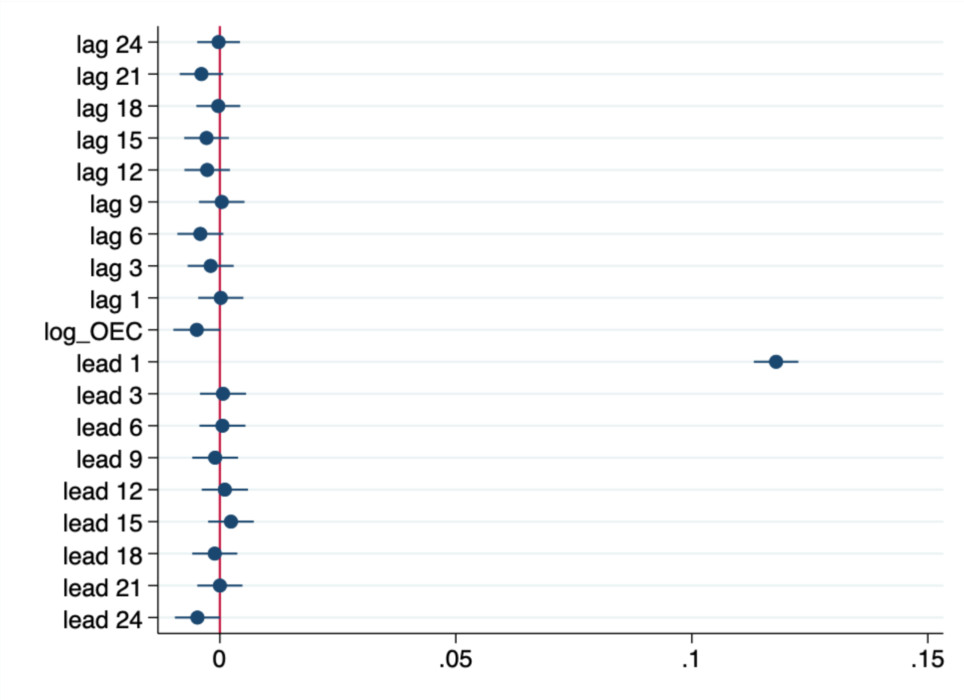
Notes: Sample: Workers in the last month of a fixed-term contract of least 0.8 years of tenure but less than 1.2 years of tenure. Coefficients of the probability of being promoted to a open-ended contract in $t+1$ on leads and lags of the log of new open ended contracts by month. Additional controls: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

Figure A.1.5: Sectoral instrument: Promotion probabilities



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

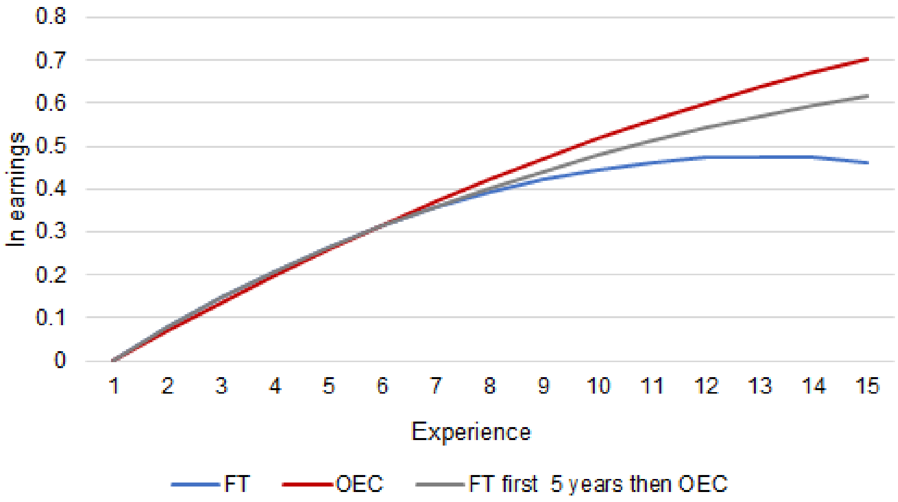
Figure A.1.6: Regional and sectoral instrument: Promotion probabilities



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

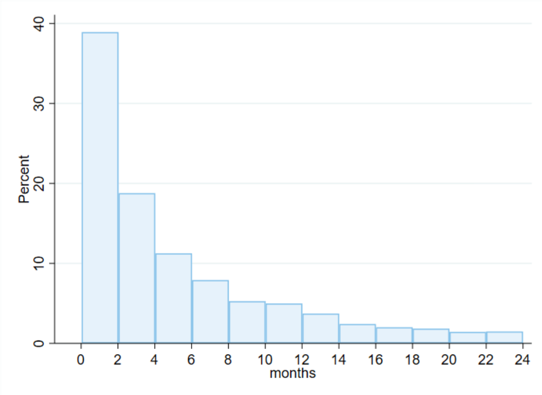
A.2 Descriptives

Figure A.2.7: Heterogeneous returns to experience by contract type

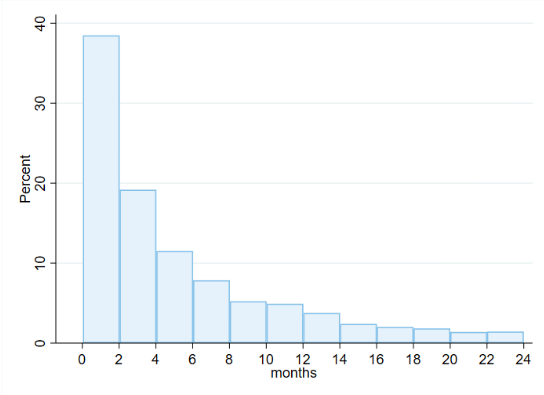


Notes: Fitted values based on experience coefficients from Column (3) in Table 2.

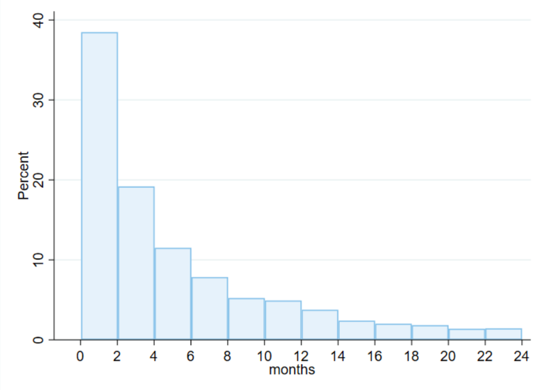
Figure A.2.8: Maximum tenure at expiration from FTC: FTC to FTC



(a) 1998-2017



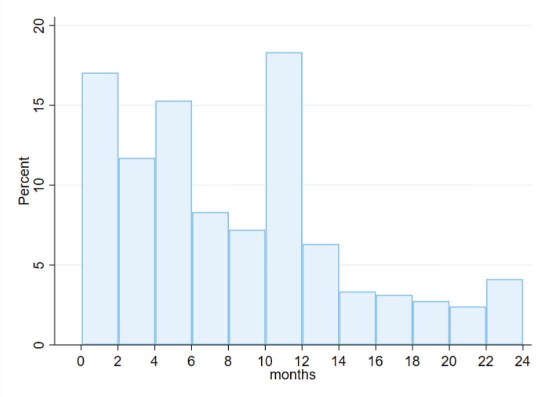
(b) 1998-2017



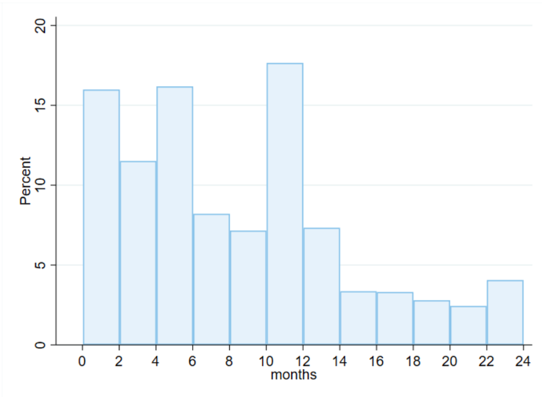
(c) 1998-2017

Notes: Maximum tenure workers that are not promoted

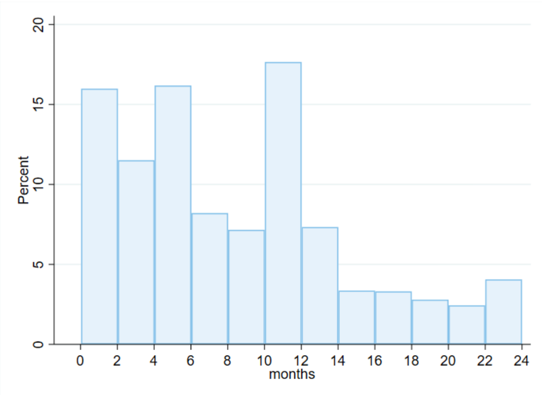
Figure A.2.9: Maximum tenure at expiration from FTC: FTC to OEC



(a) 1998-2017



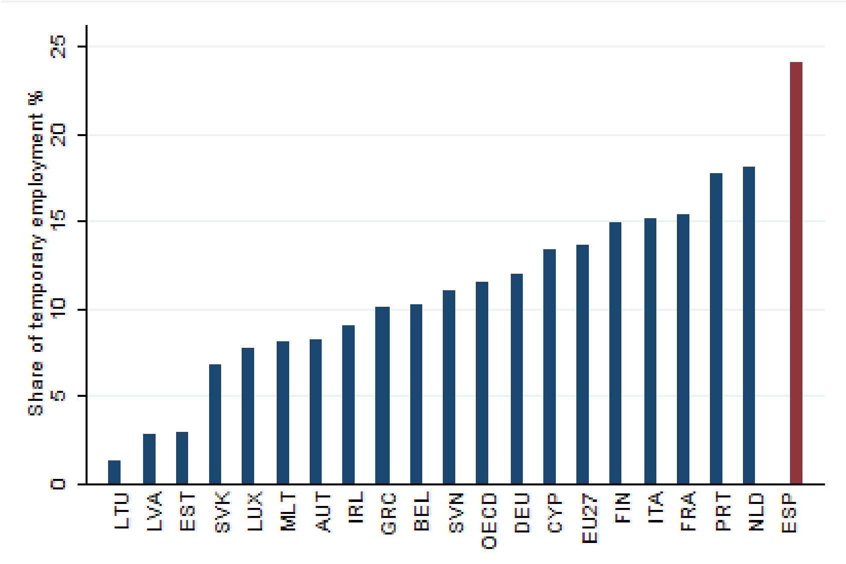
(b) 1998-2017



(c) 1998-2017

Notes: Maximum tenure workers that are promoted

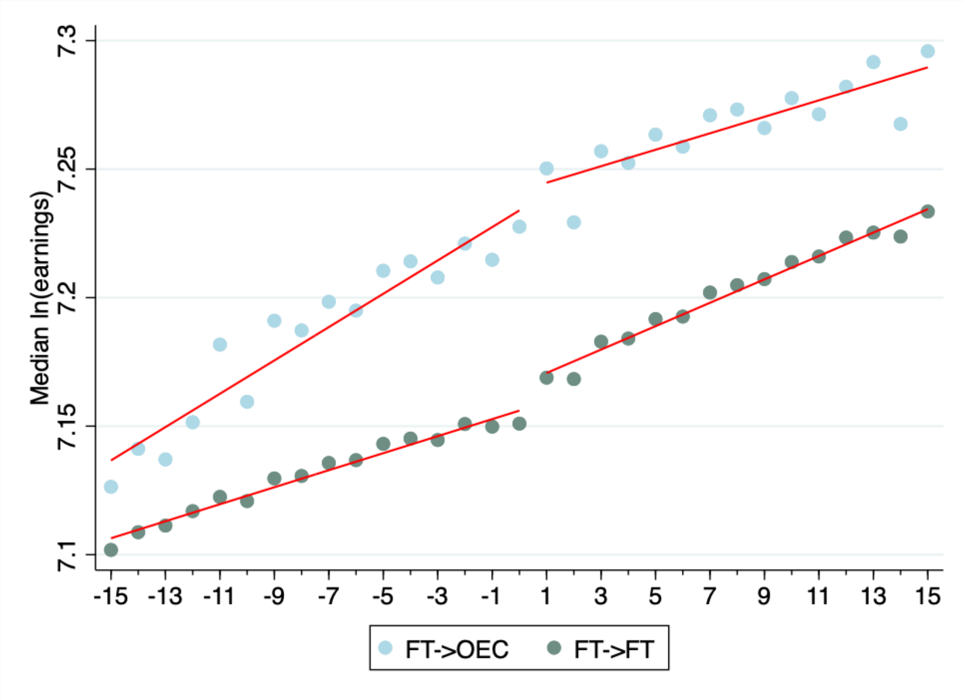
Figure A.2.10: Proportion of workers in temporary contracts by country, 2020



Notes: Temporary employment includes wage and salary workers whose job has a predetermined termination date. This indicator is measured as the percentage of dependent employees (i.e. wage and salary workers).
Source: OECD, Labour Market Statistics: Employment by permanency of the job: incidence

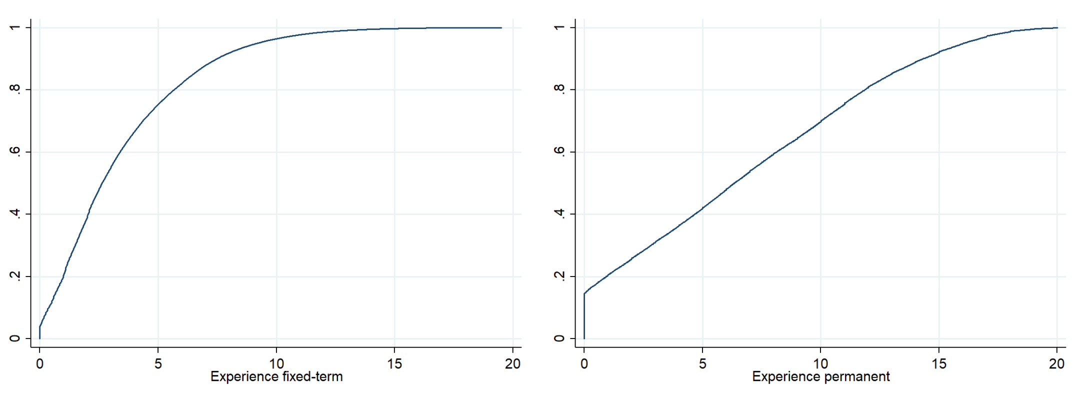
A.3 Selection into permanent positions

Figure A.3.11: Evolution of earnings: transitioning to a new contract



Notes: Median log earnings of workers 15 months before and after transitioning to a new contract.

Figure A.3.12: Cumulative distribution of maximum experience per worker



(a) Fixed-term contracts (b) Open ended contracts

Notes: Maximum experience in the estimation sample by type of contract

B Additional robustness and discussion

B.1 Inequality

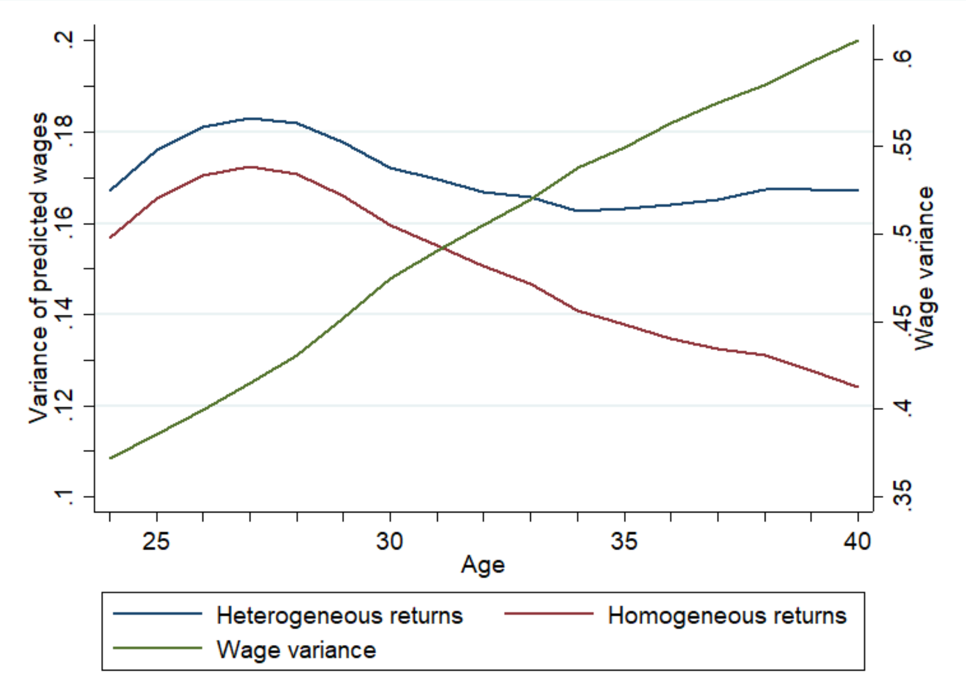
The dualism between permanent and fixed-term contracts creates persistent inequalities in the workers' earnings trajectories. The prior evidence establishes that one year of experience can generally have different returns depending on the type of contract where such experience was acquired. There is a significant share of workers who spend many years on temporary contracts, which has persistent effects on wage distribution.

We study how much of the heterogeneous long-term wage growth can be related to a different cumulative experience in fixed-term and permanent contracts. If experiences in permanent and fixed-term contracts were similarly distributed across young workers, the returns to experiences would not account for much of the variance in realized earnings. However, suppose many workers spend most of their careers on fixed-term contracts while others are just a tiny part. In that case, the returns to experiences could account for a substantial fraction of the variance in realized earnings. By using the sample of workers studied previously, the exercise tracked the variance of earnings and the part of the variance explained by differences in the accumulation of work experience. This exercise follows the approach by Arellano-Bover and Saltiel (2021) and computes:

$$\rho_a = \frac{\operatorname{Var}\left(\sum_{FT,OEC} \hat{\gamma}_m \cdot \operatorname{Exp}(m)_{\mathrm{it}} \ \mid age_{\mathrm{it}} \ = a\right)}{\operatorname{Var}\left(\ln y_{it} \mid \mathrm{age}_{it} = a\right)} \text{ and } \rho_a^H = \frac{\operatorname{Var}\left(\hat{\gamma} \cdot \operatorname{Exp}_{\dot{\imath}\mathrm{it}} \mid \mathrm{age}_{\mathrm{it}} = a\right)}{\operatorname{Var}\left(\ln y_{it} \mid \mathrm{age}_{\mathrm{it}} = a\right)}$$

Figure B.1.13 shows the fraction of the variance of wages explained by the returns to experience. The share of earnings variance accounted for experience decrease in the mid-30s, reaching 16.1% and 13.7% for heterogeneous and homogeneous returns to experience, respectively. After that, the proportion of explained volatility remains stable once the experience quality is considered, assuming homogenous returns to experience. The explained part continues decreasing. At age 40, the gap in explained earnings volatility is close to 5 p.p. Thus, the conventional approach of assuming all experience to be homogeneous substantially underestimates the fraction of earnings variance accounted for by varying experience profiles across workers.

Figure B.1.13: Variance of returns-to-experiences component over variance of log earnings



Notes: The returns to experience are calculated from a Mincerian equation on experience and interaction of education and gender, education and occupational skill group, age, age squared, sector, province, time fixed effects, and contract type. The homogeneous returns assume the returns to experience are the same regardless of the type of contract— the heterogeneous returns to experience control by the experience in fixed-term contracts and permanent contracts.

B.2 Wage growth: Causal impact of fixed-term contracts

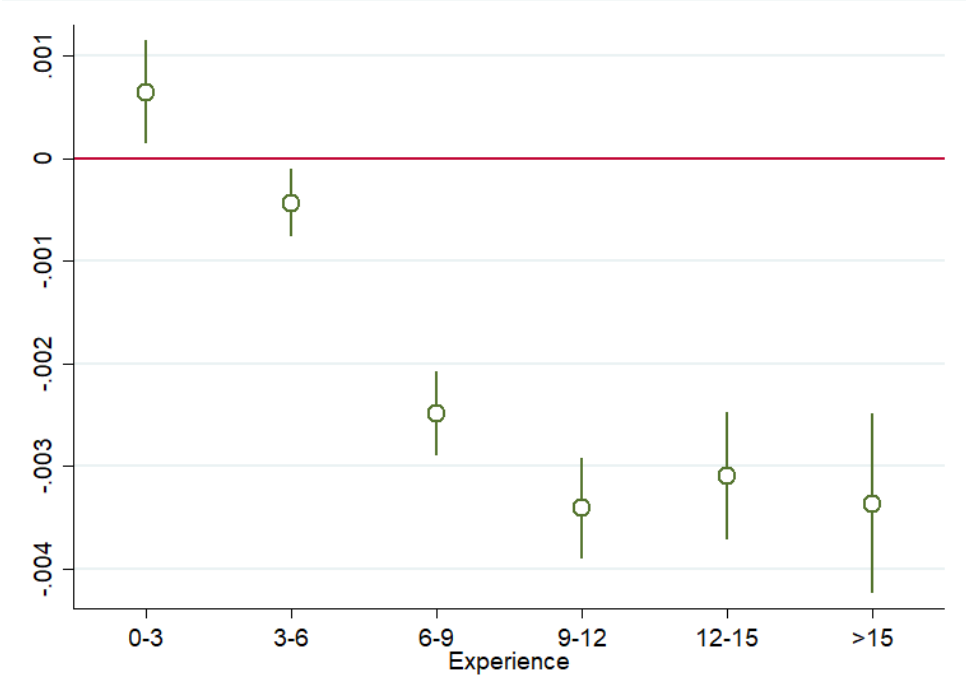
The previous results highlight that workers in permanent contracts may experience a smaller wage growth than similar workers. This section studies that directly by investigating how being in a fixed-term contract affects wage growth. Additionally, I implement an AIPW estimator to causally estimate the impact on the wage growth of individuals being hired under a fixed-term contract, conditional on a similar employment history to those hired in permanent contracts.

Consider the following equation:

$$\Delta \ln w_{ict} = contract_{it}^{FT}\gamma + \sigma_c + \psi_t + X_{it}\beta + \varepsilon_{ict}$$

where σ_c is a region fixed effect, ψ_t is a year-month fixed-effect, x_{it} is a vector of time-varying individual and job characteristics, (occupation skill level, education), and ε_{ict} is an error term. This regression is separately estimated at different experience intervals. In particular, at 0-3 years to experience, 3-6, 6-9, 9-12, and more than 15 years of experience.

Figure B.2.14: Gap on returns to experience between fixed-term and open-ended contracts

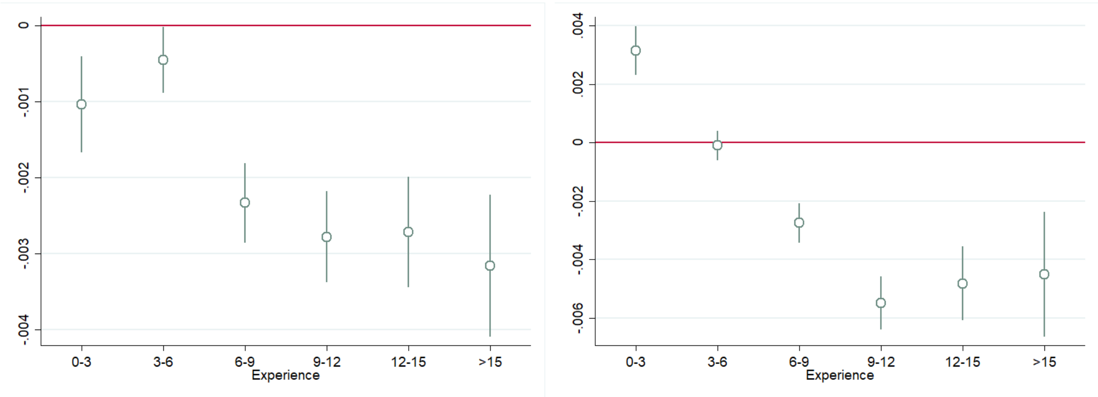


Notes: The returns to experience are calculated from and IPW design. Controls: gender, education and occupational skill group, age, age squared, sector, province, time fixed effects, and contract type.

The results suggest workers have a higher wage growth if being in a fixed-term contract during the first three years of experience. However, it turns negative in the following intervals. Workers get slightly lower wage growth from being employed in a fixed-term contract, which decreases much more at the 9-12 years of labor market experience.

The AIPW estimator has attractive theoretical properties and only requires practitioners to do two things they are already comfortable with: (1) specify a binary regression model for the propensity score and (2) specify a regression model for the outcome variable. Perhaps the most interesting property of this estimator is its so-called double robustness. The estimator remains consistent for the ATE if either the propensity score model or the outcome regression is misspecified, but the other is properly specified.

Figure B.2.15: Cumulative distribution of maximum experience per worker



(a) Non-college educated

(b) College educated

Notes: Maximum experience in the estimation sample by type of contract

B.3 Job ladder vs human capital

The key insight present in on-the-job search models is that an involuntary unemployment spell cuts a job ladder progression. This is because an unemployed worker looking for a job does not have a current employer as an option to weigh against new offers. In this sense, this brings him to the bottom of the ladder. We categorize workers based on whether they are involuntarily fired and have a period of unemployment larger than four months between one spell and the next. We would expect a pure job ladder mechanism to be unimportant among this group of workers. Hence, evidence of a positive return gap between fixed-term and open-ended contracts would be consistent with a human capital channel or another persistent effect of having more experience as a temporary worker.

We estimate equation 1 using the sub-sample of those experiencing unemployment before the next job and restriction information to the first wage after unemployment. The key takeaway is that we still see similar qualitative effects for this group of workers compared to the baseline estimate. We find evidence a job ladder channel does not fully explain the difference in returns to experience. However, the wage growth of these workers is considerably smaller, which suggests that even though a job ladder mechanism cannot fully explain results, that is very important in explaining wage growth.

Table 3: Laid-off workers: heterogeneous returns to experience by contract type

	(1)	(2)	(3)
		ln earnings	
$experience_{FT}$	0.0168***	0.0105***	0.00734***
	(0.00199)	(0.00189)	(0.00189)
$experience_{OEC}$	0.00451*	-0.000514	0.00432*
	(0.00204)	(0.00197)	(0.00198)
$exp \cdot exp_{FT}$	-0.243	-0.195	-0.147
	(0.181)	(0.171)	(0.169)
$exp \cdot exp_{OEC}$	1.447***	1.395***	1.075***
	(0.165)	(0.161)	(0.163)
Constant	6.755***	6.322***	6.266***
	(0.109)	(0.105)	(0.105)
Controls	Yes	Yes	Yes
Sector	No	Yes	Yes
Tenure	No	No	Yes
Individual FE	No	No	No

Standard errors in parentheses

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

Notes: Sample: Workers who entered the labor market between 1998 and 2003. Workers aged 18-40 years old. Every regression controls by worker fixed-effects. Column (1) Baseline specification. Column (2) Sector fixed effects. Column (3) Sector fixed effects and tenure. Column (4) Sector fixed effects, tenure, and type of contract. Standard errors clustered at the individual level. Additional controls: time and region fixed effects, interactions of gender with educational level, and interactions of occupation skill group and educational level.

C Supplementary tables

Table 4: Heterogeneous returns to experience by contract type

	(1)	(2)	(3)
		ln earnings	
experience	0.0303***		
	(0.000336)		
$experience_{FT}$		0.0247***	0.0266***
		(0.000449)	(0.000726)
$experience_{OEC}$		0.0353***	0.0359***
		(0.000380)	(0.000536)
Obs.	16266496	16266496	16255262
R2	0.474	0.482	0.751

Standard errors in parentheses

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

Notes: Sample: Workers who entered the labor market between 1998 and 2003. Workers aged 18-40 years old. Column (1) Baseline specification. Column (2) Considers separately experience in fixed-term and open-ended contracts. Column (3) Additionally includes worker fixed-effects. Additional controls: time and region fixed effects, interactions of gender with educational level, and interactions of occupation skill group and educational level. Regression using shares Source: MCVL 2006-2017

Table 5: Heterogeneous returns to experience by contract type

	(1)	(2)	(3)	(4)
		ln earnings		
$experience_{FT}$	0.0529***	0.0529***	0.112***	0.112***
	(0.00101)	(0.00101)	(0.00132)	(0.00132)
$experience_{OEC}$	0.0542***	0.0544***	0.0987***	0.0987***
	(0.000698)	(0.000697)	(0.000975)	(0.000973)
$exp * exp_{FT}$	-2.952***	-2.919***	-3.975***	-3.934***
	(0.0821)	(0.0821)	(0.0620)	(0.0619)
$exp * exp_{OEC}$	-0.961***	-0.984***	-1.714***	-1.735***
	(0.0411)	(0.0410)	(0.0298)	(0.0297)
contract fixed-term	-0.0235***	-0.0248***	-0.0348***	-0.0358***
	(0.00155)	(0.00155)	(0.00113)	(0.00113)
tenure	-0.00242***	-0.00233***	-0.00432***	-0.00414***
	(0.000411)	(0.000411)	(0.000246)	(0.000245)
Constant	8.379***	8.060***	7.411***	7.099***
	(0.0140)	(0.0215)	(0.0155)	(0.0208)
Controls	Yes	Yes	Yes	Yes
Individual FE	No	No	Yes	Yes
Sector	share	share	share	share
Tenure	Yes	Yes	Yes	Yes
Skill	FE	FE & Share	FE	FE & Share

Standard errors in parentheses

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

Notes: Sample: Workers who entered the labor market between 1998 and 2003. Workers aged 18-40 years old. Every regression controls by worker fixed-effects. Column (1) Baseline specification. Column (2) Sector fixed effects. Column (3) Sector fixed effects and tenure. Column (4) Sector fixed effects, tenure, and type of contract. Standard errors clustered at the individual level. Additional controls: time and region fixed effects, interactions of gender with educational level, and interactions of occupation skill group and educational level. Regression using shares

Table 6: Heterogeneous returns to experience by contract type

	(1)	(2)	(3)	(4)	(5)
	ln earnings				
experience	0.0508***				
	(0.000528)				
$experience^2$	-1.314***				
	(0.0323)				
exp_{FT}		0.0551***	0.0637***	0.0794***	0.0723***
		(0.000788)	(0.000795)	(0.00103)	(0.00105)
exp_{OEC}		0.0539***	0.0558***	0.0706***	0.0670***
		(0.000630)	(0.000613)	(0.000712)	(0.000759)
$exp \times exp_{FT}$		-2.454***	-3.373***	-3.312***	-2.398***
		(0.0636)	(0.0634)	(0.0552)	(0.0554)
$exp \times exp_{OEC}$		-0.975***	-1.049***	-1.446***	-1.298***
		(0.0401)	(0.0389)	(0.0311)	(0.0323)
Obs.	16266496	16266496	16266496	16255262	16255262
R^2	0.475	0.484	0.478	0.754	0.758

Standard errors in parentheses

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

Notes: exp, exp_{FT}, and exp_{OEC} account for experience, experience in fixed-term, and experience in open-ended contracts, respectively. Controls include gender and occupation-skill group interactions on education attainment, sector, region and time fixed-effects, age, age squared, and interactions of tenure with an indicator for a fixed-term contract. Errors are clustered at the worker level. Column (2) and (4) includes interactions of education with age categories

Table 7: National instrument: Baseline specification

	(1)	(2)	(3)	(4)	(5)
	Promotion to OEC in $t+1$				
$logOEC^{lag}_{12}$	0.0526***	-0.0212**	-0.0228***	-0.0191**	-0.0182**
	(0.00486)	(0.00655)	(0.00650)	(0.00637)	(0.00633)
$logOEC^{lag}_{11}$	0.0570***	0.0799***	0.0801***	0.0705***	0.0712***
	(0.00712)	(0.00822)	(0.00813)	(0.00786)	(0.00783)
$logOEC^{lag}_{10}$	0.0299***	0.0249**	0.0266**	0.0209*	0.0214*
	(0.00824)	(0.00890)	(0.00878)	(0.00852)	(0.00846)
$logOEC^{lag}_{9}$	0.0246**	0.00553	0.00286	-0.000307	-0.000491
	(0.00815)	(0.00874)	(0.00863)	(0.00844)	(0.00838)
$logOEC^{lag}_{8}$	-0.0244**	-0.0130	-0.0127	-0.00972	-0.0103
	(0.00827)	(0.00865)	(0.00855)	(0.00835)	(0.00829)
$logOEC^{lag}_{7}$	-0.0131	0.0445***	0.0448***	0.0399***	0.0398***
	(0.00855)	(0.00910)	(0.00899)	(0.00878)	(0.00871)
$logOEC^{lag}_{6}$	0.0510***	0.0195*	0.0192*	0.0151	0.0136
	(0.00831)	(0.00882)	(0.00871)	(0.00844)	(0.00839)
$logOEC^{lag}_{5}$	-0.0515***	-0.0281**	-0.0282**	-0.0277**	-0.0277**
	(0.00851)	(0.00900)	(0.00889)	(0.00864)	(0.00858)
$logOEC^{lag}_{4}$	-0.0508***	-0.0503***	-0.0487***	-0.0448***	-0.0451***
	(0.00858)	(0.00915)	(0.00904)	(0.00882)	(0.00876)
$logOEC^{lag}_{3}$	0.0265**	0.0122	0.00896	0.00963	0.0127
	(0.00865)	(0.00943)	(0.00932)	(0.00908)	(0.00902)
$logOEC^{lag}_{2}$	-0.0691***	-0.0419***	-0.0419***	-0.0371***	-0.0346***
	(0.00857)	(0.00941)	(0.00930)	(0.00904)	(0.00898)
$logOEC^{lag}_{1}$	-0.0272**	-0.0324***	-0.0288**	-0.0239**	-0.0212*
	(0.00872)	(0.00955)	(0.00944)	(0.00918)	(0.00912)
$logOEC_{0}$	-0.0479***	-0.0551***	-0.0542***	-0.0444***	-0.0437***
	(0.00905)	(0.0100)	(0.00991)	(0.00964)	(0.00959)
$logOEC^{lead}_{1}$	0.115***	0.267***	0.260***	0.247***	0.242***
	(0.00771)	(0.00944)	(0.00935)	(0.00907)	(0.00902)
$logOEC^{lead}_{2}$	0.0481***	0.0283**	0.0253*	0.0196	0.0168
	(0.00872)	(0.0107)	(0.0106)	(0.0103)	(0.0102)
$logOEC^{lead}_{3}$	-0.00237	-0.0252*	-0.0220*	-0.0188	-0.0188
	(0.00864)	(0.0103)	(0.0102)	(0.00994)	(0.00988)
$logOEC^{lead}_{4}$	0.0346***	0.00872	0.0112	0.00808	0.00931
	(0.00878)	(0.0100)	(0.00995)	(0.00975)	(0.00970)
$logOEC^{lead}_{5}$	-0.0335***	0.00845	0.00958	0.0120	0.0145
	(0.00901)	(0.0104)	(0.0103)	(0.0100)	(0.00999)
$logOEC^{lead}_{6}$	-0.0808***	-0.0431***	-0.0461***	-0.0370***	-0.0346***
	(0.00880)	(0.0101)	(0.0100)	(0.00974)	(0.00969)
$logOEC^{lead}_{7}$	0.0566***	0.0565***	0.0597***	0.0561***	0.0544***
	(0.00892)	(0.0104)	(0.0103)	(0.0100)	(0.00996)
$logOEC^{lead}_{8}$	0.00894	-0.0371***	-0.0366***	-0.0356***	-0.0343***
	(0.00914)	(0.0106)	(0.0105)	(0.0103)	(0.0102)
$logOEC^{lead}_{9}$	-0.0353***	0.00484	0.00220	0.00474	0.00269
	(0.00900)	(0.0108)	(0.0107)	(0.0104)	(0.0103)
$logOEC^{lead}_{10}$	0.0666***	0.00441	0.00680	0.000621	-0.000503
	(0.00917)	(0.0110)	(0.0109)	(0.0106)	(0.0105)
$logOEC^{lead}_{11}$	0.0445***	-0.0332**	-0.0370***	-0.0348**	-0.0372***
	(0.00904)	(0.0112)	(0.0110)	(0.0107)	(0.0107)
$logOEC^{lead}_{12}$	0.0196*	-0.0140	-0.0142	-0.0166	-0.0144
	(0.00837)	(0.0117)	(0.0116)	(0.0113)	(0.0112)
Obs.	331,467	331,467	331,467	331,467	331,467
R2	0.027	0.036	0.061	0.115	0.126
Time FE	No	Yes	Yes	Yes	Yes
Region FE	No	No	Yes	Yes	Yes
Sector FE	No	No	No	Yes	Yes
Individual FE	No	No	No	No	Yes

Standard errors in parentheses

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

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t + 1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017

Table 8: National instrument: Control by new FT contracts

	(1)	(2)	(3)	(4)	(5)
	Promotion to OEC in $t+1$				
$logOEC_{12}^{lag}$	0.0707***	-0.0343***	-0.0333***	-0.0291***	-0.0282***
	(0.00543)	(0.00763)	(0.00756)	(0.00741)	(0.00736)
$logOEC_{11}^{lag}$	0.0844***	0.108***	0.108***	0.0983***	0.100***
	(0.00782)	(0.00993)	(0.00982)	(0.00950)	(0.00946)
$logOEC_{10}^{lag}$	0.00936	0.0106	0.0129	0.00978	0.0106
	(0.00902)	(0.0103)	(0.0102)	(0.00984)	(0.00978)
$logOEC_{9}^{lag}$	0.00405	0.00903	0.00837	0.00552	0.00615
	(0.00867)	(0.00978)	(0.00966)	(0.00944)	(0.00937)
$logOEC_{8}^{lag}$	-0.0245**	-0.00208	-0.00121	0.00119	0.00109
	(0.00879)	(0.00958)	(0.00948)	(0.00925)	(0.00919)
$logOEC_{7}^{lag}$	0.0270**	0.0389***	0.0423***	0.0384***	0.0386***
	(0.00920)	(0.0100)	(0.00993)	(0.00970)	(0.00961)
$logOEC_{6}^{lag}$	0.0378***	0.0252**	0.0244*	0.0185*	0.0170
	(0.00898)	(0.00972)	(0.00961)	(0.00932)	(0.00926)
$logOEC_{5}^{lag}$	-0.0358***	-0.0135	-0.0134	-0.0145	-0.0144
	(0.00909)	(0.0101)	(0.00996)	(0.00968)	(0.00962)
$logOEC_{4}^{lag}$	-0.0270**	-0.0495***	-0.0500***	-0.0485***	-0.0493***
	(0.00924)	(0.00991)	(0.00979)	(0.00955)	(0.00948)
$logOEC_{3}^{lag}$	0.0365***	0.00636	0.00416	0.00506	0.00827
	(0.00931)	(0.0104)	(0.0103)	(0.0100)	(0.00996)
$logOEC_{2}^{lag}$	-0.0373***	-0.0504***	-0.0503***	-0.0454***	-0.0422***
	(0.00905)	(0.0103)	(0.0102)	(0.00987)	(0.00980)
$logOEC_{1}^{lag}$	-0.0370***	-0.0278**	-0.0259*	-0.0230*	-0.0199*
	(0.00917)	(0.0105)	(0.0104)	(0.0101)	(0.0100)
log_OEC_0	-0.0592***	-0.0395***	-0.0439***	-0.0382***	-0.0365***
	(0.00954)	(0.0115)	(0.0114)	(0.0111)	(0.0110)
$logOEC_{1}^{lead}$	0.179***	0.264***	0.253***	0.236***	0.231***
	(0.00891)	(0.0112)	(0.0111)	(0.0107)	(0.0107)
$logOEC_{2}^{lead}$	0.0639***	0.0428***	0.0383**	0.0280*	0.0259*
	(0.0103)	(0.0122)	(0.0121)	(0.0117)	(0.0117)
$logOEC_{3}^{lead}$	-0.0595***	-0.0334**	-0.0348**	-0.0341**	-0.0338**
	(0.00981)	(0.0115)	(0.0114)	(0.0111)	(0.0111)
$logOEC_{4}^{lead}$	-0.00244	0.0182	0.0188	0.0108	0.0128
	(0.00993)	(0.0114)	(0.0113)	(0.0111)	(0.0110)
$logOEC_{5}^{lead}$	-0.0371***	0.00316	-0.000223	-0.00246	0.000363
	(0.0102)	(0.0118)	(0.0117)	(0.0114)	(0.0113)
$logOEC_{6}^{lead}$	-0.0348***	-0.00952	-0.0157	-0.0133	-0.0116
	(0.0100)	(0.0114)	(0.0113)	(0.0109)	(0.0109)
$logOEC_{7}^{lead}$	0.0233*	0.0562***	0.0537***	0.0464***	0.0441***
	(0.0102)	(0.0122)	(0.0120)	(0.0117)	(0.0116)
$logOEC_{8}^{lead}$	0.0151	-0.0306**	-0.0333**	-0.0336**	-0.0323**
	(0.0104)	(0.0116)	(0.0115)	(0.0112)	(0.0112)
$logOEC_{9}^{lead}$	-0.0801***	0.0212	0.0151	0.0146	0.0121
	(0.0103)	(0.0123)	(0.0121)	(0.0118)	(0.0118)
$logOEC_{10}^{lead}$	-0.00803	0.00726	0.00753	-0.0000973	-0.00112
	(0.0105)	(0.0119)	(0.0118)	(0.0115)	(0.0114)
$logOEC_{11}^{lead}$	-0.0201	-0.00958	-0.0165	-0.0185	-0.0203
	(0.0103)	(0.0123)	(0.0121)	(0.0118)	(0.0117)
$logOEC_{12}^{lead}$	0.00611	-0.0131	-0.0127	-0.0149	-0.0125
	(0.00992)	(0.0133)	(0.0131)	(0.0127)	(0.0127)
Obs.	331,467	331,467	331,467	331,467	331,467
R2	0.033	0.037	0.062	0.115	0.126
Time FE	No	Yes	Yes	Yes	Yes
Region FE	No	No	Yes	Yes	Yes
Sector FE	No	No	No	Yes	Yes
Individual FE	No	No	No	No	Yes

Standard errors in parentheses

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

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t+1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017

Table 9: Regional instrument: Baseline specification

	(1)	(2)	(3)	(4)	(5)
			Promotion to OEC in $t+1$		
$logOEC_{12}^{lag}$	0.00628	-0.00202	-0.00192	0.000687	0.00111
	(0.00321)	(0.00362)	(0.00361)	(0.00351)	(0.00349)
$logOEC_{11}^{lag}$	0.0164***	0.0645***	0.0623***	0.0582***	0.0588***
	(0.00347)	(0.00384)	(0.00381)	(0.00369)	(0.00367)
$logOEC_{10}^{lag}$	0.0246***	0.0107**	0.00910*	0.00690	0.00760*
	(0.00357)	(0.00400)	(0.00397)	(0.00384)	(0.00381)
$logOEC_{9}^{lag}$	-0.00454	-0.00578	-0.00580	-0.00423	-0.00379
	(0.00357)	(0.00395)	(0.00393)	(0.00381)	(0.00379)
$logOEC_{8}^{lag}$	-0.00470	0.00464	0.00345	0.00523	0.00495
	(0.00370)	(0.00406)	(0.00402)	(0.00390)	(0.00387)
$logOEC_{7}^{lag}$	-0.0181***	0.0205***	0.0163***	0.0162***	0.0165***
	(0.00370)	(0.00409)	(0.00406)	(0.00394)	(0.00391)
$logOEC_{6}^{lag}$	-0.00562	0.0171***	0.0160***	0.0124**	0.0128***
	(0.00365)	(0.00405)	(0.00400)	(0.00388)	(0.00385)
$logOEC_{5}^{lag}$	-0.0131***	0.00357	-0.000638	-0.00207	-0.00223
	(0.00372)	(0.00410)	(0.00406)	(0.00394)	(0.00392)
$logOEC_{4}^{lag}$	-0.0252***	-0.000847	-0.00826*	-0.00840*	-0.00868*
	(0.00369)	(0.00410)	(0.00407)	(0.00394)	(0.00391)
$logOEC_{3}^{lag}$	0.00431	0.0301***	0.0262***	0.0256***	0.0261***
	(0.00371)	(0.00415)	(0.00410)	(0.00397)	(0.00395)
$logOEC_{2}^{lag}$	0.00502	0.0115**	0.00326	0.00451	0.00610
	(0.00370)	(0.00415)	(0.00411)	(0.00398)	(0.00396)
$logOEC_{1}^{lag}$	0.0204***	0.0218***	0.0165***	0.0181***	0.0186***
	(0.00372)	(0.00416)	(0.00413)	(0.00400)	(0.00397)
$logOEC_{0}$	0.0213***	0.00797	0.00601	0.00670	0.00607
	(0.00396)	(0.00422)	(0.00418)	(0.00405)	(0.00403)
$logOEC_{1}^{lead}$	0.114***	0.138***	0.135***	0.127***	0.125***
	(0.00370)	(0.00414)	(0.00409)	(0.00395)	(0.00393)
$logOEC_{2}^{lead}$	0.00571	-0.0437***	-0.0464***	-0.0412***	-0.0413***
	(0.00379)	(0.00433)	(0.00428)	(0.00413)	(0.00411)
$logOEC_{3}^{lead}$	0.000147	-0.0374***	-0.0386***	-0.0333***	-0.0322***
	(0.00376)	(0.00425)	(0.00421)	(0.00407)	(0.00405)
$logOEC_{4}^{lead}$	0.00700	-0.0174***	-0.0216***	-0.0210***	-0.0202***
	(0.00385)	(0.00431)	(0.00426)	(0.00413)	(0.00410)
$logOEC_{5}^{lead}$	-0.0139***	-0.00606	-0.0136**	-0.0146***	-0.0134**
	(0.00384)	(0.00434)	(0.00429)	(0.00415)	(0.00412)
$logOEC_{6}^{lead}$	-0.0148***	-0.0155***	-0.0230***	-0.0232***	-0.0231***
	(0.00380)	(0.00426)	(0.00421)	(0.00408)	(0.00406)
$logOEC_{7}^{lead}$	0.00476	-0.0122**	-0.0176***	-0.0194***	-0.0186***
	(0.00385)	(0.00432)	(0.00427)	(0.00414)	(0.00412)
$logOEC_{8}^{lead}$	-0.0269***	-0.0217***	-0.0277***	-0.0263***	-0.0256***
	(0.00383)	(0.00431)	(0.00426)	(0.00412)	(0.00409)
$logOEC_{9}^{lead}$	-0.0167***	-0.0178***	-0.0258***	-0.0233***	-0.0228***
	(0.00379)	(0.00428)	(0.00425)	(0.00411)	(0.00408)
$logOEC_{1}^{lead}0$	-0.0124**	-0.0353***	-0.0412***	-0.0387***	-0.0389***
	(0.00380)	(0.00433)	(0.00431)	(0.00417)	(0.00414)
$logOEC_{1}^{lead}1$	-0.00580	-0.0319***	-0.0371***	-0.0330***	-0.0320***
	(0.00373)	(0.00429)	(0.00427)	(0.00414)	(0.00411)
$logOEC_{1}^{lead}2$	-0.0286***	-0.0459***	-0.0493***	-0.0439***	-0.0418***
	(0.00373)	(0.00426)	(0.00426)	(0.00412)	(0.00410)
Obs.	331,032	331,032	331,032	331,032	331,032
R2	0.030	0.043	0.060	0.114	0.125
Time FE	No	Yes	Yes	Yes	Yes
Region FE	No	No	Yes	Yes	Yes
Sector FE	No	No	No	Yes	Yes
Individual FE	No	No	No	No	Yes

Standard errors in parentheses

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

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t + 1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017

Table 10: Regional instrument: Control by new FT contracts

	(1)	(2)	(3)	(4)	(5)
	Promotion to OEC in $t+1$				
$logOEC_{12}^{lag}$	0.0144***	0.00616	0.00270	0.00466	0.00496
	(0.00336)	(0.00366)	(0.00367)	(0.00357)	(0.00355)
$logOEC_{11}^{lag}$	0.0415***	0.0790***	0.0708***	0.0663***	0.0668***
	(0.00359)	(0.00387)	(0.00386)	(0.00374)	(0.00371)
$logOEC_{10}^{lag}$	0.0218***	0.0175***	0.0125**	0.00999*	0.0107**
	(0.00376)	(0.00404)	(0.00403)	(0.00390)	(0.00387)
$logOEC_{9}^{lag}$	-0.00666	0.000724	-0.00652	-0.00508	-0.00451
	(0.00373)	(0.00399)	(0.00398)	(0.00386)	(0.00383)
$logOEC_{8}^{lag}$	-0.00214	0.0111**	0.00499	0.00652	0.00647
	(0.00385)	(0.00408)	(0.00407)	(0.00395)	(0.00392)
$logOEC_{7}^{lag}$	-0.00266	0.0216***	0.0144***	0.0145***	0.0148***
	(0.00385)	(0.00414)	(0.00412)	(0.00400)	(0.00397)
$logOEC_{6}^{lag}$	0.00964*	0.0190***	0.0155***	0.0123**	0.0126**
	(0.00379)	(0.00408)	(0.00406)	(0.00393)	(0.00391)
$logOEC_{5}^{lag}$	-0.0108**	0.00783	0.000992	0.000277	-0.0000216
	(0.00386)	(0.00413)	(0.00412)	(0.00400)	(0.00397)
$logOEC_{4}^{lag}$	-0.000633	-0.000509	-0.00928*	-0.00898*	-0.00959*
	(0.00386)	(0.00414)	(0.00413)	(0.00400)	(0.00397)
$logOEC_{3}^{lag}$	0.0134***	0.0351***	0.0277***	0.0265***	0.0268***
	(0.00388)	(0.00417)	(0.00415)	(0.00403)	(0.00400)
$logOEC_{2}^{lag}$	0.0133***	0.0185***	0.00550	0.00620	0.00757
	(0.00386)	(0.00418)	(0.00418)	(0.00405)	(0.00402)
$logOEC_{1}^{lag}$	0.0199***	0.0278***	0.0181***	0.0187***	0.0191***
	(0.00388)	(0.00419)	(0.00418)	(0.00405)	(0.00402)
$logOEC_{0}$	0.0258***	0.0140***	0.00698	0.00682	0.00628
	(0.00405)	(0.00424)	(0.00423)	(0.00409)	(0.00407)
$logOEC_{1}^{lead}$	0.135***	0.144***	0.133***	0.125***	0.122***
	(0.00385)	(0.00417)	(0.00415)	(0.00401)	(0.00399)
$logOEC_{2}^{lead}$	-0.00344	-0.0405***	-0.0504***	-0.0457***	-0.0457***
	(0.00400)	(0.00436)	(0.00435)	(0.00421)	(0.00418)
$logOEC_{3}^{lead}$	-0.0138***	-0.0355***	-0.0474***	-0.0422***	-0.0409***
	(0.00396)	(0.00428)	(0.00428)	(0.00414)	(0.00412)
$logOEC_{4}^{lead}$	-0.00377	-0.0156***	-0.0281***	-0.0276***	-0.0265***
	(0.00403)	(0.00433)	(0.00433)	(0.00419)	(0.00416)
$logOEC_{5}^{lead}$	-0.0111**	-0.00878*	-0.0228***	-0.0236***	-0.0223***
	(0.00405)	(0.00437)	(0.00435)	(0.00421)	(0.00418)
$logOEC_{6}^{lead}$	-0.00389	-0.0163***	-0.0298***	-0.0295***	-0.0293***
	(0.00400)	(0.00430)	(0.00429)	(0.00415)	(0.00413)
$logOEC_{7}^{lead}$	-0.00202	-0.00971*	-0.0238***	-0.0249***	-0.0242***
	(0.00407)	(0.00435)	(0.00434)	(0.00421)	(0.00419)
$logOEC_{8}^{lead}$	-0.00665	-0.0176***	-0.0308***	-0.0291***	-0.0287***
	(0.00403)	(0.00433)	(0.00433)	(0.00418)	(0.00416)
$logOEC_{9}^{lead}$	-0.0153***	-0.0125**	-0.0279***	-0.0262***	-0.0258***
	(0.00402)	(0.00431)	(0.00432)	(0.00417)	(0.00414)
$logOEC_{10}^{lead}$	-0.0129**	-0.0240***	-0.0404***	-0.0385***	-0.0389***
	(0.00407)	(0.00436)	(0.00438)	(0.00424)	(0.00421)
$logOEC_{11}^{lead}$	-0.00684	-0.0191***	-0.0344***	-0.0315***	-0.0304***
	(0.00399)	(0.00433)	(0.00435)	(0.00421)	(0.00419)
$logOEC_{12}^{lead}$	-0.0199***	-0.0319***	-0.0467***	-0.0423***	-0.0402***
	(0.00395)	(0.00431)	(0.00434)	(0.00420)	(0.00417)
Obs.	331,032	331,032	331,032	331,032	331,032
R2	0.042	0.052	0.061	0.114	0.125
Time FE	No	Yes	Yes	Yes	Yes
Region FE	No	No	Yes	Yes	Yes
Sector FE	No	No	No	Yes	Yes
Individual FE	No	No	No	No	Yes

Standard errors in parentheses

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

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t+1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017