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Dual Returns to Experience

Abstract

In this paper we study how labor market duality affects human capital accumulation and wage trajectories of young workers. Using rich administrative data for Spain, we follow workers since their entry into the labor market to measure experience accumulated under different contractual arrangements and we estimate their wage returns. We document lower returns to experience accumulated in fixed-term contracts compared to permanent contracts and show that this difference is neither due to unobserved firm heterogeneity nor match quality. Instead, we provide evidence that the gap in returns is due to lower human capital accumulation while working under fixed-term contracts. In line with skill-learning complementarity, our results suggest that the widespread use of fixed-term work arrangements reduces skill acquisition of high-skilled workers, holding back life-cycle wage growth by up to 16 percentage points after 15 years since labor market entry.

JEL-Codes: J300, J410, J630.

Keywords: labor market duality, human capital, earnings dynamics.

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1 Introduction

Short-term flexible labor practices are becoming increasingly popular and, together with the rise of the *gig* economy, have attracted a high level of attention (Krueger, 2018). In recent years, the use of short-term work arrangements, such as temporary contracts, has become widespread in many European countries, where labor markets are relatively more rigid and regulated than those in the United States and the United Kingdom (ter Weel, 2018).

Despite allowing employers to easily adapt to fluctuations in demand (Aguirregabiria and Alonso-Borrego, 2014), the impact of temporary arrangements on worker's labor market careers is still debated. On the one hand, workers might benefit from their availability since they ease job finding (de Graaf-Zijl et al., 2011) and mitigate wage losses associated with skill depreciation during non-employment (Guvenen et al., 2017; Jarosch, 2021). On the other hand, they could be detrimental if they induce an unstable career (Blanchard and Landier, 2002; García-Pérez et al., 2019) or lower firm-sponsored on-the-job training (Cabrales et al., 2017; Bratti et al., 2021).

Temporary contracts might shorten non-employment spells and let workers accumulate experience with fewer interruptions, but the quality of that experience may be worse due to poorer learning opportunities, translating eventually into wage losses. In this paper we shed light on how human capital accumulates under different types of contracts, fixed-term versus open-ended, and how this affects workers' wage trajectories during their first years in the labor market. We perform our analysis in the context of the Spanish labor market, where the use of fixed-term contracts is the rule rather than the exception: more than 90% of the contracts signed each month are fixed-term and around 25% of the workforce is under some form of temporary employment (Felgueroso et al., 2018).

We rely on rich administrative data that allow us to follow individuals since labor market entry and to measure the exact time worked under permanent and temporary contracts separately. We use these precise measures of accumulated experience to estimate reduced-form wage regressions derived from a stylized framework of human capital accumulation in a dual labor market. In our empirical analysis, we are able to control for workers' permanent heterogeneity as well as contemporaneous job-firm characteristics. This allows us to account for sorting of the best workers into the best jobs, and hysteresis of contracts along workers' careers. The dual nature of the Spanish labor market together with our rich dataset provide a unique setting to investigate how experience accumulated in alternative contracts shapes individual wage profiles.

We document lower returns to accumulated experience under fixed-term contracts relative to open-ended contracts. We find that, after accounting for observed match components and unobserved worker heterogeneity, one additional year of accumulated experience in permanent employment is, on average, associated with 18.5% higher returns compared to one extra year of experience in fixed-term contracts. We provide evidence that the estimated gap in returns is neither due to differences in unobserved match quality nor firms' unobserved heterogeneity: accounting for firm-specific unobserved wage differences explains up to 15% of the gap, while removing match-specific components results in a larger gap.

Our analysis suggests that the observed difference in returns is, instead, related to worse human capital accumulation under fixed-term contracts. First, we show that the gap in returns prevails among workers who switch jobs, suggesting a human capital channel since for these workers there is a clear dissociation between the job where experience is acquired and the job where it is valued. Second, we find that the gap in returns persists when workers move to jobs with similar skill requirements, while it vanishes when they move to jobs where prior accumulated skills are less portable.

Differences in returns to contract-specific experience are positively correlated with observed and unobserved individual ability, suggesting complementarity between workers' skills and learning opportunities. These results have important implications for life-cycle wage profiles: low-ability individuals do not suffer significant wage losses whereas high-ability workers are the most penalized. Comparing counterfactual wage trajectories in fixed-term and open-ended contracts reveals that workers at top of the ability distribution (90th percentile) may face up to 16 percentage points lower wage growth 15 years after entering the labor market, a loss that corresponds to a shift from the 67th to the 77th percentile of the wage growth distribution.

This paper contributes to different strands of the literature. A large literature has focused on the consequences of flexibility at the margin (coexistence of fixed-term contracts with low firing costs along with highly protected open-ended contracts) for labor market performance (Boeri, 2011; Bentolila et al., 2020). One of the dimensions analyzed is the existence of contemporaneous wage differentials between temporary and permanent workers. Most of the results point to a wage penalty for workers on fixed-term contracts (e.g., Booth et al., 2002; Mertens et al., 2007; Kahn, 2016; Laß and Wooden, 2019) though some recent evidence highlight potential wage premiums (Albanese and Gallo, 2020). We add to this literature by focusing on how past experience accrued in temporary versus permanent jobs affects current wages. Our results suggest that the costs of being employed on temporary contracts build up over the course of workers' careers, leading to a lower wage return on experience accumulated with fixed-term contracts.

A parallel literature has investigated the impact of temporary employment on workers' careers. Although empirical evidence on whether temporary employment is a stepping stone or a dead end to stable employment is mixed (Ichino et al., 2008; Filomena and Picchio, 2021), what is less controversial is that fixed-term contracts penalize workers in the long run, due to a less continuous employment path and lower wage growth (e.g., Booth et al., 2002; Amuedo-Dorantes and Serrano-Padial, 2007; Autor and Houseman, 2010; García-Pérez et al., 2019). We contribute to this literature by showing that even when workers are able to continuously work during their career, they are penalized from acquiring experience in fixed-term contracts.

Our analysis also contributes to the growing literature that links heterogeneous returns to experience to differences in learning opportunities based on firm type (Pesola, 2011; Gregory, 2020; Arellano-Bover and Saltiel, 2021), coworkers quality (Jarosch et al., 2021), or city size (de la Roca and Puga, 2017). We show that one-the-job learning under alternative contractual arrangements also leads to heterogeneous wage-experience profiles.

We also complement the existing literature on human capital accumulation and skill transferability. Existing studies have looked at the portability of skills across industries (Neal, 1995; Sullivan, 2010), occupations (Kambourov and Manovskii, 2009; Robinson, 2018), locations (Jara-Figueroa et al., 2018), firms (Lazear, 2009), tasks (Gibbons and Waldman, 2004), or more generally across jobs (Gathmann and Schönberg, 2010). We contribute to this line of work by linking the acquisition of skills in fixed-term and open-ended contracts to their portability. We show that differences in learning opportunities between contracts generate wage penalties when workers move to jobs where their skills are transferable and could be compensated.

Finally, our paper relates to the literature that studies the consequences of flexible labor practices, such as zero-hours contracts (Dolado et al., 2021), informal contracts (Ponczek and Ulyssea, 2021), or dependent self-employment contracts (Roman et al., 2011). Our results points to lower human capital accumulation in fixed-term contracts, a channel for negative labor market performance that potentially extends to other short-term flexible work arrangements.

The remainder of the paper proceeds as follows. Section 2 characterizes the Spanish labor market, whereas Section 3 presents the conceptual framework behind our reduced-form analysis. Section 4 describes the data. Section 5 introduces our econometric approach and discusses the results on contract-specific returns to experience. Section 6 explores the human capital channel behind our results, and Section 7 documents the implications for wage trajectories. Section 8 concludes.

2 The Spanish Dual Labor Market

The Spanish labor market is characterized by a strong segmentation between workers in open-ended contracts (OECs) and fixed-term contracts (FTCs): 90 percent of monthly hires are on FTCs and nearly a quarter of the labor force is on temporary employment (Felgueroso et al., 2018). The existing duality in the labor market is attributable to the large difference in employment protection legislation introduced with the 1984 labor market reform, which liberalized the use of temporary contracts. The main objective of that reform was to promote flexibility and stimulate job creation in a rigid labor market with high unemployment (Bentolila et al., 2008; García-Pérez et al., 2019). The most relevant aspects of the reform were that (i) it eliminated the requirement for the activity associated with a fixed-term contract be of a temporary nature, (ii) it reduced the firing costs for this type of contract, and (iii) it did not alter the high degree of employment protection of permanent contracts.

This “two-tier” reform led to almost all new hires being conducted under temporary contracts, improving job creation without properly addressing high unemployment (Dolado et al., 2002; Bentolila et al., 2012). The spike in the use of temporary contracts led the Spanish authorities to adopt several compensatory reforms in 1994, 1997, 2001, 2006, 2010 and 2012, all of which proved mostly unsuccessful in reducing labor market duality (Bentolila et al., 2008; Conde-Ruiz et al., 2010; García-Pérez and Domenech, 2019).[1] The 2012 reform was the most profound: it substantially reduced employment protection for permanent workers and it made easier for firms to implement internal flexibility measures (OECD, 2013). The broad scope of the reform had certain effects on worker

[1]Most of these reforms sought to address the duality of the labor market by discouraging the use of temporary contracts, either by increasing social security contributions, or by limiting cases where employers could resort to fixed-term contracts, introducing social security bonuses into permanent contracts, or lowering firing costs for targeted groups.

mobility (García-Pérez and Domenech, 2019; Garcia-Louzao, 2022), but its main objective of reducing labor market duality was limited (Felgueroso et al., 2018; García-Pérez and Domenech, 2019; Conde-Ruiz et al., 2019).

The consequence of labor market segmentation on workers' labor market outcomes are broad. Existing evidence suggests that workers in FTCs experience higher turnover rates with larger incidence of unemployment but short unemployment spells (Amuedo-Dorantes and Serrano-Padial, 2007; Barceló and Villanueva, 2016). Workers in temporary contracts are also less likely to receive formal training (Alba-Ramirez, 1994; Dolado et al., 2000; Cabrales et al., 2017). In addition, low conversion rates into OECs leads workers to rotate between different temporary contracts and different companies (Amuedo-Dorantes, 2000; Güell and Petrongolo, 2007; Rebollo-Sanz, 2011). As a result, a relevant share of workers end up trapped in temporary employment (Gorjón et al., 2021). The evidence also indicates that temporary workers suffer a wage penalty relative to workers in permanent positions (Toharia and Jimeno, 1993; Bentolila and Dolado, 1994; de la Rica, 2004; Bonhomme and Hospido, 2017) and have lower wage growth (Amuedo-Dorantes and Serrano-Padial, 2007). Contemporaneous wage gaps and less stable careers translate into long-run earning losses (García-Pérez et al., 2019).

The evidence suggests that labor market duality might penalize workers, either because of foregone experience or because the experience accumulated is of poorer quality. The Spanish institutional setting offers a unique case study for understanding how experience accumulated under different contractual arrangements affect current wages.

3 Earnings Trajectories in a Dual Labor Market

In this section we lay out a parsimonious framework linking labor market duality to on-the-job human capital accumulation and wages. The model adapts the framework of Arellano-Bover and Saltiel (2021) to a setting with dual labor market and two types of contracts, fixed-term and open-ended.[2] We use this framework to derive our main earnings equation.

[2]Our analysis complements that of Arellano-Bover and Saltiel (2021): they study firm-specific experience, we focus on contract-specific experience. Returns to contract-specific experience may vary within firms, since similar firms might combine fixed-term and open-ended contracts differently.

Human Capital. Consider an individual i in period t. We define the stock of human capital for this individual as

$$H_{it} = \eta_i + h_{it} \tag{1}$$

where η_i is the human capital developed before labor market entry (innate ability, and education level), assumed to be fixed over time, while h_{it} is the stock of human capital accumulated since labor market entry up to period t.

Human capital, h_{it}, is acquired on the job and varies according to the types of contract worker i has been employed up to time t. Formally, skill acquisition between two consecutive periods is governed by the following law of motion

$$h_{it+1} = h_{it} + \mu_{it}^c \tag{2}$$

where c denotes the type of contract, fixed-term vs open-ended, and μ_{it}^c is an i.i.d. draw from a contract-specific distribution F^c, such that $\mathbf{E}[\mu_{it}^c] = \gamma^c$. Differences in human capital accumulation between workers with FTCs and OECs are governed by differences in the distributions F^c. For example, companies may be less willing to invest in people employed on temporary contracts due to the potential finite nature of the labor relationship (Crawford, 1988; Poulissen et al., 2021), which translates into worse skill acquisition for workers during episodes of temporary employment.[3] Workers in fixed-term contracts may also be less willing to make an effort to learn on the job if the likelihood of contract conversion is low (Sanchez and Toharia, 2000; Dolado et al., 2016).[4] In absence of differences in skill acquisition among workers employed under different type of contracts, human capital accumulation would depend exclusively on the total experience acquired on the job (Mincer, 1974). In our stylized framework the current stock of human capital accumulated since labor market entry depends on the entire employment history across different contracts:

$$h_{it} = \sum_{k=1}^{t-1} \mu_{ik}^{c(i,k)} \tag{3}$$

and

$$\mathbf{E}[h_{it}|\mathbf{oec}_{it}, \mathbf{ftc}_{it}] = \sum_{k=1}^{t-1} \sum_{m \in \{\text{ftc,oec}\}} \mathbf{1}[c(i,k) = m]\gamma^m \tag{4}$$

[3]Ferreira et al. (2018) show that although workers on temporary contracts are less likely to receive *formal* training, they participate more actively in informal learning than their peers in permanent contracts. This higher commitment to informal training is especially significant at the beginning of their careers to secure a permanent contract.

[4]Engellandt and Riphahn (2005) document for Switzerland that workers in temporary positions with significant "upward mobility" potential are more likely to exert effort.

where $\mathbf{oec}_{it}$ and $\mathbf{ftc}_{it}$ are the complete histories in open-ended and fixed-term contracts since labor market entry up to time t, while $\mathbf{1}[c(i,k) = m]$ is an indicator function equal to one if worker i was employed under a FTC or OEC in period k.

Earnings. The structure of (log) earnings of worker i at period t is governed by the following process

$$\ln w_{it} = H_{it} + X_{it}\Omega \tag{5}$$

where X_{it} includes contemporaneous job and firm characteristics. Substituting our definition of H_{it}, the expected log earnings can be re-written as follows:

$$\mathbf{E}[\ln w_{it}|i, X_{it}, \mathbf{oec}_{it}, \mathbf{ftc}_{it}] = \eta_i + \gamma^{\text{oec}}\text{oec}_{it} + \gamma^{\text{ftc}}\text{ftc}_{it} + X_{it}\Omega \tag{6}$$

where oec_{it} and ftc_{it} are measures of accumulated experience under open-ended and fixed-term contracts since labor market entry up until time t, defined respectively as

$$\text{ftc}_{it} = \sum_{k=1}^{t-1} \mathbf{1}[c_{c(i,k)} = \text{ftc}] \qquad \text{and} \qquad \text{oec}_{it} = \sum_{k=1}^{t-1} \mathbf{1}[c_{c(i,k)} = \text{oec}]$$

The sum of oec_{it} and ftc_{it} represents the standard experience component in a Mincer regression, which does not differentiate returns across contracts.

Ultimately, it is an empirical question whether we find any difference in the returns to experience accumulated on different contracts. This is what we explore in the remainder of the paper.

4 Data

Social Security Records. Our analysis is based on the Spanish Continuous Sample of Employment Histories (*Muestra Continua de Vidas Laborales* or MCVL), an administrative dataset collected annually by the Spanish Social Security administration and linked to the Residents' Registry and Tax Records since 2005.[5] The MCVL is a representative 4 percent random sample of individuals who had any relationship with the Social Security at any time in the reference year.[6] The data set has a longitudinal design, since any individual who is present in a given year and stays registered with the Social Security

[5]The first version of the MCVL corresponds to 2004. This wave is disregarded because most of the structure of the information differs from that available for the following years.

[6]This includes employed and self-employed workers, recipients of unemployment benefits and pension earners, but excludes individuals registered only as medical care recipients, or those with a different social assistance system.

administration remains a member of the sample.[7] The MCVL is refreshed each year, remaining representative of both the stock and flows of individuals.

For each sample member, the MCVL retrieves *all* relationships with the Social Security since the date of the first job spell, or 1967 in the case of those who entered earlier. All spells are followed from their start up to their end, or to the 31st of the December of the last reference year. This unique feature allows us to track individuals over time and calculate the exact number of days worked since labor market entry. For each employment episode, we observe detailed information on the labor relationship including part-time status, occupation category, type of contract (with reliable information since 1997), employer identifier, workplace location, sector of activity, and labor income.[8] Importantly, for each worker in the dataset, we observe all employers she has worked for since entering the labor market. However, given the nature of the data, we do not observe all workers in a given firm. Demographic information is also reported, e.g. age, gender, education, nationality, and household composition.[9]

Analysis Sample. We use the 2005-2018 MCVL original files to select our estimation sample. For each individual in the dataset, we define labor market entry as the (education-specific) predicted year of graduation (see Appendix C).[10] We focus on individuals who entered the labor market after 1996 to be able to track days worked under alternative job contracts. We exclude from the sample all foreigners because we do not have information on any previous work experience abroad, so we cannot compute their complete labor market history. Similarly, we remove individuals whose first employment observations is more than 5 years after labor market entry. We further restrict the sample to employees in the General Regime of the Social Security, thereby excluding employment episodes in special regimes such as agriculture, fishing, mining, or household activities as well as

[7] Persons who stop working remain in the sample as long as they receive unemployment benefits or other social benefits (e.g., retirement pension), but leave the sample when they die or leave the country permanently.

[8] Information on labor income comes from Social Security contribution bases which are top-coded. We correct the upper tail of the wage distribution by fitting cell-by-cell Tobit models to log daily wages. Appendix B provides a detailed discussion on the correction method and offers a comparison between original and corrected wage distributions.

[9] Appendix C provides a detailed description of the variables.

[10] We rely on the predicted graduation year to define labor market entry, since we only observe workers from the moment they start contributing to Social Security. Thus, the predicted graduation year allows us to define a specific moment from which we start following workers belonging to the same cohort.

self-employment.[11] From this sample of job spells, we construct an individual-year panel to study individual wages up to the first 15 years after predicted graduation. These restrictions yield a final sample of 242,774 individuals observed over a total of 1,954,097 employment (worker-year) observations between 1997 and 2018.

Table A.1 in Appendix A reports descriptive statistics. In our sample, workers are, on average, 22 years old during their first work experience. About 54% of these workers are women and approximately 37% have a university degree. During their first year of employment after graduation, they work for about 190 days, 80% of which under FTCs. In terms of long-term outcomes, the average worker in our sample is observed for about 10 years, during which she actually worked around 6. Over this period of her career, she had an average yearly wage growth equal to 6.5%. The incidence of permanent and temporary contracts is almost evenly distributed: 45% of the time the worker held temporary jobs, while the rest was under OECs.[12] Strikingly, only 9% of workers have never had a job with a temporary contract. Even when considering workers whose first job was with an OEC (Column 3), 44% of them hold at least one temporary job at some point in their career.

Figure A.2 shows that while the incidence of FTCs decreases with actual labor market experience, there is still 8% of the workers who never held a permanent position after having accumulated 9 years of actual experience. The large incidence of temporary employment on workers' careers can be understood if one takes into account the widespread use of FTCs across sectors, occupations or regions (see Figures A.3 to A.5). Figure A.3 shows the temporary contract rates by sector. The incidence of temporary employment is above 15% for all sectors, reaching 40% in construction and primary activities. Similarly, Figure A.4 reveals that the use of FTCs is mostly prevalent among low-skilled occupations, with a rate above 50%, although it is substantial among high-skilled occupations as well, with an incidence ranging from 10% to 20%.[13]

[11]If an individual has more than one labor relationship with different employers, we keep only the main employer defined as the one reporting the highest annual earnings. Similarly, if an individual hold more than one contract with the same employer within a year, we select the job characteristics coming from the last job contract observed in that year. However, to compute our measures of experience we count days worked under a given type of contract each year with *all* the employers.

[12]Similar shares of time employed under FTCs relative to OECs are achieved by accumulating a higher number of fixed-term contracts, which are average much shorter (296 days, as opposed to 1,274 days for OECs), as suggested by the distribution of contract duration in Figure A.1.

[13]Using an alternative dataset, in Appendix D, Figure D.1, we provide evidence that the use of FTCs is also widespread among employer classes defined by age or size categories, or firm fixed effects. This evidence aligns with recent work by Pijoan-Mas and Roldan-Blanco (2022) on the use of FTCs by firms in dual labor markets.

5 Returns to Experience in a Dual Labor Market

5.1 Econometric Model

The stylized framework in Section 3 provides us with a flexible specification to estimate the returns to experience under different types of contract. To this purpose, we adapt equation (6) and estimate a linear panel data model for the logarithm of real daily wages of individual i and year t

$$\ln w_{it} = \eta_i + \sum_{c \in \{\text{ftc},\text{oec}\}} \gamma^c \text{c}_{it} + X_{it}\Omega + \delta_{e(it)} + \delta_t + \epsilon_{it} \tag{7}$$

where η_i stands for pre-labor market permanent individual ability while oec_{it} and ftc_{it} denote the amount of experience accumulated up to time t by worker i under open-ended and fixed-term contracts since labor market entry.[14] Experience is measured in days and then converted into years. X_{it} refers to contemporaneous job-firm characteristics (tenure, type of contract, part-time status, skill level, plant size and age, location, and sector of activity), whereas $\delta_{e(it)}$ and δ_t are potential experience of workers i at year t and year fixed effects, respectively.[15] The inclusion of potential experience together with contemporaneous job-firm characteristics ensures that differences in the returns to accumulated experience can only be driven by heterogeneous past histories in the labor market. Individual fixed effects are intended to account for the sorting of workers based on unobserved permanent heterogeneity. Under the assumption that ϵ_{it} is an i.i.d. random term, consistent estimates can be obtained by applying the standard panel fixed effects estimator.

5.2 Dual Returns to Experience

We start our discussion by looking at the returns to experience estimated in equation (7). To ease the interpretation, we also estimate returns to overall experience using a standard Mincerian equation and compare it to the estimates of returns by type of contract, controlling for individual unobserved heterogeneity. All of our specifications include contemporaneous job-firm characteristics, including current type of contract. This

[14] For simplicity, we include experience linearly but we also estimate equation (7) using a step-wise specification for contract-specific returns to experience. See Figure A.6 in the Appendix.

[15] We include fixed effects (3-year length groups) for potential experience, i.e., years since entry into the labor market, rather than age effects because some age groups are only identified by the less educated individuals. For example, college graduates are not observed before reaching the age of 24. In addition, accounting for potential experience effects ensures that we are comparing individuals at the same point in their careers.

is key because it allows us to take into account the hysteresis of contracts along workers' careers (Gorjón et al., 2021). Our results are reported in Table 1. For comparison, we also present estimates from a version of this model without individual fixed effects, including education and gender indicators to control for differences in pre-labor market human capital.

Table 1: Dual Returns to Experience

	OLS		Fixed-Effects	
	(1)	(2)	(3)	(4)
Current FTC	-0.0463***	-0.0320***	-0.0327***	-0.0359***
	(0.0011)	(0.0010)	(0.0009)	(0.0009)
Experience	0.0294***		0.0497***	
	(0.0003)		(0.0005)	
Experience OEC		0.0351***		0.0500***
		(0.0003)		(0.0005)
Experience FTC		0.0209***		0.0421***
		(0.0004)		(0.0006)
Gap in Returns (%)		68.31***		18.52***
		(2.74)		(1.05)
Observations	1,954,097	1,954,097	1,954,097	1,954,097
R-squared	0.6330	0.6343	0.3057	0.3064

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications include controls for a quadratic polynomial in tenure, type of contract, a dummy for part-time jobs, indicators for occupation-skill category (2), sector of activity (10), workplace location (50), small and medium enterprises (plant size $<$ 50), young organizations (plant age $<$ 10), potential experience dummies (5), and year dummies (22). OLS regressions include additional controls for education and gender. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}}{\gamma^{ftc}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported in Columns (3) and (4) is within workers.

We find each additional year of experience raises individual wages by 2.9%, or by 4.9% if both contemporaneous job-firm characteristics and individual heterogeneity are taken into account. Moreover, we show that workers currently employed under a temporary contract suffer a wage penalty of about 3.3% (Column 3). Of primary interest, the returns to experience vary depending on whether such experience was accumulated under fixed-term or open-ended contracts. One additional year of experience in OECs is associated with wage gains of 3.5%, while returns are 1.5 percentage points (pp) lower for experience accumulated in FTCs. The difference is reduced once unobserved individual heterogeneity is factored in ($\sim$0.8pp), suggesting that sorting of workers into contractual arrangements (and other *observed* match components) is important, but does not fully explain the gap in returns. This gap corresponds to a 18.5% higher yearly return from accumulating one year more of experience in OECs relative to FTCs.[16] To the extent that the relationship between current wages and past experience reveals workers' past on-the-job learning opportunities, our results are indicative of lower skill accumulation under FTCs.

In Appendix A we perform a detailed sensitivity analysis that confirms our results. First, we show that our findings are robust to alternative measures of labor income (Table A.3) and to alternative controls used to account for exogenous life-cycle wage differences (Table A.4). Second, the protection gap between FTCs and OECs decreased substantially after the 2012 reform. As discussed in Dolado et al. (2016), reforms with that goal (i.e., reducing the EPL gap) could have led to (i) more conversion rates from temporary to permanent and (ii) more on-the-job training to temporary workers, which in turn might have increased their productivity and wages. The gap in returns is still present when we allow contract-specific returns to vary after 2012 and, if anything, becomes larger (see Table A.5). Alternatively, we estimate our baseline model using only the oldest cohorts: those who graduated between 1996 and 1999. One would expect a minimal impact of the reform on the returns to experience for this group of workers, since it occurred at a late stage of their careers. The results in Table A.6 confirm this intuition.

Our findings are also robust to allowing returns to tenure to be contract-specific, which control for seniority-based wage floors set by collective bargaining agreement (Table A.7), and extend to the samples of only men and only women with similar magnitudes (Table A.8). Finally, we model contract-specific returns to experience non-parametrically using 22-step functions for each type of experience.[17] This specification reveals that, although returns increase monotonically for both types of experience, the gap between these returns is highly non-linear (see Figure A.6).

5.3 Differences in Experience Levels

Workers under temporary employment may face more job interruptions than individuals employed in permanent positions. Non-working episodes could result into lower experience levels and, potentially, lower human capital overall.[18] This could affect how returns

[16]The gap in contract-specific returns to experience is twice as large as the differences in annual returns to experience between men and women (9.2%) and as large as that between education groups (18.7%). See Table A.2 in in Appendix A.

[17]We choose as many bins to have a sufficient and balanced number of observations within each cell.

[18]Notice that, in our context, each year of non-employment implies a year of lost experience. However, this is not necessarily the case for human capital, if workers engage in some form of retraining while

are estimated and explain the non-linearity of the estimated gap discussed above.

To investigate this issue, we adapt our benchmark model and compare individuals with the same level of total experience but heterogeneous incidence of temporary employment in their career. First, we discretize our measure of overall actual experience into Q-bins, where $q = \{\{0\}, (0,4], (4,7], (7,10], (10,15], ..., (90,95], (95,97], (97,100]\}$ denote brackets of percentiles in the distribution of actual experience every year. Second, we estimate the following regression model

$$\ln w_{it} = \eta_i + \sum_{m=1}^{3} \sum_{q=0}^{Q} \beta_{m(q)} \mathbb{1}\{\text{exp}_{it} = q\} \times \mathbb{1}\{\text{ftc}_{it} = m\} + X_{it}\Omega + \delta_{e(it)} + \delta_t + \epsilon_{it} \qquad (8)$$

where $\mathbb{1}\{exp_{it} = q\}$ takes value one if worker i falls into the qth-bin of actual experience in period t. For example, $q = (0,4]$ identifies workers within the 1st and the 4th percentile of the actual experience distribution in a given year. We then interact this variable with an indicator for the incidence of temporary employment during their career.

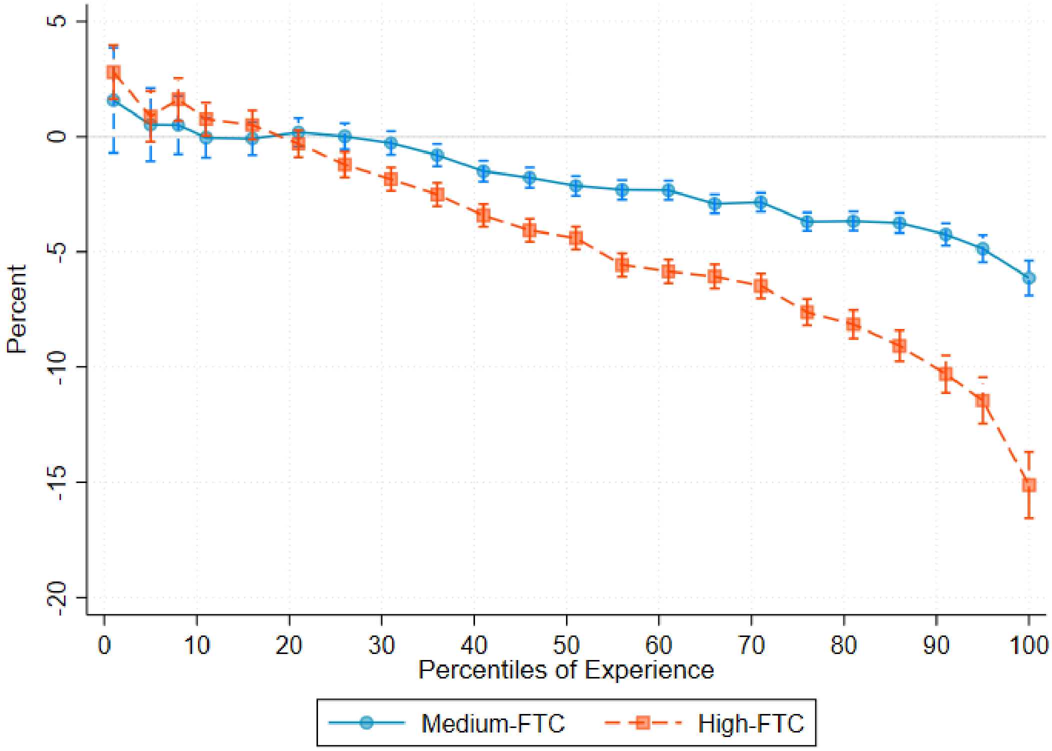
More precisely, we create three groups of workers based on the ratio of experience under FTCs to overall experience: low (ratio lower than 0.3), medium (between 0.3 and 0.9) and high incidence (above 0.9). Thus, $\mathbb{1}\{\text{ftc}_{it} = m\}$ is an indicator variable identifying workers in a given group according to the incidence of temporary employment since labor market entry up to time t. Notice that the parameters $\beta_{m(q)}$ are only identified up to a normalization. We impose the impact of accumulated experience on individuals wages to be zero for the first observation, when experience in the labor market is equal to none. This implies that $\beta_{m(0)}$ is equal to zero for each of the three m-groups, thereby estimating Q parameters overall for each m-group. The point estimates $\beta_{2(q)}$ and $\beta_{3(q)}$ capture the wage gap between individuals who have been employed for the same amount of time since labor market entry but have had a higher incidence of temporary employment in the past.

Figure 1 plots $\beta_{2(q)}$ and $\beta_{3(q)}$ parameters from equation (8). The estimates reveal several interesting patters.[19] First, we do not find a negative impact of higher incidence of temporary employment among low experienced individuals. Second, relative wage losses become apparent from the fortieth percentile of overall experience distribution. Third, the greater the acquisition of experience in FTCs, the greater the losses. Finally, highly experienced individuals face wage losses of up to 15% due to higher incidence of

not employed. In our analysis, we abstract from this dimension and focus only on the human capital accumulated on the job.

[19]The results are robust to alternative definitions of temporary employment incidence (see Figure A.7).

Figure 1: Dual Returns to Experience: Incidence of Temporary Employment



Notes: Estimates (×100) and 95% confidence intervals of $\beta_{2(q)}$ and $\beta_{3(q)}$ from equation (8). Standard errors are clustered at the individual level. Medium-FTC (High-FTC) incidence refers to individuals whose actual experience on a temporary contract relative to overall actual experience is between 0.3 and 0.9 (above 0.9).

FTC in the past. Taken together, our results suggest that workers are penalized from accumulating experience under FTCs compared to OECs, even if they manage to gain the same level of experience.[20]

5.4 Firm Heterogeneity and Match Quality

Individual wages are determined by who the worker is, but also by the firm where she works and the success of the idiosyncratic job match. In our setting, individuals with the same level of experience and innate ability who hold jobs with the similar observable characteristics might still receive different wages due to unobserved heterogeneity across firms or match quality. Therefore, the omission of either component could result into

[20]In Appendix A, Table A.9, we document that workers who spent their entire career in FTCs with no interruptions have a 10 percent lower daily wage compared similar workers whose career was fully developed in OECs (Column 5). As we progressively expand the sample to include workers who have worked less than 100% of their potential experience (Columns 1 to 4), the wage penalty increases. This could be related to the intermittency of non-employment spells between employment spells, leading to human capital depreciation or job discrimination. However, comparison across estimates suggests that employment interruptions, while relevant, can only explain up to 50% of the overall wage penalty associated with higher FTC experience.

biased estimates for the gap in returns to experience. In this sub-section, we provide evidence on the relevance of both sources of potential bias.

Firm Heterogeneity. A substantial amount of the literature emphasizes the relevance of firms in wage determination (see Card et al., 2018, for a recent review on the role of firms in the labor market). Because of skill complementarity and job shopping, high-ability and more experienced workers are more likely to be employed in high-paying firms. Moreover, if individuals with longer working history in permanent contract were also more likely to match with high-paying firm (for instance, because of better skill signaling), one would expect an even larger bias in the estimates for the returns to experience in open-ended contracts. Hence, ignoring the sorting of workers across firms could threaten the correct identification of the gap in returns.

To investigate the relevance of this margin, we conduct the following exercise. First, we create an annual panel of employment observations that includes *all* workers observed between 1997 and 2018 in the dataset. Second, from this panel, we select only firms for which we observe at least 10 workers each year during the period of interest.[21] Third, we fit linear wage models that include additive person and establishment fixed effects as in Abowd et al. (1999) (AKM, henceforth), further controlling for workers' part-time status and time effects in the form of genuine year and age dummies. Fourth, we recover the firm fixed effects from the estimation and match them with our baseline sample. Finally, we use the estimated firm fixed effects as an additional control in our estimation using the matched sample. This exercise allows us to provide suggestive evidence about the role of pay differences across firms in explaining the gap in returns.

Column 3 of Table 2 reports the results of this exercise. Standard errors (in parenthesis) are bootstrapped with 100 replications. For comparison, we also present our benchmark results in Column 1, and the estimation results of our benchmark model on the matched sample in Column 2. Notice that returns to experience identified in the matched sample are higher compared to the benchmark sample.[22] This is particularly true for the return to experience in open-ended contracts, which generates a larger gap.

[21] Recall that, in our data, we do not observe all workers in a given firm. Therefore, we select firms in which we observe several workers in order to be able to identify firm-specific pay components.

[22] Table A.10 reports the estimates of a linear probability model for the workers' likelihood to be in the matched sample. Workers with college education, in high-skill occupations, longer actual experience and longer tenure are more likely to be in the matched sample, as well as to have higher wage and higher experience in OECs. Interestingly, workers under FTCs are also more likely to be in the matched sample, as it is more likely to workers from firms that rely more intensively on temporary employees.

Table 2: Dual Returns to Experience: Firm Heterogeneity

	Baseline Sample	Matched Sample	
	(1)	(2)	(3)
Experience OEC	0.0500***	0.0575***	0.0541***
	(0.0005)	(0.0011)	(0.0009)
Experience FTC	0.0421***	0.0440***	0.0431***
	(0.0006)	(0.0013)	(0.0011)
Gap in Returns (%)	18.52***	30.50***	25.71***
	(1.05)	(2.21)	(1.83)
Observations	1,954,097	456,364	456,364
No. Workers	242,774	99,714	99,714
R-squared	0.3064	0.2372	0.3067
Estimated firm FE	No	No	Yes

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. Estimated firm fixed effects (FE) are recovered from a standard AKM model using *all* workers in the MCVL employed by firms for which we observe at least 10 workers each year between 1997-2018. Column (1) replicates our benchmark specification in Table 1 Column (4). Columns (2) and (3) estimate our benchmark model in a restricted sample for which we can match the estimated *out-of-sample* firm FE. All specifications include the same set of controls as the fixed effect panel data model estimates in Column (4) in Table 1. Standard errors (in parenthesis) are clustered at the individual level and, in Column (3), are bootstrapped with 100 replications. Gap in returns is computed as $100\times(\frac{\gamma^{oec}}{\gamma^{ftc}}-1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers.

Despite these differences, we can still learn about the bias that could arise when the role of firm heterogeneity is neglected by comparing results in the matched sample with and without including the estimated firm fixed effects. The comparison between Columns 2 and 3 indicates that accounting for firm fixed effects reduces the gap in returns to experience by roughly 15% ($\sim$5pp). To the extent that the magnitude of the bias was the same in our baseline sample, a back-of-the-envelope calculation suggests that the identified gap in returns would drop from 18.5% to 15.7% if differences in firm-specific pay components were taken into account. Therefore, this result suggests that firm heterogeneity, while important, can only explain a limited part of the estimated gap in returns.[23]

[23] AKM estimates may suffer from the incidental parameter problem, often referred to as limited mobility bias (Bonhomme et al., 2022a). This bias can emerge due to the large number of firm-specific parameters that are solely identified from workers who move across firms. In Appendix A, we apply a clustering algorithm following Bonhomme et al. (2022b) to classify firm types in order to address this potential bias. Table A.11 reports the estimation outcomes for alternative firm clustering fixed

Table 3: Dual Returns to Experience: Match Quality

	Altonji and Shakotko (1987)		(1) & (2) Subsidies availability	
	(1)	(2)	(3)	(4)
Experience OEC	0.0435***	0.0462***	0.0434***	0.0474***
	(0.0009)	(0.0035)	(0.0009)	(0.0035)
Experience FTC	0.0345***	0.0297***	0.0345***	0.0311***
	(0.0012)	(0.0038)	(0.0011)	(0.0038)
Gap in Returns (%)	26.14***	55.40***	26.08***	52.73***
	(1.96)	(8.46)	(1.95)	(7.70)
Observations	1,954,097	1,954,097	1,954,097	1,954,097
R-squared	0.4789	0.4784	0.4789	0.4784

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications include individual FE plus the same set of controls as Column (4) in Table 1. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}}{\gamma^{ftc}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers.

Match Quality. Although it can be argued that worker fixed effects largely capture employee's underlying ability or productivity, this might not be necessarily the case for firm fixed effects: high-productivity firms are not always high-paying firms, and large variation in wages is instead explained by match quality (Woodcock, 2015). Omitting match effects could therefore bias the estimated returns to experience (Altonji and Shakotko, 1987; Topel, 1991; Moscarini, 2005). More precisely, unobserved match quality is likely to be positively correlated with experience, since more experienced workers have had more time to locate themselves into good matches. Likewise, one could expect the unobserved match quality to be correlated with the tenure variables, since a worker employed in a good match is at the same time more likely to be receiving high wages and more likely to keep that job longer. Importantly, in our context, the strength of these correlations may vary between OEC and FTC experience, affecting the identified gap in returns.

To examine the role of match quality, we adopt a traditional approach in the literature proposed by Altonji and Shakotko (1987) and used, among the others, by Kambourov and Manovskii (2009). This procedure consists of instrumenting experience and tenure with

effects. The results are broadly consistent with the AKM estimates, although they attribute slightly less explanatory power to unobserved firm heterogeneity. Hence the AKM estimates could be understood as an upper bound for the role of unobserved firm heterogeneity.

their deviations with respect to the average computed within contract and match history of each worker. By construction these instruments are orthogonal to match-contract unobserved components that are time-invariant.[24] However, they do not address the possible correlation between the experience variables and (i) the unobserved contract-specific *time-varying* component or (ii) the unobserved *non-own* contract-specific components (Kambourov and Manovskii, 2009). Moreover, the assignment of workers across OECs and FTCs might still be non-random, and selection into contracts could lead to accumulation of contract-specific experience over workers career which depends on unobserved factors. To mitigate these concerns and address potential (unobserved) incentives that companies may have to create job matches using FTCs or OECs, we extend the IV strategy to include an additional instrument based on regional variation in the availability of subsidies for hiring workers under OECs.[25]

The validity of the subsidy instrument is based on two major identifying assumptions. First, subsidy availability cannot be correlated with wages beyond experience accumulation in OECs and/or FTCs. One of the main threats to this assumption is the spatial correlation between subsidy availability and the stock of FTCs and OECs induced by the distribution of unobserved worker quality or unobserved firm characteristics across regions. Controlling for unobserved worker heterogeneity together with a dummy variable for the current contract in the wage equation should alleviate the first concern, while the second concern is less likely to be empirically relevant given the widespread use of FTCs across firm types (Pijoan-Mas and Roldan-Blanco, 2022). The second identification assumption is that the composition of the eligible and ineligible worker groups should remain stable over time. As discussed in García-Pérez and Rebollo-Sanz (2009) this is unlikely to be a problem, since our study includes regions where the policy remains unchanged over the course of several years.

Table 3 presents the estimates of the contract-specific returns to experience once match effects are removed. In Column 1, we de-mean experience at the contract-individual level to control for heterogeneity in match quality across contracts. Alternatively, in Column 2, we de-mean the experience measures at the match-contract-individual level, which allows to account for heterogeneity within contracts. Columns 3 and 4 report the estimates for

[24]See Altonji and Shakotko (1987) for a derivation of this result. See Light and McGarry (1998) and Booth et al. (2002) for a discussion.

[25]See García-Pérez and Rebollo-Sanz (2009) for a detailed description of the regional subsidies in Spain, and Barceló and Villanueva (2016) or Nieto Castro (2018) for applications of the same instrumental variable.

the two strategies above combined with the additional instrument based on availability of subsidies for hiring under OEC.[26] In line with the existing literature, the results highlight that the omission of matching effects generates an upward bias in the estimated returns to experience. However, what is most relevant for our analysis is that the estimated gap in returns prevails and, if anything, widens.

Our results indicate, therefore, that the omission of firm- and match-specific effects may bias the estimated returns to experience. However, the ultimate impact of any of these components on contract-specific returns implies that the gap in returns is still present when firm-specific wage components or the quality of the job match are taken into account. Given the nature of biases, we take a conservative stance and consider the FE estimates in Table 1 as our preferred estimates, which are likely to represent a lower bound for the gap in returns.

6 Human Capital Channel

In this section, we analyze the link between human capital and our results. First, we show that the difference in contract-specific returns may be associated with differences in human capital accumulation between temporary and permanent jobs. In addition, we document that lower human capital accumulation in FTCs primarily affects high-skilled workers, suggesting complementarity between skills and learning opportunities across contracts.

6.1 Portability of Skills

To shed light on whether the gap in returns is driven by differential skill accumulation by contract, we examine the first re-employment observation of workers who switched jobs in our sample. In this way, we can dissociate jobs where experience has been accumulated from jobs where that experience is being valued, detached from the effects of tenure.

Column 1 in Table 4 reports fixed effect estimates of equation (7) for the sample of job switchers. Column 2 reports the estimates obtained using a two-stage Heckman correction model, where we use household composition of workers as an exclusion restriction to estimate the probability of job switching and to correct the wage equation from selection

[26] Estimates of the first stage regression and various F-statistics to test the strength of the instruments are reported in Appendix A, Tables A.12-A.15.

bias.[27] The results are aligned with our baseline estimates: returns to experience acquired under OECs are roughly 23% higher relative to FTCs, suggesting lower skill acquisition during temporary employment.

If the returns to contract-specific experience were linked to different human capital accumulation across contracts, we should observe the gap in returns to persist whenever workers move to jobs where previous experience can be transferred, and is therefore valuable. Instead, we should observe the difference in returns to disappear whenever a worker moves to a job where previous experience is not transferable.

We examine this hypothesis by comparing workers who switch jobs between and within industries.[28] Columns 3 and 6 in Table 4 report the standard fixed effect estimates. Columns 4 and 7 report the estimates obtained using a two-stage Heckman correction model that corrects for job switching (same as Column 2), while Columns 5 and 8 refine these estimates by simultaneously correcting for job *and* industry switching.[29]

The findings confirm that returns to experience accumulated in OECs are higher relative to FTCs, but only for workers switching jobs within the same industry. The gap is about 1.6pp (see Column 4) and corresponds to a 47% higher return to experience acquired in OECs relative to FTC. Workers who switched jobs *and* industries face a much smaller gap in returns, 0.4pp (see Column 6), which is approximately one fourth of that faced by those who remain in the same industries. The gap persists among those who stay in the same industry after controlling for selection into the sample of job switchers (Columns 5 and 6), while it disappears among those who switch both jobs and industry (Columns 7 and 8). These results confirm that poorer learning opportunities arise under FTCs. These findings also mitigate concerns related to rent-sharing or pass-through effects of firms' shocks to wages (Card et al., 2018), especially if they were larger for workers in open-ended contracts. Potentially, the gap in returns could be driven simply by the persistence of higher wages emanating from past rents. However, if this were the case, we should observe such differences regardless of the sector where workers move to.

To strengthen the idea that the gap in returns is due to differences in human capital accumulation, we relate contract-specific experience for job switchers to portability of

[27] Estimates of the first stage regression are reported in Appendix A, Table A.20.

[28] We consider 10 major sectors of activities, corresponding to primary sector, manufacturing, utilities, construction, trade and transport, accommodation and restaurants, business services, public sector, private health institutions, education, and other services. See Appendix C for details.

[29] We use past wages as an exclusion restriction for industry switching. Estimates of the first stage regression are reported in Appendix A, Table A.21.

Table 4: Dual Returns to Experience: Job Switchers

	All		Within Industries			Across Industries		
	FE	FE + Heckman	FE	FE + Heckman		FE	FE + Heckman	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Experience OEC	0.0495***	0.0447***	0.0501***	0.0457***	0.0487***	0.0435***	0.0390***	0.0379***
	(0.0008)	(0.0008)	(0.0013)	(0.0013)	(0.0013)	(0.0014)	(0.0015)	(0.0015)
Experience FTC	0.0380***	0.0364***	0.0341***	0.0326***	0.0337***	0.0392***	0.0378***	0.0376***
	(0.0010)	(0.0010)	(0.0016)	(0.0016)	(0.0016)	(0.0019)	(0.0019)	(0.0019)
Inverse Mills Ratio		0.0482***		0.0491***			0.0417***	
(job switching)		(0.0022)		(0.0034)			(0.0040)	
Inverse Mills Ratio					0.0337***			0.1027***
(industry/job switching)					(0.0050)			(0.0050)
Gap in Returns (%)	30.19***	22.91***	46.97***	40.38***	44.74***	11.01***	3.15	0.92
	(2.35)	(2.32)	(4.35)	(4.29)	(4.33)	(3.72)	(3.67)	(3.59)
Observations	447,098	447,098	235,882	235,882	235,882	211,216	211,216	211,216
R-squared	0.3197	0.3208	0.2968	0.2982	0.2971	0.3357	0.3364	0.3387

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. Heckman correction in Columns (2), (4), and (7) uses household composition for the job switching equation as exclusion restriction. Columns (5) and (8) estimate a simultaneous job-industry switching equation where the exclusion restriction for job switching is household composition, whereas past wage is used for the industry switching equation. All specifications include the same set of controls as Column (4) in Table 1 except for the polynomial in tenure. In these specifications we use only the first re-employment observation after a job change. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}}{\gamma^{ftc}} - 1)$ and standard errors are obtained using the Delta method. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The R-squared reported is within workers. Job switchers = 167,702.

skills across industries. To do so, we construct the following measure of similarity in skill-intensity between each pair of industries (k, j),

$$\text{dist}_{kj} = \sqrt{\sum_{q=1}^{3} (\text{skill-q}_k - \text{skill-q}_j)^2}$$

where skill-q_k denotes the share of workers within sector k belonging in one of the q Social Security contribution groups defined in Appendix C.[30] These groups are determined by the education level required for the specific job and by the complexity of the tasks involved in that same job. For instance, the first group includes jobs with the highest skill requirement, like engineers and senior managers. The second group includes middle-skilled jobs, like administrative workers, while the third group includes manual jobs. The higher the share of group-q workers in sector k, the higher the value of skill-q_k. The larger the differences in skill-q_k across sectors, the higher the value of dist_{kj}, and the lower skill-portability is likely to be. Notice that, by construction, $\text{dist}_{kj} = 0 \ \forall k = j$.[31]

Table 5 reports fixed effect estimates of our benchmark model extended to include our measure of skill similarity and its interaction with our contract-specific experience variables. Column 1 presents the results from the standard panel data model, while Column 2 includes the Heckman correction term for endogenous job switching.[32] Our results confirm that those who change sectors are penalized compared to those who remain in the same sector, and reveal that the penalty is greater the lower the similarities in skill content between the origin and the destination industries. Relative to those who stay in the same industry, industry switchers earn a daily wage that is up to 4.5% lower.[33] This underlines that workers are compensated for skills that are neither completely general nor firm-specific but rather specific to their industry (Neal, 1995; Parent, 2000; Sullivan, 2010). In addition, when skills can be fully transferred across jobs, we find that greater experience accumulated in permanent contracts provides job changers with higher wages, relative to those with more experience in temporary contracts. When $\text{dist}_{jk} = 0$, the

[30]To construct the share of workers in each Social Security contribution group, we use individuals who graduated before 1996 and exploit their employment observations between 1997 and 2018. We exclude workers in our sample to avoid any endogeneity issues that may emerge.

[31]While our measure accounts for how similar tasks performed across industries are, it might not capture the entire span of human capital portability. Skills might also be portable along alternative dimensions, such as occupations or jobs. See Kambourov and Manovskii (2009) and Arellano-Bover and Saltiel (2021) for a discussion.

[32]We use the same selection equation for job switchers as in Table 4, Column 2.

[33]The relative penalty is computed multiplying the average wage return from moving between industries, -0.0638, by the maximum distance across industries, 0.7439. See Table 5.

Table 5: Dual Returns to Experience: Industry Mobility and Skills

	FE	FE + Heckman
	(1)	(2)
Distance	-0.0651***	-0.0638***
	(0.0046)	(0.0046)
Experience OEC	0.0499***	0.0452***
	(0.0008)	(0.0009)
Experience FTC	0.0371***	0.0355***
	(0.0010)	(0.0010)
Experience OEC × Distance	-0.0067***	-0.0074***
	(0.0014)	(0.0014)
Experience FTC × Distance	0.0032**	0.0033**
	(0.0014)	(0.0014)
Inverse Mills Ratio		0.0477***
		(0.0022)
Observations	447,098	447,098
R-squared	0.3214	0.3214
Gap in Returns (%)		
Minimum distance (= 0)	34.33***	27.42***
	(2.57)	(2.54)
Maximum distance (= 0.7439)	13.64***	4.61
	(3.98)	(3.94)

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications include the same set of controls as Column (4) in Table 1 except for the polynomial in tenure. In these specifications we use only the first re-employment observation after a job change. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}+\beta^{oec}\times \text{dist}}{\gamma^{ftc}+\beta^{ftc}\times \text{dist}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers. Job switchers = 167,702.

estimated gap in returns is equal to 27.4%. (Column 2 of Table 5). The difference in returns gradually disappears as skill differences between sectors increase, and human capital becomes less portable. When the difference in skill portability across sector is the highest (dist_{jk} = 0.74), the gap in returns lowers up to 4.6% and is not statically significant.

Our baseline mobility analysis is based on all job switchers observed in our time frame, and we use a Heckman-type selection model to address endogenous mobility. However, this strategy does not tackle selection on unobservables, which in turn could bias our results. In Appendix A, we address this issue with two complementary exercises. In the first one, we replicate our benchmark mobility analysis on a sample of workers who moved from their job due to employer-initiated separations.[34] A caveat of this strategy is that we cannot adequately differentiate workers who have been displaced due to economic circumstances, plausibly exogenous to them, from workers laid off due to reasons correlated with their unobserved characteristics. However, we have enough variation in the data to control for unobserved individual heterogeneity even in the sample of involuntary switchers, which likely mitigates that concern. In the second robustness, we extend the Heckman selection equation to include the annual change in US sectoral employment shares as an additional exclusion restriction.[35] The validity of this exclusion restriction is based on two assumptions. The first requires changes in sectoral composition of employment in the U.S. to be correlated with changes in the composition of employment in Spain and, therefore, with the mobility of workers between jobs and sectors. This, for example, could be due to structural transformation forces, common across countries. The second assumption is that it does not directly affect wage trajectories, after controlling for observed and unobserved worker heterogeneity. The outcomes of these two exercises are reported in Tables A.16 to A.19 and confirm our previous results.

Taken together, these findings reinforce the idea that on-the-job learning is contract-specific. Temporary employment might be associated with lower accumulation of human capital, and the lack of human capital would reflect into lower wage (relative to those with longer experience in OECs) only when workers move into jobs where their prior accumulated skills could be transferred.

6.2 Skill-Learning Complementarity

Wage-experience profiles are likely to be heterogeneous across workers and steeper for high-ability individuals, as they take better advantage of learning opportunities (Heckman et al., 2006). Therefore, if the gap in returns we identify arises from differences in skill acquisition across contracts, we should observe higher penalties among high-ability workers, as they are mostly penalized by poorer learning opportunities in fixed-term contracts. We investigate this hypothesis in the following sub-sections.

[34]Employer initiated separations are identified using the Social Security reason from the end of the job spell, which refers to workers who separate from their employers because of individual as well as collective dismissals, or terminations of temporary contracts (see Appendix C for a more detailed explanation of how we identify involuntary movers).

[35]We construct this variable using the 2013 and the 2016 releases of the Socio-Economic Accounts (SEA) by the Groningen Growth and Development Centre. Sectors in SEA are classified as ISIC REV.3 and can be directly linked to the Spanish industry classification, CNAE93 and CNAE09. See Appendix C for a more detailed descriptions of the sector classification.

Table 6: Dual Returns to Experience: Observed Ability

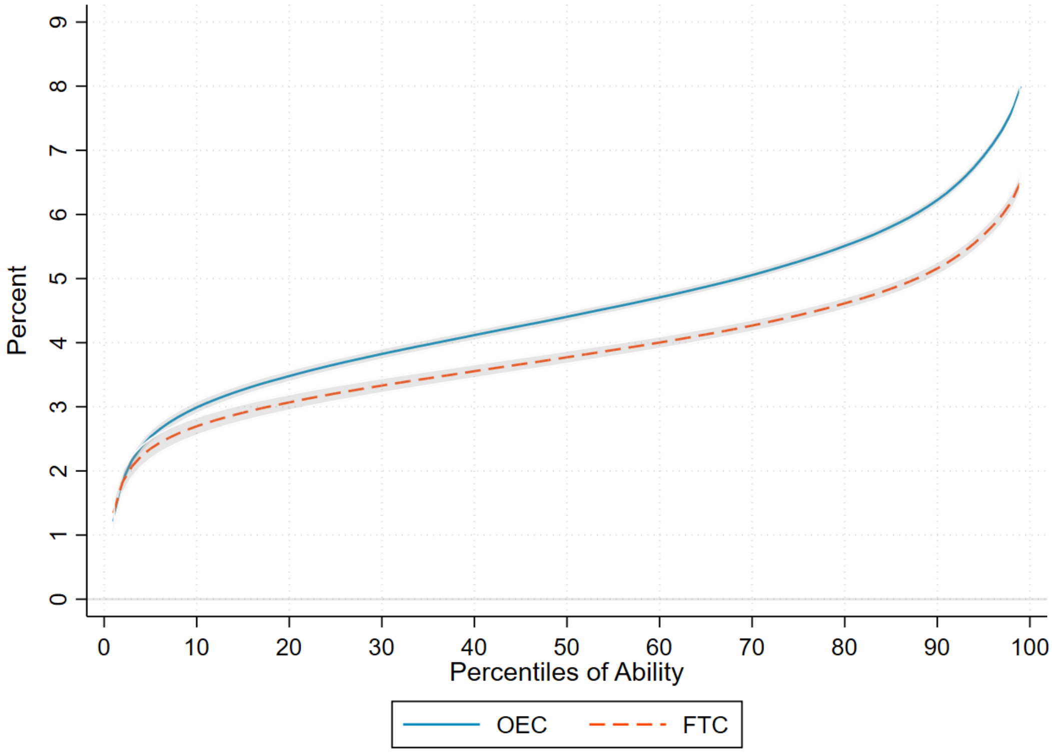
	Education		Occupation	
	Non-College	College	Low-Skill	High-Skill
Experience OEC	0.0421***	0.0590***	0.0461***	0.0540***
	(0.0005)	(0.0009)	(0.0005)	(0.0015)
Experience FTC	0.0428***	0.0438***	0.0420***	0.0368***
	(0.0007)	(0.0011)	(0.0006)	(0.0017)
Gap in Returns (%)	-1.67	34.83***	9.77***	46.84***
	(1.08)	(1.95)	(1.09)	(3.55)
Observations	1,180,999	773,098	1,523,962	430,135
R-squared	0.3051	0.3052	0.3060	0.2873

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. Non-college includes both high-school dropouts and high-school graduates. Low-Skill includes both medium and low-skill occupations as defined in Section C. A worker is considered high-skill (low-skill) if she has been employed more than 50% of her career in a high-skill (low-skill) occupation. All specifications include the same set of controls as Column (4) in Table 1, except for skill dummies in the last two columns. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}}{\gamma^{ftc}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers.

Observed Ability. We estimate contract-specific returns to experience separately by education level. Results are reported in first two columns of Table 6. Workers without a college degree face no differential returns to experience based on whether such was acquired under FTCs or OECs. College graduates instead, while exhibiting similar returns to experience in FTCs, enjoy substantially higher returns to experience from permanent jobs, resulting in a larger gap in returns. In particular, we find that return to experience accumulated in OECs is 35% higher than that from temporary employment. Similar results hold when we split the sample between workers who spent more than 50 percent of their career in high-skill occupations and those who did not (Columns 3 and 4 in Table 6). Specifically, the gap in returns is more than 4 times larger among high-skilled workers compared to low-skilled individuals.

Unobserved Ability. Heterogeneity in returns to experience by observed ability suggest that differences in skill acquisition across contracts might be related to individual (unobserved) ability to learn. To explore this complementarity, we incorporate the interaction between worker's unobserved ability and the learning benefits of fixed-term and

Figure 2: Dual Returns to Experience: Unobserved Ability



Notes: Contract-specific returns to experience computed for each percentile of unobserved ability (individual FE) using estimates ($\times 100$) from equation (9). 95% confidence bands are calculated using the clustered-wild bootstrap (100 repetitions) procedure by Cameron et al. (2008). OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively.

open-ended contracts into our framework and extend equation (7) as follows

$$\ln w_{it} = \eta_i + \sum_{c \in \{\text{ftc,oec}\}} \gamma^c \mathrm{c}_{it} + \sum_{c \in \{\text{ftc,oec}\}} \varphi^c \eta_i \mathrm{c}_{it} + X_{it}\Omega + \delta_e + \delta_t + \epsilon_{it} \tag{9}$$

where the parameter φ^c captures differential returns to contract-specific experience across workers. We estimate equation (9) using de la Roca and Puga (2017)'s algorithm.[36]

Figure 2 shows that both returns are increasing with individual abilities, pointing to a strong complementarity in wages between unobserved skills and acquired experience. However, while past OEC experience has a higher reward on average, the gap in returns increases with individual ability. More specifically, we find that an additional year of experience is associated with 2.5% higher wages, regardless of the type of contract under

[36]The algorithm requires to guess a set of individual fixed effects, η_i^0 and use them to estimate equation (9) by OLS. Therefore we obtain a new set of estimates of worker fixed effects, η_i^1 as

$$\eta_i^1 = \frac{\ln w_{it} - \sum_{c \in \{\text{ftc,oec}\}} \gamma^c \mathrm{c}_{it} - X_{it}\Omega - \delta_e - \delta_t}{\sum_{c \in \{\text{ftc,oec}\}} \varphi^c \mathrm{c}_{it}}$$

and use them as new guess. We iterate this process until the absolute-value norm between η_i^0 and η_i^1 averaged across i is lower than a tolerance level ε. We choose $\varepsilon = 0.001$.

which that experience was acquired. However, for workers above the 90th percentile of the ability distribution, an additional year of experience in OECs translates into 8% higher earnings, while the return to FTC experience is 6.4%, resulting in a 25% gap.[37]

The larger gap in returns among high-ability individuals is consistent with steeper wage-experience profiles of workers who are able to take full advantage of the better learning opportunities offered by permanent jobs. This reinforces the idea of lower skill acquisition during temporary employment episodes. However, due to skill-learning complementarity, only high-skilled individuals seem to be penalized from on-the-job learning in FTCs.

7 Implications for Wage Trajectories

Finally, we assess the extent to which dual on-the-job learning can affect earnings trajectories. To do so, we compare wage growth 15 years after labor market entry for alternative work histories based on the incidence of the two contractual arrangements. Specifically, we use estimates from equation (9) to predict the counterfactual wage growth of workers who spent 15 years in OECs and compare it to the alternative scenario in which workers spend 15 years in FTCs. Given the complementarity between ability and returns to experience, we examine low and high ability workers in the two scenarios. We put these values in context by comparing them to the actual wage growth observed after 15 years of potential experience and report the associated percentile in the distribution.[38]

Table 7 reports the results of this exercise. On the one hand, low-skilled workers do not suffer any significant penalty from accumulating experience in FTCs. After 15 years in the labor market, workers who have always been employed in OECs would face 4pp higher wage growth, allowing them to move only marginally in the wage growth distribution (from the 43th to the 46th percentile). On the other hand, highly skilled workers would be greatly disadvantaged by accumulating experience only through FTCs. The penalty of being continuously employed under FTCs relative to OECs amounts to approximately 16pp lower wage growth, which corresponds to a shift from the 67th to the 77th percentile of the wage growth distribution after 15 years in the labor market.

[37] We have also experimented with a quantile regression approach to estimate equation (7). The results are consistent with the existence of heterogeneous returns across the skill distribution: difference in returns widens along the wage distribution. Results are available upon request.

[38] To compute the actual wage growth distribution, we rely only on the oldest cohorts whom we observed at least 15 years since labor market entry.

Table 7: Life-Cycle Wage Trajectories

Unobserved Ability	Employment Trajectory	Counterfactual Wage Growth, %	Actual Wage Growth, Percentiles
10th Percentile	Always in FTC	40.45	43
10th Percentile	Always in OEC	44.85	46
90th Percentile	Always in FTC	77.37	67
90th Percentile	Always in OEC	93.37	77

Notes: Wage growth calculated as the log difference between entry-level daily wages and daily wages observed 15 years after. Counterfactual wage growth is computed for alternative employment trajectories based on the continuous incidence of OEC or FTC and using (unobserved) ability-specific returns from equation (9). Actual wage growth stands for wage growth for workers observed during 15 years in the labor market.

8 Conclusions

This paper investigates how labor market duality affects human capital accumulation and life-cycle wage profiles of young workers. Our analysis reveals that the return to experience acquired in fixed-term contracts is lower compared to permanent contracts, a difference that is neither due to unobserved firm heterogeneity nor idiosyncratic job match quality. Instead, our results are consistent with limited on-the-job learning during episodes of temporary employment, which mainly penalizes high-skilled workers.

Our findings have implications that go beyond the heterogeneity in returns to experience. Labor market duality affects workers' early careers over and above the instability of employment histories. Experience accumulated in fixed-term contracts is less valuable, and poorer learning opportunities in temporary employment have implications for wage inequality over the life cycle. Policies aimed at increasing human capital accumulation in temporary contracts (e.g., on-the-job training subsidies) could be beneficial in reducing wage differentials in the long run.

Our analysis also suggests that the extensive use of fixed-term contracts is detrimental to high-skilled workers, as it slows their acquisition of skills and wage growth. While restricting fixed-term contracts might improve human capital accumulation among the high-skilled, it can instead penalize low-skilled workers by reducing their job finding rates, or by turning down other non-monetary amenities provided by FTCs (e.g. working-time flexibility). An institutional framework featuring a single contract with increasing firing costs could facilitate hiring decisions, as fixed-term contracts do, and promote investment in human capital, since all employment relationships would be ex-ante open-ended. However, the joint evaluation of both margins would require the use of a structural model, which we leave for future research.

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A Additional Results and Robustness Checks

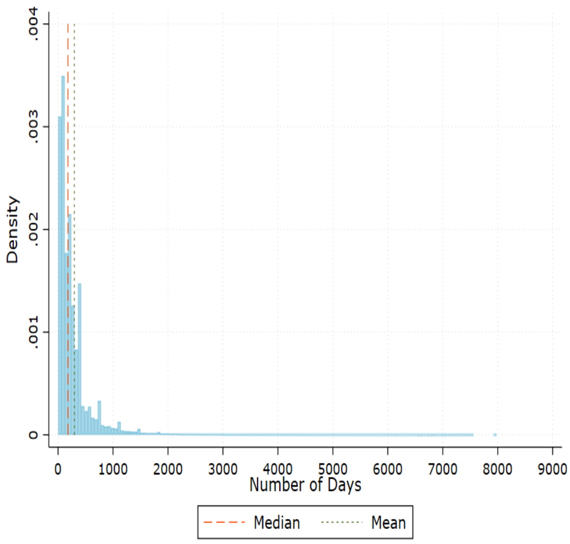
Table A.1: Descriptive Statistics

	(1) All	(2) FTC Entry	(3) OEC Entry
Age	22.41	22.29	23.04
Female	0.523	0.516	0.558
College	0.367	0.339	0.506
LM Entry Outcomes			
Daily Wage	39.51	38.82	43.02
Days Worked	189.56	176.28	256.38
under OEC	33.71	2.05	193.01
under FTC	155.85	174.23	63.37
No. Jobs	3.33	3.46	2.68
Long-Term Outcomes			
Years in the Labor Market	10.50	10.52	10.40
Years of Actual Experience	5.82	5.67	6.56
under OEC	3.22	2.82	5.21
under FTC	2.60	2.85	1.35
No. Jobs	10.49	11.05	7.71
Never on FTC	0.093	0	0.558
Annual Wage Growth	0.065	0.063	0.077
Workers	242,774	202,514	40,260

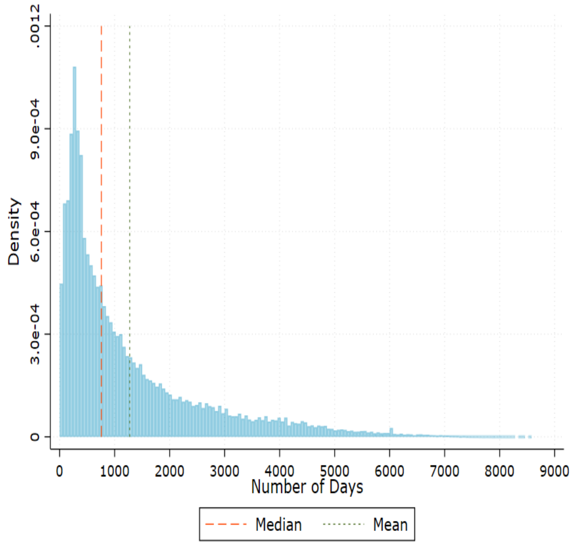
Notes: FTC (OEC) entry column refer to individuals who had a fixed-term (open-ended) contract during the first year of employment after the predicted year of graduation. Age measured at entry into the labor market (LM), i.e., the first year of employment after the predicted year of graduation. LM entry outcomes refer to the first year of employment. Long-term outcomes correspond to the last worker observation. Years in the labor market stand for years after predicted graduation. Actual experience measured at the last individual observation using daily information and converted into years. Annual wage growth stands for year-on-year wage growth averaged over all observations. Wages are expressed in 2018:12 euros deflated using the Spanish monthly consumer price index.

Figure A.1: Distribution of Contract Duration

A. FTCs

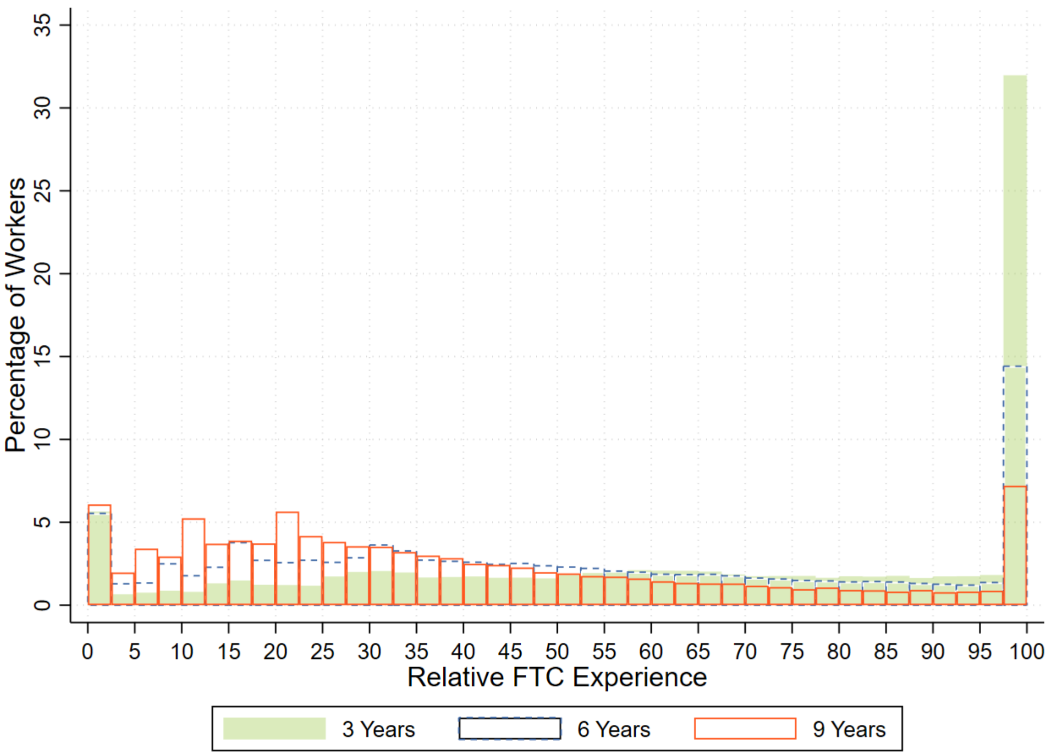


B. OECs



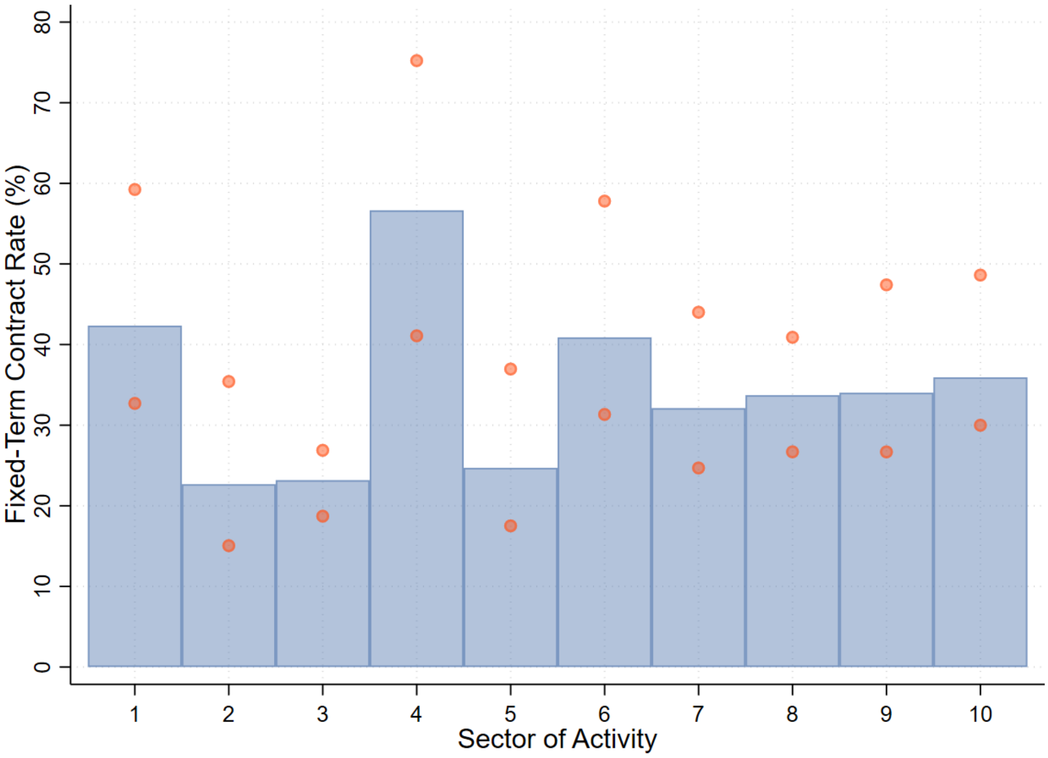
Notes: The figure plots the distribution of duration of fixed-term (Panel A) and open-ended (Panel B) contracts. Vertical lines represent the median (dashed line) and the mean (dotted line) of each distribution.

Figure A.2: Distribution of Workers by Relative FTC Experience



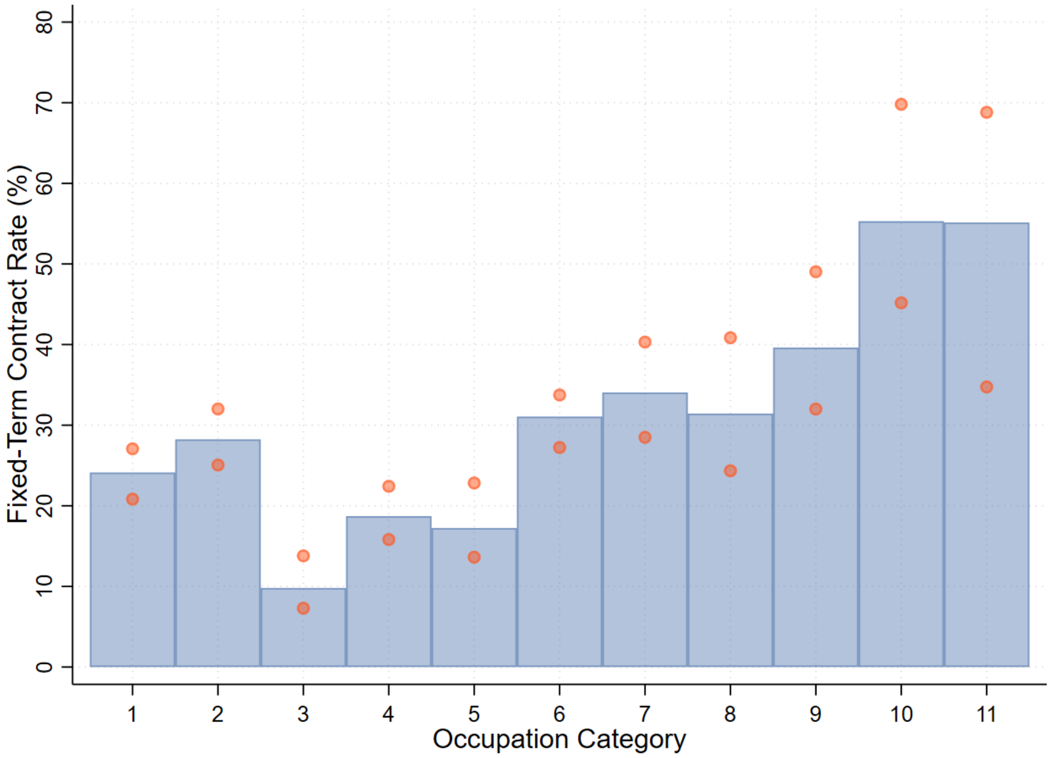
Notes: The figure shows the percentage of workers according to their relative experience in FTC after accumulating 3, 6 and 9 years of overall experience. Relative FTC experience refers to the percent of experience accumulated under fixed-term contracts relative to overall experience.

Figure A.3: Fixed-Term Contract Rate across Sectors



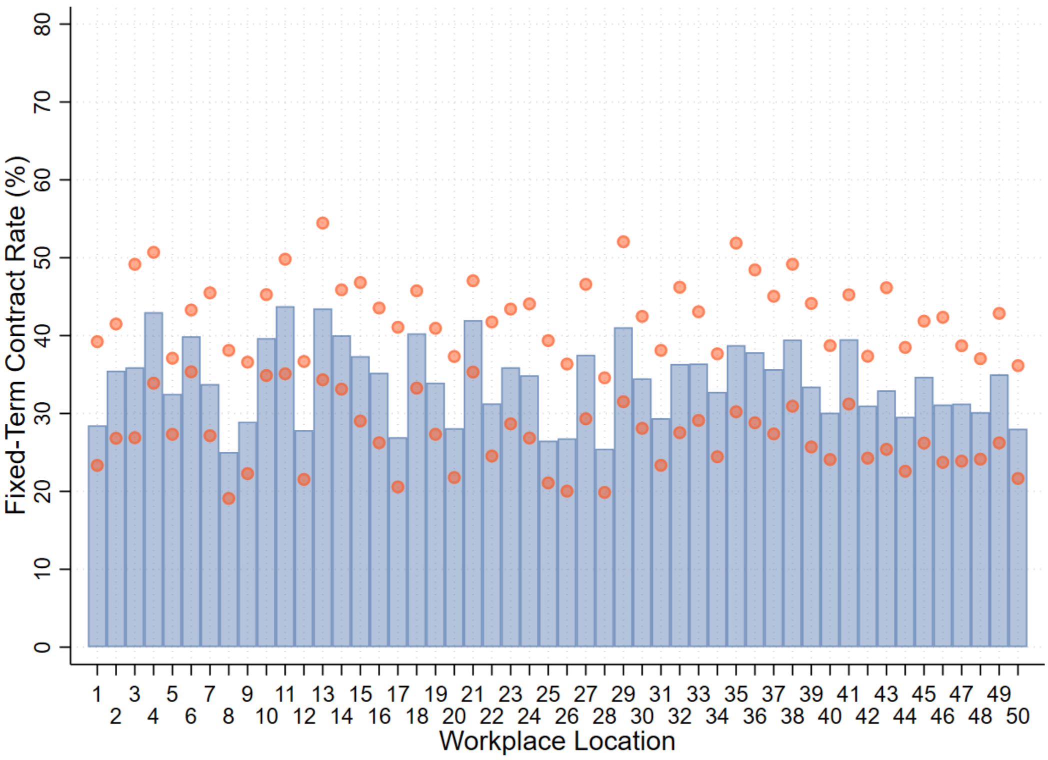
Notes: The figure plots the average fixed-term contract rate (employees on fixed-term contract relative all employees) over the period 1997-2018 along with minimum and maximum values across sectors using all workers in the MCVL. Sector of activities: 1. Primary sector, 2. Manufacturing, 3. Utilities, 4. Construction, 5. Trade and transport, 6. Accommodation and restaurants, 7. Business services, 8. Public sector, 9. Private health institutions and education, and 10. Other services.

Figure A.4: Fixed-Term Contract Rate across Occupations



Notes: The figure plots the average fixed-term contract rate (employees on fixed-term contract relative all employees) over the period 1997-2018 along with minimum and maximum values across occupation groups using all workers in the MCVL. Occupation groups: 1. College/Senior managers, 2. Technicians, 3. Administrative managers, 4. Managerial assistants, 5. First grade administrative workers, 6. Second grad administrative workers 7. Auxiliary administrative staff 8. First and second grade manual workers, 9. Third grade manual workers, and 10. Unqualified workers.

Figure A.5: Fixed-Term Contract Rate across Locations



Notes: The figure plots the average fixed-term contract rate (employees on fixed-term contract relative all employees) over the period 1997-2018 along with minimum and maximum values across provinces using all workers in the MCVL. Provinces: 1. Alava, 2. Albacete, 3. Alicante, 4. Almeria, 5. Avila, 6. Badajoz, 7. Baleares, 8. Barcelona, 9. Burgos, 10. Caceres, 11. Cadiz, 12. Castellon, 13. Ciudad Real, 14. Cordoba, 15. A Coruna, 16. Cuenca, 17. Girona, 18. Granada, 19. Guadalajara, 20. Guipuzcoa, 21. Huelva, 22. Huesca, 23. Jaen, 24. Leon, 25. Lleida, 26. La Rioja, 27. Lugo, 28. Madrid, 29. Malaga, 30. Murcia, 31. Navarra, 32. Ourense, 33. Asturias, 34. Palencia, 35. Las Palmas, 36. Pontevedra, 37. Salamanca, 38. Tenerifa, 39. Cantabria, 40. Segovia, 41. Sevilla, 42. Soria, 43. Tarragona, 44. Terual, 45. Toledo, 46. Valencia, 47. Valladolid, 48. Vizcaya, 49. Zamora, and 50. Zaragoza.

Table A.2: Dual Returns to Experience: Gap in Returns to Experience

	OEC vs FTC	Male vs Female	College vs Non-college
	(1)	(2)	(3)
Experience OEC	0.0500***		
	(0.0005)		
Experience FTC	0.0421***		
	(0.0006)		
Experience ×		0.0519***	
		(0.0005)	
Experience × Female		-0.0044***	
		(0.0003)	
Experience ×			0.0439***
			(0.0005)
Experience × College			0.0082***
			(0.0003)
Gap in Returns (%)	18.52***	9.16***	18.65***
	(1.05)	(0.53)	(0.65)
Observations	1,954,097	1,954,097	1,954,097
R-squared	0.3064	0.3062	0.3074

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications control for the same variables as the fixed effect panel data model estimates in Column (4) in Table 1. Standard errors clustered at the individual level in parenthesis. Gap in contract-specific returns is computed as $100\times(\frac{\gamma^{\text{oec}}}{\gamma^{\text{ftcc}}}-1)$ and standard errors are obtained using the Delta method. The gaps in returns to experience between men and women, and between college and non-college educated individuals, are constructed similarly, without including contemporaneous wage gaps. *** p<0.01, ** p<00.05, * p<00.1. The R-squared reported is within workers.

Table A.3: Dual Returns to Experience: Robustness to Income Measure

	Censored	Tax Data	Pooled Income
Experience OEC	0.0398***	0.0474***	0.0495***
	(0.0004)	(0.0006)	(0.0005)
Experience FTC	0.0371***	0.0410***	0.0439***
	(0.0006)	(0.0007)	(0.0006)
Observations	1,954,097	1,508,948	1,954,097
R-squared	0.3112	0.2306	0.2684

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. Censored specification uses original labor income without correcting for top-coding. Tax data uses information on income coming from tax records for the period 2005-2018. Pooled income consider as measure of daily wages income earned from *all* employers in a given year divided by total days worked in such year. All specifications control for the same variables as the fixed effect panel data model estimates in Column (4) in Table 1. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers.

Table A.4: Dual Returns to Experience: Robustness to Life-Cycle Control

	Cubic Potential Exp.	Excl. Potential Exp	Age Effects
	(1)	(2)	(3)
Experience OEC	0.0514***	0.0456***	0.0481***
	(0.0005)	(0.0005)	(0.0005)
Experience FTC	0.0433***	0.0394***	0.0414***
	(0.0006)	(0.0006)	(0.0006)
Observations	1,954,097	1,954,097	1,954,097
R-squared	0.3152	0.3080	0.3089

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. Potential experience stands for number of years after labor market entry. All specifications control for the same variables as the fixed effect panel data model estimates in Column (4) in Table 1 except for potential experience fixed effects. Column (1) controls parametrically for potential experience but includes only the squared and cubic terms of potential experience, as the linear term is not identified in the presence of year and individual fixed effects. Column (2) does not include any control for life-cycle differences. Column (3) includes as control fixed effects for age-categories. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers.

Table A.5: Dual Returns to Experience: Robustness to 2012 EPL Reform

	OLS		Fixed-Effects	
	(1)	(2)	(3)	(4)
Experience	0.0296***		0.0547***	
	(0.0004)		(0.0005)	
Experience $\times$ $\mathbf{1}[t \geq 2012]$	-0.0003		-0.0053***	
	(0.0003)		(0.0003)	
Experience OEC		0.0353***		0.0535***
		(0.0005)		(0.0005)
Experience OEC $\times$ $\mathbf{1}[t \geq 2012]$		-0.0003		-0.0032***
		(0.0004)		(0.0003)
Experience FTC		0.0221***		0.0513***
		(0.0005)		(0.0007)
Experience FTC $\times$ $\mathbf{1}[t \geq 2012]$		-0.0024***		-0.0114***
		(0.0004)		(0.0004)
Observations	1,954,097	1,954,097	1,954,097	1,954,097
R-squared	0.6330	0.6343	0.3062	0.3073

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications control for the same variables as the fixed effect panel data model estimates in Column (4) in Table 1. OLS regressions include additional controls for education and gender. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported in columns (3) and (4) is within workers.

Table A.6: Dual Returns to Experience: Robustness to Cohort Analysis

	Graduation year cohorts			
	1996	1997	1998	1999
Experience OEC	0.0491***	0.0513***	0.0522***	0.0537***
	(0.0018)	(0.0018)	(0.0018)	(0.0017)
Experience FTC	0.0421***	0.0450***	0.0448***	0.0449***
	(0.0022)	(0.0022)	(0.0022)	(0.0022)
Observations	154,435	158,164	160,100	161,174
R-squared	0.3050	0.2993	0.2990	0.3039

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications control for the same variables as the fixed effect panel data model estimates in Column (4) in Table 1. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported in columns (3) and (4) is within workers.

Table A.7: Dual Returns to Experience: Robustness to Contract-Specific Tenure

	OLS		Fixed-Effects	
	(1)	(2)	(3)	(4)
Experience OEC	0.0357***	0.0357***	0.0499***	0.0502***
	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Experience FTC	0.0200***	0.0210***	0.0431***	0.0433***
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
Observations	1,954,097	1,954,097	1,954,097	1,954,097
R-squared	0.6344	0.6343	0.3064	0.3066

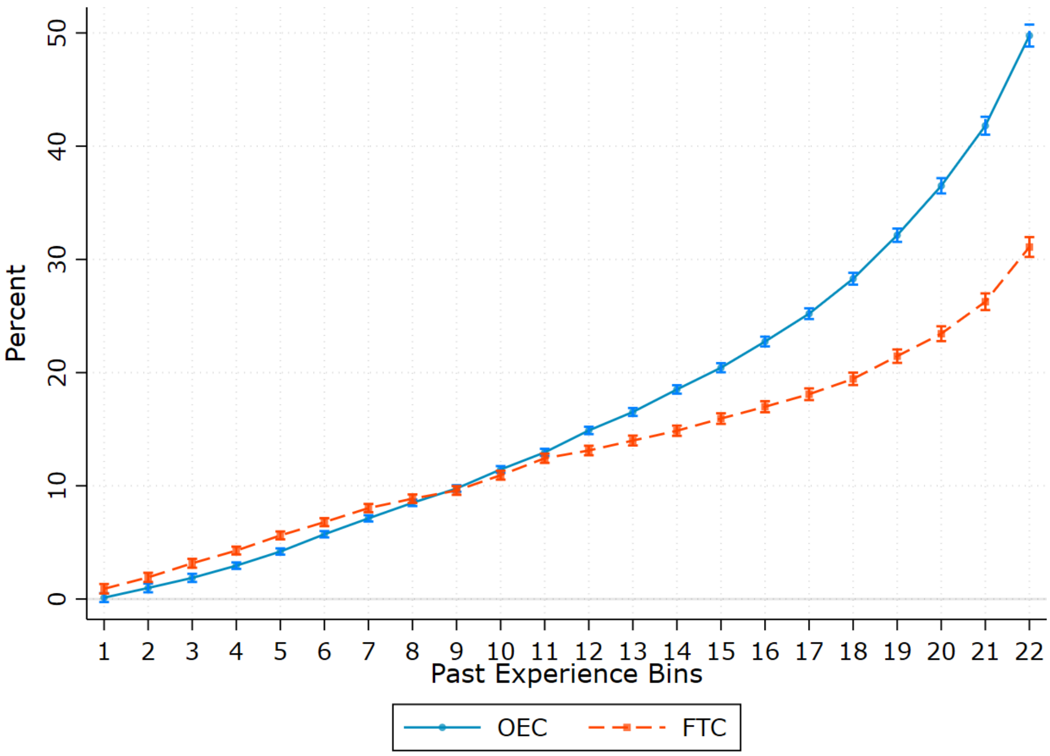
Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. Specifications (1) and (3) include contract-specific quadratic polynomials in tenure. Specification (2) and (4) include contract-specific cubic polynomials in tenure. All specifications control for the same variables as the fixed effect panel data model estimates in Column (4) in Table 1, except for tenure. OLS regressions include additional controls for education and gender. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported in columns (3) and (4) is within workers.

Table A.8: Dual Returns to Experience: Robustness to Gender-Specific Returns

	Males		Females	
	OLS	FE	OLS	FE
	(1)	(2)	(3)	(4)
Experience OEC	0.0418***	0.0507***	0.0385***	0.0490***
	(0.0005)	(0.0007)	(0.0005)	(0.0007)
Experience FTC	0.0234***	0.0406***	0.0232***	0.0427***
	(0.0006)	(0.0008)	(0.0006)	(0.0008)
Observations	934,294	934,294	1,019,803	1,019,803
R-squared	0.6073	0.2870	0.6282	0.3242

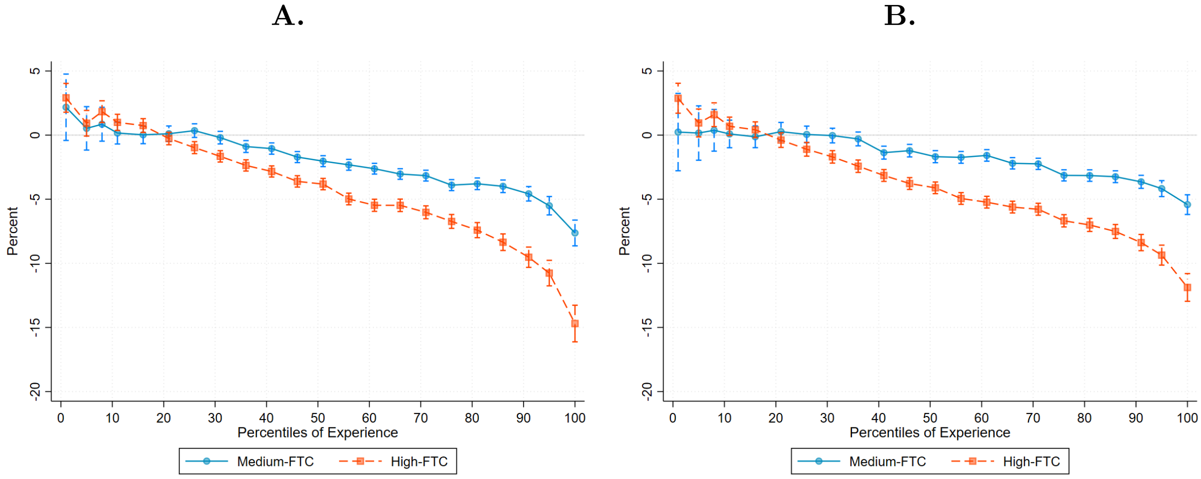
Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications include controls for a quadratic polynomial in tenure, type of contract, a dummy for part-time jobs, indicators for occupation-skill category (2), sector of activity (10), workplace location (50), small and medium enterprises (plant size < 50), young organizations (plant age < 10), potential experience dummies (5), and year dummies (22). OLS regressions include additional controls for education. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported in Columns (3) and (4) is within workers.

Figure A.6: Robustness to Non-Parametric Experience: Returns to Experience Accumulated under Different Contracts



Notes: Estimates (×100) and 95% confidence intervals of return to experience in fixed-term (FTC) and open-ended (OEC) contracts. Standard errors are clustered at the individual level. Contract-specific experience is measured in days, converted into years, and then discretized into 22 bins. Number of bins are chosen to have a sufficient and balanced number of observations within each cell. The model controls for the same variables as the fixed effect panel data model in Column (4) in Table 1.

Figure A.7: Robustness to Thresholds: Incidence of Temporary Employment



Notes: Estimates (×100) and 95% confidence intervals of the scarring effects of temporary employment, $\beta_{2(q)}$ and $\beta_{3(q)}$, from equation 8. Standard errors are clustered at the individual level. Medium-FTC (High-FTC) incidence refers to individuals whose actual experience on a temporary contract relative to overall actual experience is in Panel A between 0.5 and 0.9 (above 0.9) and in Panel B between 0.3 and 0.6 (above 0.6).

Table A.9: Dual Returns to Experience: Continuously Employed Workers

Actual experience, % of potential experience	≥0%	≥50%	≥80%	≥90%	=100%
	(1)	(2)	(3)	(4)	(5)
Current FTC	-0.0370***	-0.0418***	-0.0540***	-0.0611***	-0.0716***
	(0.0009)	(0.0012)	(0.0018)	(0.0024)	(0.0059)
Share of Experience FTC	-0.1984***	-0.1670***	-0.1348***	-0.1233***	-0.1001***
	(0.0023)	(0.0030)	(0.0041)	(0.0050)	(0.0076)
Observations	1,954,097	1,235,490	636,241	411,096	183,045
R-squared	0.3047	0.2899	0.2751	0.2621	0.2305

Notes: Experience is measured in days and then it is converted into years. Share of Experience FTC stands for experience acquired under fixed-term contracts divided by 1 + total actual experience. All specifications include individual fixed effects, a quadratic polynomial in tenure, a dummy for part-time jobs, indicators for occupation-skill category (2), sector of activity (10), workplace location (50), small and medium enterprises (plant size $<$ 50), young organizations (plant age $<$ 10), potential experience dummies (5), and year dummies (22). Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared is within workers.

Table A.10: Worker Observed in the Matched Sample

	(1)
Actual Experience	0.0047***
	(0.0002)
College	0.0722***
	(0.0019)
Female	0.0302***
	(0.0016)
High-Skill	0.1255***
	(0.0024)
Part-Time	-0.0120***
	(0.0015)
FTC	0.1110***
	(0.0014)
Tenure	0.0101***
	(0.0004)
Manufacturing	0.0538***
	(0.0067)
Construction	0.2760***
	(0.0123)
Services	0.1267***
	(0.0065)
Big City	0.0191***
	(0.0015)
Observations	1,954,097
R-squared	0.0593

Notes: The table reports the results of a linear probability model where the dependent variable is an indicator for workers observed in the matched sample for which firm fixed effects are obtained, as explained in Section 5.4. FTC is an indicator variable for fixed-term contracts. Big city includes the largest four cities in Spain: Madrid, Barcelona, Valencia, and Sevilla. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: Dual Returns to Experience: Firm-cluster fixed effects (BLM, 2022)

	Baseline Sample	BLM Restricted Sample			
	(1)	(2)	(3)	(4)	(5)
Experience OEC	0.0500***	0.0575***	0.0570***	0.0562***	0.0564***
	(0.0005)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Experience FTC	0.0421***	0.0440***	0.0448***	0.0444***	0.0446***
	(0.0006)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
Gap in Returns (%)	18.51***	30.50***	27.18***	26.45***	26.43***
	(1.05)	(2.21)	(2.05)	(2.05)	(2.03)
Observations	1,954,097	456,364	456,364	456,364	456,364
R-squared	0.3064	0.2372	0.2212	0.2180	0.2174
Firm-clusters	NO	NO	$K = 5$	$K = 50$	$K = 100$

Notes: Firm-clusters are defined following Bonhomme et al. (2022b) using a $k-$means clustering minimization algorithm over the empirical distributions of log earnings, after controlling for time, age, education and part-time status fixed effects. The classification is based on all workers in the MCVL employed by firms for which we observe at least 10 workers each year between 1997-2018. Experience is measured in days and then it is converted into years. All specifications include the same set of controls as the fixed effect panel data model estimates in Column (4) in Table 1. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The R-squared is within workers.

Table A.12: Dual Returns to Experience: Match Quality - First stage

	Experience OEC	Experience FTC	Tenure	Tenure squared
Experience OEC, deviation	0.6185***	-0.2690***	-0.0030**	-0.0771***
	(0.0010)	(0.0009)	(0.0013)	(0.0118)
Experience FTC, deviation	-0.4866***	0.6706***	0.0029*	-0.1428***
	(0.0013)	(0.0011)	(0.0016)	(0.0140)
Tenure, deviation	0.0529***	0.0233***	0.7992**	-1.5503***
	(0.0006)	(0.0006)	(0.0009)	(0.0078)
Tenure squared, deviation	0.0014***	0.0008***	0.0093**	1.0715***
	(0.0001)	(0.0001)	(0.0001)	(0.0006)
Observations	1,929,990	1,929,990	1,929,990	1,929,990
Partial R-squared	0.2116	0.2117	0.5511	0.8357
Kleibergen-Paap rk Wald F statistic		10722.75		
Sanderson-Windmeijer F statistics	53399.94	53796.77	2.9e+06	1.7e+07

Notes: Experience is measured in days and then it is converted into years. All specifications include individual fixed effects, a quadratic polynomial in tenure, a dummy for part-time jobs, indicators for occupation-skill category (2), sector of activity (10), workplace location (50), small and medium enterprises (plant size < 50), young organizations (plant age < 10), potential experience dummies (5), and year dummies (22). Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A.13: Dual Returns to Experience: Match Quality - First stage

	Experience OEC	Experience FTC	Tenure	Tenure squared
Experience OEC, deviation	0.4899***	-0.3355***	0.1638***	1.1866***
	(0.0015)	(0.0013)	(0.0013)	(0.0104)
Experience FTC, deviation	-0.5443***	0.6156***	0.1332***	0.9268***
	(0.0015)	(0.0012)	(0.0015) (	0.0119)
Tenure, deviation	0.0965***	-0.0355***	0.7144***	-2.1725***
	(0.0011)	(0.0009)	(0.0013)	(0.0113)
Tenure squared, deviation	0.0005***	0.0034***	0.0064***	1.0510***
	(0.0001)	(0.0001)	(0.0001)	(0.0005)
Observations	1,929,990	1,929,990	1,929,990	1,929,990
Partial R-squared	0.0091	0.0091	0.0128	0.0860
Kleibergen-Paap rk Wald F statistic		1547.810		
Sanderson-Windmeijer F statistics	6225.21	6448.12	18306.89	2.6e+05

Notes: Experience is measured in days and then it is converted into years. All specifications include individual fixed effects, a quadratic polynomial in tenure, a dummy for part-time jobs, indicators for occupation-skill category (2), sector of activity (10), workplace location (50), small and medium enterprises (plant size < 50), young organizations (plant age < 10), potential experience dummies (5), and year dummies (22). Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A.14: Dual Returns to Experience: Match Quality - First stage

	Experience OEC	Experience FTC	Tenure	Tenure squared
Experience OEC, deviation	0.6185***	-0.2690***	-0.0030***	-0.0769**
	(0.0010)	(0.0010)	(0.0013)	(0.0118)
Experience FTC, deviation	-0.4865***	0.6706***	0.0029*	-0.1429***
	(0.0013)	(0.0011)	(0.0016)	(0.0140)
Tenure, deviation	0.0529***	0.0233***	0.7992***	-1.5505***
	(0.0006)	(0.0006)	(0.0009)	(0.0078)
Tenure squared, deviation	0.0014***	0.0008***	0.0093***	1.0716***
	(0.0001)	(0.0001)	(0.0001)	(0.0006)
Subsidies availability	0.0130***	0.0034**	-0.0032	-0.0351
	(0.0016)	(0.0014)	(0.0026)	(0.0224)
Observations	1,929,990	1,929,990	1,929,990	1,929,990
Partial R-squared	0.2117	0.2117	0.5512	0.8357
Kleibergen-Paap rk Wald F statistic		8592.661		
Sanderson-Windmeijer F statistics	26735.09	26930.80	1.4e+06	8.4e+06

Notes: Experience is measured in days and then it is converted into years. All specifications include individual fixed effects, a quadratic polynomial in tenure, a dummy for part-time jobs, indicators for occupation-skill category (2), sector of activity (10), workplace location (50), small and medium enterprises (plant size < 50), young organizations (plant age < 10), potential experience dummies (5), and year dummies (22). Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A.15: Dual Returns to Experience: Match Quality - First stage

	Experience OEC	Experience FTC	Tenure	Tenure squared
Experience OEC, deviation	0.4896***	-0.3354***	0.1638***	1.1867***
	(0.0015)	(0.0013)	(0.0013)	(0.0104)
Experience FTC, deviation	-0.5442***	0.6155***	0.1331***	0.9267***
	(0.0015)	(0.0012)	(0.0015)	(0.0119)
Tenure, deviation	0.0968***	-0.0356***	0.7144***	-2.1727***
	(0.0011)	(0.0009)	(0.0013)	(0.0113)
Tenure squared, deviation	0.0005***	0.0034***	0.0064***	1.0511***
	(0.0001)	(0.0001)	(0.0001)	(0.0005)
Subsidies availability	0.0621***	-0.0294***	-0.0064***	-0.0556***
	(0.0035)	(0.0027)	(0.0026)	(0.0223)
Observations	1,929,990	1,929,990	1,929,990	1,929,990
Partial R-squared	0.0093	0.0093	0.0132	0.0897
Kleibergen-Paap rk Wald F statistic		1268.399		
Sanderson-Windmeijer F statistics	3189.16	3307.31	9478.40	1.3e+05

Notes: Experience is measured in days and then it is converted into years. All specifications include individual fixed effects, a quadratic polynomial in tenure, a dummy for part-time jobs, indicators for occupation-skill category (2), sector of activity (10), workplace location (50), small and medium enterprises (plant size < 50), young organizations (plant age < 10), potential experience dummies (5), and year dummies (22). Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A.16: Dual Returns to Experience: Involuntary Movers

	All	Within Industries	Across Industries
	(1)	(2)	(3)
Experience OEC	0.0428***	0.0444***	0.0355***
	(0.0011)	(0.0017)	(0.0020)
Experience FTC	0.0353***	0.0323***	0.0357***
	(0.0013)	(0.0020)	(0.0023)
Gap in Returns (%)	21.17***	37.62***	-0.72
	(3.03)	(5.41)	(5.09)
Observations	307,637	161,468	146,169
R-squared	0.3238	0.3004	0.3381

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications include the same set of controls as Column (4) in Table 1 of the manuscript except for the polynomial in tenure. In these specifications we use only the first re-employment observation after a job change. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}}{\gamma^{ftc}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers. Job switchers = 167,702.

Table A.17: Industry Mobility and Skills for Involuntary Movers

	(1)
Distance	-0.0768***
	(0.0059)
Experience OEC	0.0434***
	(0.0011)
Experience FTC	0.0343***
	(0.0013)
Experience OEC × Distance	-0.0092***
	(0.0020)
Experience FTC × Distance	0.0031***
	(0.0017)
Observations	307,637
R-squared	0.3259
Gap in Returns (%)	
Minimum distance (= 0)	26.35***
	(3.36)
Maximum distance (= 0.7439)	-0.16
	(5.30)

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. The specification includes the same set of controls as Column (4) in Table 1 of the manuscript except for the polynomial in tenure. We use only the first re-employment observation after a job change. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}+\beta^{oec}\times\text{dist}}{\gamma^{ftc}+\beta^{ftc}\times\text{dist}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers. Job switchers = 167,702.

Table A.18: Dual Returns to Experience: Expanded Heckman Correction

	All	Within Industries		Across Industries	
	(1)	(2)	(3)	(4)	(5)
Experience OEC	0.0521***	0.0522***	0.055***	0.0455***	0.0440***
	(0.0011)	(0.0017)	(0.0017)	(0.0020)	(0.0020)
Experience FTC	0.0436***	0.0376***	0.0388***	0.0456***	0.0452***
	(0.0014)	(0.0022)	(0.0021)	(0.0024)	(0.0024)
Inverse Mills Ratio	0.0431***	0.0044***		0.0377***	
(job switching)	(0.0025)	(0.0037)		(0.0045)	
Inverse Mills Ratio			0.0352***		0.0960***
(industry/job switching)			(0.0055)		(0.0055)
Gap in Returns (%)	19.53***	38.69***	41.74***	-0.23	-2.74
	(2.26)	(4.48)	(4.49)	(3.56)	(3.47)
Observations	338,983	177,888	177,888	161,095	161,095
R-squared	0.3174	0.2928	0.2920	0.3361	0.3383

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. All specifications include the same set of controls as Column (4) in Table 1 of the manuscript except for the polynomial in tenure. In these specifications we use only the first re-employment observation after a job change. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}}{\gamma^{ftc}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers.

Table A.19: Industry Mobility and Skills with Expanded Heckman Correction

	(1)
Distance	-0.0581***
	(0.0052)
Experience OEC	0.0528***
	(0.0011)
Experience FTC	0.0429***
	(0.0014)
Experience OEC × Distance	-0.0092***
	(0.0016)
Experience FTC × Distance	0.0021***
	(0.0016)
Inverse Mills Ratio	0.0427***
	(0.0025)
Observations	338,983
R-squared	0.3190
Gap in Returns (%)	
Minimum distance (= 0)	23.06***
	(2.44)
Maximum distance (= 0.7439)	3.36
	(3.92)

Notes: Experience is measured in days and then it is converted into years. OEC and FTC stand for experience acquired under open-ended and fixed-term contracts, respectively. The specification includes the same set of controls as Column (4) in Table 1 of the manuscript except for the polynomial in tenure. We use only the first re-employment observation after a job change. Standard errors clustered at the individual level in parenthesis. Gap in returns is computed as $100 \times (\frac{\gamma^{oec}+\beta^{oec}\times\text{dist}}{\gamma^{ftc}+\beta^{ftc}\times\text{dist}} - 1)$ and standard errors are obtained using the Delta method. *** p<0.01, ** p<0.05, * p<0.1. The R-squared reported is within workers.

Table A.20: Job Switching Selection Equation

	Baseline $\mathbf{1}[j_{it} \neq j_{it-1}]$ (1)	Expanded $\mathbf{1}[j_{it} \neq j_{it-1}]$ (2)
Δ Employment share$_{k(it)}$		5.8369***
		(0.4148)
Cohabitant$\in$[0,6]$_{it-1}$	-0.0639***	-0.0508***
	(0.0046)	(0.0054)
Cohabitant$\in$[7,15]$_{it-1}$	0.0101*	0.0111*
	(0.0052)	(0.0061)
Cohabitant$\in$[16,64]$_{it-1}$	0.0075**	0.0078*
	(0.0037)	(0.0044)
High-school$_i$	0.0020	0.0015
	(0.0053)	(0.0062)
College$_i$	0.0235***	0.0181***
	(0.0059)	(0.0069)
Female$_i$	-0.0120***	-0.0118***
	(0.0027)	(0.0031)
Mid-Skill$_{it-1}$	-0.0984***	-0.1000***
	(0.0031)	(0.0036)
High-Skill$_{it-1}$	-0.1968***	-0.1976***
	(0.0043)	(0.0051)
FTC$_{it-1}$	0.6102***	0.6211***
	(0.0028)	(0.0033)
Part-Time$_{it-1}$	0.0782***	0.0590***
	(0.0029)	(0.0035)
Small-medium Firm$_{it-1}$	0.2890***	0.3014***
	(0.0027)	(0.0031)
Young Firm$_{it-1}$	0.0538***	0.0597***
	(0.0024)	(0.0028)
Tenure$_{it-1}$	-0.1263***	-0.1383***
	(0.0018)	(0.0021)
Tenure$^2_{it-1}$	0.0052***	0.0066***
	(0.0002)	(0.0002)
Potential Experience $\in$[4,6]$_{it-1}$	0.0064*	-0.0052
	(0.0037)	(0.0041)
Potential Experience $\in$[7,9]$_{it-1}$	-0.0072*	-0.0264***
	(0.0041)	(0.0047)
Potential Experience $\in$[10,12]$_{it-1}$	-0.0277***	-0.0441***
	(0.0046)	(0.0054)
Potential Experience $\in$[13,15]$_{it-1}$	-0.0564***	-0.0584***
	(0.0053)	(0.0066)
Observations	1,626,148	1,225,573

Notes: Province, sector, and year fixed effects are also included as additional controls. Small and medium firms are plants with plant-size below 50. Young firms refer to firms with less than 10 years of activity. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A.21: Conditional Industry Switching Selection Equation

	Baseline		Expanded	
	$\mathbf{1}[k_{it} = k_{it-1} \| j_{it} \neq j_{it-1}]$	$\mathbf{1}[j_{it} \neq j_{it-1}]$	$\mathbf{1}[k_{it} = k_{it-1} \| j_{it} \neq j_{it-1}]$	$\mathbf{1}[j_{it} \neq j_{it-1}]$
	(1)	(2)	(3)	(4)
Δ Employment share$_{k(it)}$			1.4837***	6.3380***
			(0.5323)	(0.4058)
Cohabitant$\in [0,6]_{it-1}$		-0.0543***		-0.0442***
		(0.0042)		(0.0047)
Cohabitant$\in [7,15]_{it-1}$		0.0029		0.0030
		(0.0045)		(0.0053)
Cohabitant$\in [16,64]_{it-1}$		0.0045		0.0052
		(0.0032)		(0.0037)
Hign-School$_i$	-0.0401***	0.0012	-0.0506***	0.0011
	(0.0069)	(0.0053)	(0.0080)	(0.0062)
College$_i$	-0.1002***	0.0210***	-0.1021***	0.0164**
	(0.0085)	(0.0058)	(0.0094)	(0.0069)
Female$_i$	0.0383***	-0.0105***	0.0375***	-0.0097***
	(0.0041)	(0.0027)	(0.0046)	(0.0031)
Mid-Skill$_{it-1}$	0.0228***	-0.0954***	0.0107*	-0.0972***
	(0.0057)	(0.0031)	(0.0058)	(0.0036)
High-Skill$_{it-1}$	0.0386***	-0.1900***	0.0054	-0.1911***
	(0.0123)	(0.0044)	(0.0117)	(0.0051)
FTC$_{it-1}$	0.3450***	0.6123***	0.3545***	0.6230***
	(0.0114)	(0.0028)	(0.0108)	(0.0033)
Part-Time$_{it-1}$	0.0082	0.0773***	-0.0025	0.0582***
	(0.0052)	(0.0029)	(0.0056)	(0.0035)
Small-Medium Firm$_{it-1}$	0.2792***	0.2903***	0.2877***	0.3030***
	(0.0036)	(0.0027)	(0.0040)	(0.0030)
Young Firm$_{it-1}$	0.0757***	0.0552***	0.0786***	0.0609***
	(0.0032)	(0.0024)	(0.0036)	(0.0028)
Tenure$_{it-1}$	-0.0575***	-0.1240***	-0.0676***	-0.1358***
	(0.0040)	(0.0018)	(0.0043)	(0.0022)
Tenure$^2_{it-1}$	0.0013***	0.0050***	0.0023***	0.0064***
	(0.0003)	(0.0002)	(0.0003)	(0.0002)
Potential Experience $\in [4,6]_{it-1}$	-0.0021	0.0047	-0.0075	-0.0070*
	(0.0047)	(0.0037)	(0.0051)	(0.0041)
Potential Experience $\in [7,9]_{it-1}$	-0.0148***	-0.0100**	-0.0257***	-0.0291***
	(0.0053)	(0.0041)	(0.0059)	(0.0046)
Potential Experience $\in [10,12]_{it-1}$	-0.0388***	-0.0317***	-0.0475***	-0.0475***
	(0.0060)	(0.0047)	(0.0070)	(0.0054)
Potential Experience $\in [13,15]_{it-1}$	-0.0450***	-0.0611***	-0.0513***	-0.0620***
	(0.0071)	(0.0053)	(0.0089)	(0.0066)
(log) Daily wage$_{it-1}$	0.1578***		0.1600***	
	(0.0051)		(0.0051)	
Error correlation (Fisher's transformation)	1.4665***		1.524***	
	(0.0844)		(0.0836)	
Observations	1,626,148	1,626,148	1,225,573	1,225,573

Notes: Province, sector, and year fixed effects are also included as additional controls. Small and medium firms are plants with plant-size below 50. Young firms refer to firms with less than 10 years of activity. Standard errors clustered at the individual level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

B Censoring Correction

The MCVL reports data on monthly labor income from Social Security contribution, which are either bottom or top-coded.[39] In the data, around 13 percent of the log real daily wages of the worker-month observations are top-coded.[40]

Following other studies that face censored earnings in administrative data (Dustmann et al., 2009; Card et al., 2013; Bonhomme and Hospido, 2017), we correct the upper tail by fitting cell-by-cell Tobit models to log real daily wages separately by gender. Each cell, c, is defined according to occupational groups (3 categories), age groups (5 categories), and years (39) for a total of 2×585 cells. Consistent with a vast literature that finds that log-normality provides a reasonable approximation to empirical wage distributions, within each cell, log-daily wages are assumed to follow a Gaussian distribution with cell-specific mean and variance, i.e. $log\ w \sim N(X\beta_c, \sigma_c^2)$.[41]

The parameters of interest are estimated within each cell by maximum likelihood. Denoting Φ the standard normal cdf, the cell-specific maximum likelihood takes the following form (up to an additive constant).

$$\sum_{cens_{it}=0} \left[-\frac{1}{2} ln\ \sigma_c^2 - \frac{1}{2\sigma_c^2} (ln(w_{ijt}) - X_{it}\beta_c)^2 \right] + \sum_{cens_{ijt}=1} ln \left(1 - \Phi \left(\frac{ln(\bar{w}) - X_{ijt}\beta_c}{\sigma_c} \right) \right)$$

where w_{it} represents real log daily wages of individual i in plant j in moment t (a worker-month pair), $\bar{w}$ is the maximum cap, $cens_{ijt} = 1$ if the observation is top-coded. X_{ijt} is a set of controls such as age, categorical variables for full-time jobs, sector of activity (10), workplace location (50), firm age (3), and monthly dummies (12). Following Card et al. (2013), we also include individual-specific components of the wages using the mean log daily wages in other months, fraction of censored wages in other months, and a dummy for individuals observed only once as additional controls. For individuals who are only observed once, we set the mean log daily wages to the sample mean, and the fraction of censored wages to the share of censored earnings in the sample.

[39] See Appendix C for a more detailed description the labor income concept.

[40] Less than 8 percent of the observations are bottom-coded. However, we do not correct the lower tail due to the existence of a national minimum wage.

[41] The choice of the distribution is important and a natural concern is that the results may differ depending on the technique. In this sense, Dustmann et al. (2009) offer an extensive robustness analysis in which they evaluate four different distributional assumptions, and conclude that the results are similar to different specifications. Similarly, Bonhomme and Hospido (2017) use the MCVL to compare the performance of the cell-by-cell Tobit model and a linear quantile censoring correction method with respect to non-censored earnings coming from tax records, and find that the fit is superior with the Tobit model.

After the estimation, we impute an uncensored value for each censored observation using the maximum likelihood estimates of each Tobit model. Specifically, we replace censored observation by the sum of the predicted wages and a random component, drawn from a normal distribution with mean zero and cell-specific variance. The imputation rule is:

$$lnw_{ijt} = X_{ijt}\hat{\beta}_c \; + \; \hat{\sigma}_c \, \Phi^{-1} \left[\Phi\left(\frac{ln\ \bar{w} - X_{ijt}\hat{\beta}_c}{\hat{\sigma}_c} \right) + u_{ijt} \times \left(1 - \Phi\left(\frac{ln\ \bar{w} - X_{ijt}\hat{\beta}_c}{\hat{\sigma}_c} \right) \right) \right]$$

where $(\hat{\beta}_c, \hat{\sigma}_c)$ are the maximum likelihood estimates of each cell, Φ denotes the standard normal cdf, and u represents a random draw from the uniform distribution, $U[0, 1]$.

Table B.1: Censored and imputed wage distributions

Percentiles	Censored	Imputed
5th	3.00	3.00
10th	3.33	3.33
25th	3.70	3.70
50th	4.04	4.04
75th	4.43	4.45
90th	4.74	5.17
95th	4.78	5.68

Notes: Wages refer to log real daily wages earned by workers in a given employer each month. Wages are expressed in 2018:12 euros deflated using the Spanish monthly consumer price index. Moments of the the log daily wage distribution are computed over month-worker-firm observations (93,407,145).

C Variables Definition

Birth date. Obtained from personal files coming from the Spanish Residents registry. We select this information from the most recent wave and, if there is any inconsistency, we choose the most common value over the waves for which it is available.

Education. Retrieved from the Spanish Residents registry up to 2009, and from 2009 thereafter the Ministry of Education directly reports individuals' educational attainment to the National Statistical Office and this information is used to update the corresponding records in the Residence registry. Therefore, the educational attainment is imputed backwards whenever it is possible, i.e. when a worker is observed in the MCVL post-2009. In the imputation, we assigned 25 years as the minimum age to recover values related to university education.[42]

Gender. Obtained from the Spanish Residence registry. We select this information from the most recent wave and, if there is any inconsistency, we choose the mode over the waves in which it is available.

Household composition. Obtained from the Spanish Residence registry. The variable includes the number of individuals living in the household in three age categories: cohabitants under 6 years old, 7 to 15 years old and over 16 years old.

Nationality. Obtained from Spanish Residents registry. The variable reports the link between the individual and Spain in terms of legal rights and duties. This variable allows to distinguish between individuals with Spanish nationality (N00 code) and other worldwide nationalities.

Labor market entry. To define labor market entry, we exploit information on education attainment and compute predicted graduation year of each individual. Specifically, education-specific graduation years are assigned as the years when high-school drop-outs turn 16, people with high-school degrees turn 18, and college graduates turn 23. We track workers after their predicted graduation year to compute time employed and out-of-work.

[42] The age threshold is the average graduation age for a Bachelor's degree in Spain: https://www.oecd.org/education/education-at-a-glance-19991487.htm

Employment status. An individual is considered to be employed in a given year if annual income is at least equal to one quarter of full-time work at half of the national minimum wage.

Experience. Defined as the time actually worker after labor market entry. We compute actual working days using information on all the spells available for each worker in the MCVL since labor market entry. Specifically, at each year, we count the exact number of days worked and compute our measure of experience as the share of time actually worked in the past relative to the potential time that an individual could have worked since labor market entry.

Tenure. Computed as the number of days continuously worked for the same employer, regardless of the type of contract or other characteristics of the work. This measure is reset to zero if there is at least one month between two periods of work with the same employer during which the worker is not an employer or works in another company.

Labor income. The MCVL reports labor income from two different sources: Social Security contribution basis and income tax records. Contribution bases capture gross monthly labor earnings plus one-twelfth of year bonuses.[43] Earnings are bottom and top-coded. The minimum and maximum caps vary by Social Security regime and contribution group, and they are adjusted each year according to the evolution of the minimum wage and inflation rate. The data is supplemented with information provided by the Fiscal Authorities on the total wages that employers pay to employees on an annual basis. The advantage of this measure is that it is not censored. However, fiscal information is only included from 2005 onwards and excludes Basque Country and Navarra. Our main analysis relies on labor income coming from Social Security contributions and we correct top-censored earnings fitting cell-by-cell Tobit models to log real daily wages (see Appendix B). Wages are expressed in 2018:12 euros deflated using the Spanish monthly consumer price index.

Contract type. The MCVL contains a long list of contract types (over 100) that are summarized in two broad categories, according to its permanent or temporary nature. Permanent contracts include regular permanent contracts (*contrato indefinido fijo*)

[43]Exceptions include extra hours, travel and other expenses, and death or dismissal compensations.

and intermittent (seasonal) permanent contracts (*indefinido fijo-discontinuo*). Temporary contracts include specific project or service contracts (*temporal por obra o servicio*), temporary increase in workload (*eventual de produccion*), and substitution contracts (*interinidad o relevo*).

Occupation category. Based on Social Security contribution group. These groups indicate a level in a ranking determined by the worker's contribution to the Social Security system, which is determined by both the education level required for the specific job and the complexity of the task. The MCVL contains 10 different contribution groups that are aggregated according to similarities in skill requirements. High-Skill: Group 1 (engineers, college, senior managers —in Spanish *ingenieros, licenciados y alta direccion*), Group 2 (technicians —*ingenieros tecnicos, peritos y ayudantes*), and Group 3 (administrative managers —*jefes administrativos y de taller*). Medium-Skill: Group 4 (assistants —*ayudantes no titulados*) and Group 5-7 (administrative workers —*oficiales administrativos* (5), *subalternos* (6) and *auxiliares administrativos* (7)). Low-Skill: Group 8-10: (manual workers —*oficiales de primera y segunda* (8), *oficiales de tercera y especialistas* (9) y *mayores de 18 años no cualificados* (10)).

Reason for job spell termination. Reported by the firm to the Social Security Administration. This variable is relevant for determining entitlements to severance pay, unemployment benefits, or family as well as health related benefits. Non-voluntary separations refer to the following codes 54, 77, 91, 92, 93 and 94. These keys identify individual as well as collective dismissals, or terminations of temporary contracts.[44]

Establishment. Defined by its Social Security contribution account (*codigo de cuenta de cotizacion*). Each firm is mandated to have as many accounts as regimes, provinces, and relation types with which it operates. The contribution accounts are assigned by the Social Security administration, and they are fixed and unique for each treble province-Social Security regime-type of employment relation.[45] Thus, contribution accounts can be thought of as establishments.

[44]Prior to 2012, codes 91 to 94 were included within code 54. Since we cannot differentiate these causes for the entire period, we include them all for consistency.

[45]According to the Social Security administration, around 85 percent of the firms are single unit organizations, i.e. there have just one contribution account per firm. Each firm has typically one account for each treble province-Social Security regime-type of employment relation.

Establishment creation date. Date when the first employee was registered in the contribution account. We rely on this date as a proxy for the workplace creation date to classify employers into age bins.

Establishment size. Number of employees working in the establishment at the moment of data extraction. Unfortunately, this variable is missing before 2005. For the years in which the variable is missing, we assigned the average size observed for that establishment from 2005 onwards. In the case of establishments not observed after 2005, we assigned a value of zero.[46]

Establishment location. The municipality in which the establishment conducts its activity if above 40,000 inhabitants, or the province for smaller municipalities (*domicilio de actividad de la cuenta de cotizacion*). Based on that, we group all locations into the 50 Spanish provinces.

Sector of activity. The MCVL provides information on the main sector of activity at a three-digit level (*actividad economica de la cuenta de cotizacion, CNAE*). Due to a change in the classification in 2009, the MCVL contains CNAE93 and CNAE09 for all establishments observed in business from 2009 onwards, but only CNAE93 for those which stop their activity before. We rely on the CNAE09 classification when available, and CNAE93 otherwise. Then, we aggregate the three-digit industry information into 11 categories corresponding to primary sector, manufacturing, utilities, construction, trade and transport, accommodation and restaurants, business services, public sector, private health institutions, education, and other services.

Socio-economic accounts. These accounts are obtained from the Groningen Growth and Development Centre and can be accessed through the following links (i) `https://www.rug.nl/ggdc/valuechain/wiod/wiod-2013-release` and (ii) `https://www.rug.nl/ggdc/valuechain/wiod/wiod-2016-release`.

[46] We tested our results by including a dummy variable in our regressions to identify firms not observed after 2004. However, our main results are not affected, so we avoid including such an indicator.

D Estimates using the Employer-Employee Panel

The Employer-Employee Panel (*Panel de datos de Empresas-Trabajadores*, PET, in Spanish) is an additional source of information constructed from administrative records of the Spanish Social Security. The dataset consists of an extraction of establishments (secondary contribution accounts of the General Regime) and their workers including the characterization of employers and the working lives of the employees. The sample is stratified by establishment size, using the following percentages: from 1-4 and from 5-9 workers, 3%; from 10-14 and from 25-49, 5%; from 50-99 and from 100-249, 8%; from 500-1,499 and from 1,500+, 15%. This extraction choice ensures the representativeness of each of the segments of the population of establishments. The data covers the period 2013 to 2016.

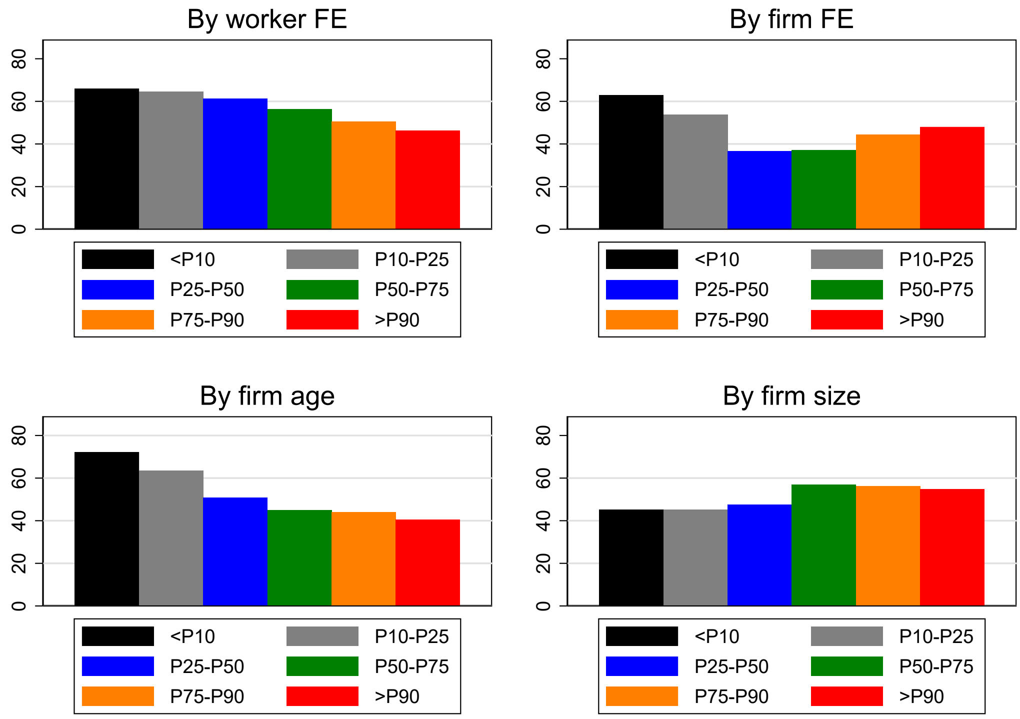
We construct a yearly panel in which the observation unit is the establishment that is part of the sample with the affiliation episodes of its workers in the reference year. We restrict the analysis to those workers born between 1973 and 2000, aged between 20 to 40, and use observations of contribution years from 1997 given that lack of information on the type of contract before that date. Using this sample, we fit linear wage models that include additive worker and establishment fixed effects as in Abowd et al. (1999), further controlling for workers' part-time status, age dummies, and year dummies. More precisely, we estimate models of the following form

$$w_{ijt} = \eta_i + \psi_{j(i,t)} + X_{ijt}\beta + \epsilon_{ijt} \tag{D.10}$$

where w_{ijt} are log-daily wages of worker i at time t in firm j, η_i is the unobserved worker effect, $\psi_{j(i,t)}$ is the unobserved effect of firm j where worker i is employed at t, X_{ijt} are covariates such as part-time status, age and calendar effects, ϵ_{ijt} is the error term.

Figure D.1 shows the FTC rates across worker and firm FE effects estimated from Equation (D.10), as well as the FTC rates across employer's age and size categories. The results confirm previous insights on the incidence of temporary contracts across sectors, occupations and industries, and reinforce the widespread use of temporary contracts in the Spanish economy.

Figure D.1: Fixed-Term Contract Rate across AKM FE and Firm Types



Source: PET dataset and own calculations. The figure shows the share of fixed-term contracts in PET establishments by worker and firm fixed effects (recovered from Equation D.10), firm age and firm size. The sample includes workers born between 1973 and 2000 who are between 20 and 40 years old between 1997 and 2016.