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Employment protection, job insecurity, and job mobility*

Marco Bertoni, Simone Chinetti, Roberto Nisticò

Abstract

We use the 2015 Italian Jobs Act as a natural experiment to study how reducing employment protection affects workers' perceived job insecurity and actual job loss, search behaviour, and mobility. The reform eliminated reinstatement rights for unfair dismissals and introduced tenure-based severance pay, applying exclusively to new hires in large firms. Using Labour Force Survey data and matched employer–employee records, we implement a difference-in-differences design combined with a sharp severance pay discontinuity at two years of tenure. We find that low-tenure workers hired under the new regime are 17% more likely to fear job loss. Consistent with these perceptions, their probability of being laid off increases by 2%. In response, they increase on-the-job search (+21%) and job-to-job mobility (+22–28%), particularly from low-paying sectors, where moves lead to higher wages. In high-paying sectors, workers instead raise effort (+3–5% days worked). All these effects disappear when severance pay increases by 50% after two years of tenure. Our findings highlight a central policy trade-off: while lower firing costs at low tenure levels foster mobility and labour market efficiency, they also increase job insecurity, calling for tenure-targeted unemployment insurance.

JEL classification: J22; J41; J62; J65.

Keywords: EPL; fear of job loss; job mobility; on-the-job search; severance pay.

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Highlights

- Italian Jobs Act cut employment protection and linked severance pay to tenure
- Low-tenure workers faced higher perceived insecurity (+17%) and actual job loss (+2%)
- Insecurity raised on-the-job search (+21%) and job-to-job mobility (+22–28%)
- Effects disappear once severance pay increases by 50% after two years of tenure
- Policy trade-off: higher mobility vs higher insecurity, calls for tenure-based UI

1 Introduction

Efficient labour markets rely on workers' ability to move quickly toward jobs where they are most productive, allowing economies to adjust to shocks while limiting adjustment costs. Employment protection legislation (EPL) plays a central role in this process. By insuring workers against dismissal, EPL provides valuable security, but higher firing costs can discourage firms from terminating unproductive matches and from creating new ones in the first place (Bentolila and Bertola, 1990; Pries and Rogerson, 2005). The theory thus points to a trade-off between insurance and efficiency, and empirical work shows that stricter EPL tends to slow worker flows and reallocation (Autor et al., 2004; Cahuc et al., 2019; Kugler and Saint-Paul, 2004; Saez et al., 2023).[1]

EPL affects not only hiring and separations, but also workers' behaviour. For example, entitlement to severance pay can discourage job changes or delay job finding after layoff (Cederlof et al., 2025), while weaker protection may heighten perceptions of job insecurity and influence job search and mobility (Garcia-Louzao, 2022; Gielen and Tatsiramos, 2012; Kettemann et al., 2017). Despite its centrality in policy debates, causal evidence on these behavioural responses remains scarce.

This paper aims to close this gap by studying how weakening employment protection affects workers' perceived and actual job insecurity, as well as their behavioural responses in terms of job search, mobility, and effort. We exploit the 2015 Italian Jobs Act, which eliminated reinstatement rights for unfair dismissal under open-ended contracts and replaced them with tenure-dependent severance pay. Before the reform, permanent workers could be dismissed only for just cause and had the right to appeal in labour courts. Expected firing costs were therefore very high—estimated at close to 40 months of pay (Gianfreda and Vallanti, 2017)—reflecting the combination of reinstatement rights and lengthy, uncertain

[1] See also Boeri and Garibaldi (2019), Boeri and Jimeno (2005), and others for Italy; Fraisse et al. (2015), Pérez and Osuna (2014), and Von Below and Thoursie (2010) for France, Spain, and Sweden; and Cahuc and Postel-Vinay (2002), Gómez-Salvador et al. (2004), Messina and Vallanti (2007) for cross-country evidence.

court proceedings.[2]

After the reform, compensation was set at four months of wages for workers with up to two years of tenure and increased by two months for each additional year, up to a maximum of 24 months. This "graded security" schedule generates a sharp discontinuity at the two-year threshold, where severance pay increases by 50%, from four to six months of wages.

These provisions applied only to workers hired on open-ended contracts after March 7, 2015 in large firms (i.e., those with more than 15 employees). Workers hired earlier in large firms continued to enjoy reinstatement rights, while hires in small firms remained unaffected regardless of the hiring date. This combination of treatment and control groups, together with the discontinuity in severance pay at two years of tenure, underpins our difference-in-differences design, which allows us to identify the effects of weaker employment protection on perceived and actual job loss, search intensity, effort and mobility, and how these effects evolve once severance pay increases at the two-year cutoff.[3]

Understanding how EPL changes affect workers' behaviours is particularly relevant in Europe, where labour market rigidities have long been associated with strong employment protection (Blanchard and Portugal, 2001). Past deregulation efforts often liberalised temporary contracts while leaving permanent ones largely untouched, contributing to labour market dualism and precarious employment (Behaghel et al., 2008; Berton and Garibaldi, 2012; Boeri and Garibaldi, 2007; Daruich et al., 2023). In response, several scholars and policymakers have advocated for unified contracts with graded security, in which severance pay increases smoothly with tenure, aiming to combine mobility and insurance.[4]

We draw on both survey and administrative sources. Perceived job insecurity and other

[2]Although unfair dismissal cases were relatively rare—about 3,000 per year (Boeri and Garibaldi, 2019)—these potential liabilities acted as a strong deterrent to both dismissals and new permanent hires.

[3]The Jobs Act also introduced a temporary hiring incentive for new open-ended contracts starting January 1, 2015. The subsidy, available to both large and small firms, covered social security contributions up to €8,060 per year for three years. While it stimulated job creation, particularly among smaller and lower-wage firms, it did not alter the employment protection rules at the core of our analysis (Boeri and Garibaldi, 2019).

[4]Proposals of this kind have been debated in France (Blanchard and Tirole, 2008; Cahuc, 2012; Cahuc and Kramarz, 2005), Italy (Boeri and Garibaldi, 2008; Boeri et al., 2017), and Spain (Bentolila et al., 2008, 2012; Pérez and Osuna, 2014).

self-reported outcomes come from the Italian Labour Force Survey (LFS), a quarterly survey of about 75,000 households conducted by the national statistical office (ISTAT). We complement these data with matched employer–employee records from the Italian Social Security Institute (INPS), which provide longitudinal information on employment histories. Together, these data allows us to capture both perceptions and realised outcomes, providing a comprehensive view of how the Jobs Act affected workers.

Our analysis shows that workers hired under the new regime experienced higher perceived and actual job insecurity at low tenure: fear of job loss rose by about 17% and the layoff probability increased by 2%.[5] In response, workers intensified on-the-job search (+21%) and job-to-job mobility (+22–28%), especially in low-paying sectors, where moves were associated with higher wages. In high-paying sectors, by contrast, workers responded mainly by increasing effort, working 3–5% more days per year.

Importantly, all these effects vanish once severance pay increases by 50% after two years of tenure. This result is novel in the literature and highlights that the impact of eliminating protection against unfair dismissal on workers' insecurity and mobility is substantially weakened by the provision of higher severance pay, thereby building a bridge between the analysis of wrongful discharge laws and the study of severance pay.

Our contribution to the literature is twofold. First, we extend the literature on wrongful discharge laws, which has mainly examined employment, wages, and productivity (Autor et al., 2004, 2007; Bassanini et al., 2009; Bjuggren, 2018; Butschek and Sauermann, 2022; Cappellari et al., 2012; Cingano et al., 2010, 2016; Leonardi and Pica, 2013; MacLeod and Nakavachara, 2007; Martins, 2009). We provide new evidence on workers' search, mobility, and effort responses to greater insecurity.

Second, we add to the limited evidence on severance pay (Boeri et al., 2017; Cahuc et al.,

[5]The welfare relevance of job insecurity is well established in the literature, as it affects behaviours, well-being, and major life choices, such as marriage, childbearing, homownership. See, among others, Böckerman et al. (2011), Clark and Postel-Vinay (2009), Origo and Pagani (2009), Georgieff and Lepinteur (2018), László et al. (2010), Lepinteur (2021), De Paola et al. (2021), Mistrulli et al. (2023), Clark and Lepinteur (2022), Clark et al. (2023), Ruiz-Valenzuela (2020), Barceló and Villanueva (2016), Clark et al. (2022).

2019; Cederlof et al., 2025; Garibaldi and Violante, 2005; Jappelli and Padula, 2016; Kugler, 2005; Lazear, 1990; Marinescu, 2009). Existing work shows that stricter protection reduces quits in cross-country settings (Gielen and Tatsiramos, 2012) and that reforms lowering severance pay in specific contexts, such as mass layoffs, can increase mobility (Kettemann et al., 2017) and quits (Garcia-Louzao, 2022). We complement this evidence by exploiting a comprehensive labour market reform that simultaneously changed dismissal rules and severance schedules for all new open-ended hires.

Our findings highlight a central policy trade-off. Reducing employment protection at low tenure levels raises insecurity but can foster reallocation and upward mobility. Effective labour market design therefore requires complementary policies—such as unemployment insurance targeted to short-tenure workers—that mitigate insecurity without eroding efficiency gains.

2 Employment protection legislation in Italy and the 2015 Jobs Act

Historically, permanent employees in Italy have enjoyed strong protection against dismissal. Under Article 18 of the 1970 Labour Rights Charter, layoffs were allowed only for objective (economic) or subjective (misconduct) just cause. In cases of unfair dismissal confirmed by a court, the employer was required to reinstate the worker or provide a compensation of 15 months' pay, plus the foregone earnings and social security contributions. This regime only applied to firms with more than 15 employees, creating a size-based dualism in firing costs. Law 108/1990 partially addressed this asymmetry by extending protection to smaller firms, but allowed them to choose between reinstatement and a capped compensation package.

A first step toward greater flexibility came with the 2012 Monti-Fornero Reform (Law 92/2012). This law limited reinstatement to discriminatory, null, or oral dismissals and made monetary compensation the main remedy in most other cases, subject to caps on the

maximum award. By reducing both the frequency of reinstatement and the potential liability for employers, the reform lowered the expected cost and unpredictability of dismissals (see Bottasso et al., 2025).

At the end of 2014, the center-left government led by Prime Minister Matteo Renzi obtained a broad parliamentary mandate to reform labour market legislation. Building on this trajectory, the Jobs Act (Law 183/2014 of December 20, 2014) introduced a new *graded security contract* for all permanent hires in firms above the 15-employee threshold after March 7, 2015. Reinstatement was restricted to discriminatory and a few specific disciplinary cases. All other unfair dismissals were compensated with a tenure-dependent severance payment: four months of wages within the first two years, and two additional months for each year thereafter, up to a maximum of 24 months at twelve years of tenure. The law also established an optional out-of-court conciliation procedure, under which the employer could avoid litigation by paying a lump-sum allowance equal to two months' wages in the first two years plus one month per additional year, capped at eighteen months. These pre-set schedules not only reduced firing costs in large firms, but also removed the uncertainty associated with court rulings.

The reform applied only to new permanent hires in large firms. Existing permanent workers in such firms retained Article 18 protection, granting reinstatement, while contracts in small firms were already governed by more flexible rules, with severance capped at six months' pay. In addition, if a small firm crossed the 15-employee threshold after March 7, 2015, the Jobs Act stipulated that all of its permanent employees - both existing and new - would fall under the new dismissal rules. Importantly, the graded security mechanism was weakened by a decision of the Constitutional Court dating 26 September 2018 and published on 8 November 2018, ruling that tying compensation solely to tenure was unconstitutional and restoring judicial discretion in determining compensation within the statutory bands prescribed by the Jobs Act.

In parallel, the 2015 Budget Law introduced a hiring incentive for permanent contracts

starting on or after January 1, 2015. The measure granted employers a three-year exemption from social security contributions, with a cap at €8,060 per year and worker. As a result, the subsidy exempted employers from paying social security contributions for gross wages up to about €24,000. For wages above that level, there was no full exemption of employers from social security contributions. Only workers without a permanent position in the previous 6 months were eligible for the tax rebate. Importantly, the subsidy applies to both large and small firms.[6]

3 Data

3.1 Italian Labour Force Survey (LFS)

We use repeated cross-sections from the Italian Labour Force Survey (LFS), a quarterly survey covering about 75,000 households per wave. The LFS collects detailed information on labour market status and socio-demographic characteristics for a representative sample of the Italian population.

Since the new graded security contract applies only to permanent full-time employees in the private sector, our sample is restricted to workers hired under these contractual arrangements between 2010 and 2018, and surveyed between the first quarter of 2013 and the third quarter of 2018.We start in 2013 because questions on perceived job insecurity—our main outcome—were introduced only that year. To examine the effects of increasing severance pay with tenure, we focus on employees with up to three years of tenure, distinguishing between those with up to two years (entitled to four months of severance pay) and those with three years (entitled to six months). We stop at three years because this matches the duration of the hiring subsidy introduced by the Jobs Act: dismissals may increase after the subsidy expires, generating selective attrition (Ardito et al., 2025). Moreover, we restrict the sample to 2018q3, just before the Constitutional Court ruling that weakened the ex-ante

[6]The size and availability of the subsidy also changed over time. See Ardito et al. (2020) for details.

determination of severance payments for Jobs Act hires in large firms. Table A1 shows the range of observed tenures by hiring and interview year.

Our main measure of perceived insecurity is based on the LFS question: "Do you think it is likely that you will lose your current job in the next six months?", with yes/no answers.[7] The survey also provides information on on-the-job search ("Are you looking for another job?", yes/no), and on labour market outcomes potentially influenced by insecurity, such as hours worked, overtime, weekend work, and monthly net earnings.

According to the Jobs Act, the new regime applies only to workers hired from March 7, 2015 in firms with more than 15 employees. Workers in small firms or hired in large firms before that date remained under the previous regime (see Section 2). We define a dummy variable *Large* equal to 1 for firms above the 15-employee threshold at the time of the interview, and 0 otherwise.[8] As the LFS does not provide the exact hiring date or interview week, we approximate the hiring date by imputing the interview to the mid-point of the known interview quarter (i.e., mid-February for Q1, mid-May for Q2, etc) and subtracting reported job tenure (in months). We then define a dummy *Jobs Act* equal to 1 if the imputed hiring date falls after March 7, 2015, and to 0 otherwise.

Our final LFS sample includes 43,356 observations without missing values for gender, age, education, tenure, immigrant status, marital status, job position (blue- or white-collar), 1-digit ATECO sector, region, quarter, and year.

Table A2 reports descriptive statistics. One-third of workers are women, the average age is 39 years, average schooling is 11 years, half are married, and 24% are immigrants, 79% reside in Northern regions, 68% are blue-collar workers, and average monthly earnings are €1,244. About 44% work in large firms, 32% were hired after the Jobs Act, and 14% are in both large firms and hired after March 7, 2015 - thus subject to the new contract. Average tenure in the job is 1.7 years: 59% of workers have a tenure level of at most 2 years, and 41%

[7] As an alternative measure, we use job stability satisfaction, assessed on a 0–10 scale.

[8] Firm size is reported in brackets (up to 9; 10–15; 16–19; 20–49; 50–249; 250+). We exclude workers who answered "don't know" or "don't know, but more than 10 employees", while we classify as small firms those who answered "don't know, but fewer than 10 employees."

are in their third year at the firm. Regarding outcomes, 9% fear losing their job in the next 6 months, 4% are searching for a new job, the average workweek is just over 40 hours, 1% are on sick leave during the reference week, 5% do overtime, and 42% work during weekends.

3.2 LoSai (Longitudinal Sample INPS) administrative records

We complement these data with administrative records from LoSai (Longitudinal Sample INPS), a matched employer–employee dataset managed by the Italian Social Security Institute. LoSai covers about 7% of employees in the non-agricultural private sector between 1985 and 2018, based on workers born on the 1st and 9th day of each month.

The dataset records individual employment and earnings histories, including annual gross earnings, days worked per year, contract type (full-/part-time, permanent/temporary), and broad occupational categories (apprentice, blue-collar, white-collar, middle manager, manager). It also reports demographics (year of birth, gender, region), and firm characteristics: size (in brackets)[9] and 2-digit industry (NACE Rev. 2).

Given that this sample is far larger than the LFS, we can consider permanent full-time contracts started between March–December 2014 and March–December 2015, a narrower window around the Jobs Act introduction in March 2015. This choice also permits a follow-up of up to three years (until December 2018) without right-censoring, but we again limit our sample to September 2018 to avoid contamination from the Constitutional Court ruling.

From this dataset, we construct three samples to document separations, job-to-job mobility and effort - measured by days worked.[10] Details on sample construction are reported in Appendix 1.

[9] Firm size brackets are 0–5, 6–10, 11–15, 16–20, 21–25, 26–30, 31–40, 41–50, 51–100, 101–200, 201–300, 301–400, 401–500, $\geq$ 500, based on INPS "firm labour force", a full-time equivalent measure considered the most reliable proxy for the 15-employee threshold. See Boeri and Garibaldi (2019).

[10] No measure of hours worked is recorded in the data. While workers arguably have more scope to adjust hours than days, they can still do overtime work on weekends, reduce holidays or sick leave.

4 Empirical strategy

We exploit the Jobs Act as quasi-experimental variation in employment protection. The reform lowered protection for workers hired in large firms after March 7, 2015 and, via the graded-security schedule, raised severance pay discretely at two years of tenure. Conditional on tenure, we estimate difference-in-differences (DiD) models that compare workers in large versus small firms, before versus after the reform.

For the LFS, we estimate by Ordinary Least Squares (OLS) the following model:

$$Y_{i,t} = \alpha + \beta LARGE\ FIRM_{i,t} + \gamma JOBS\ ACT_{i,t} + \delta LARGE\ FIRM_{i,t} \times JOBS\ ACT_{i,t} + \theta X_{i,t} + \lambda_t + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is an outcome of interest observed for worker i at the survey interview carried out in time period t; $LARGE\ FIRM_{i,t}$ is a dummy equal to 1 for employees in large firms (more than 15 employees) and to 0 otherwise; $JOBS\ ACT_{i,t}$ is a dummy equal to 1 for employees hired after 7 March 2015 and to 0 otherwise; $LARGE\ FIRM_{i,t} \times JOBS\ ACT_{i,t}$ is an interaction term whose coefficient δ measures the treatment effect of interest. X_{it} includes age (and age^2), gender, education, marital status, immigrant status, occupation (blue/white collar), region, and 1-digit ATECO sector. λ_t are quarter-by-year of interview dummies. Standard errors are clustered at the Large$\times$quarter-year level (Bertrand et al., 2004). We estimate (1) separately for tenure bins [0,2] and (2,3] years, and test for equality of δ across bins using seemingly unrelated estimation. The comparison of the impacts of the Jobs Act across tenure levels is informative about the role played by the change in severance pay, which increases substantially at the 2-year tenure cutoff.

Several challenges threaten the validity of our identification strategy, and we describe how we address them in Section 5.2 below.

The approach we adopt to investigate the effects on separations, job-to-job mobility, and workers' effort using administrative data is very similar. Our specifications differs from Equation (1) as, given the longitudinal structure of the sample, we replace the vector of individual controls with workers fixed effects, and consistently cluster standard errors at the

worker level.

5 Empirical results

5.1 Effects on perceived job insecurity

Table 1 reports in a 2×2 matrix the share of workers fearing job loss by firm size and hiring cohort, separately by tenure.

Table 1: Fear of Job Loss in Small and Large Firms Before and After the Jobs Act

	Before JA	Obs.	After JA	Obs.	After-Before JA Diff.
Tenure level: [0-2]					
Small Firm	0.145	8635	0.081	5536	-0.064***
	(0.004)		(0.003)		(0.005)
Large Firm	0.079	5536	0.040	4841	-0.039***
	(0.004)		(0.003)		(0.005)
Large-Small Firm Diff.	-0.066***		-0.041***		0.025***
	(0.005)		(0.004)		(0.007)
Tenure level: (2-3]					
	Before JA	Obs.	After JA	Obs.	After-Before JA Diff.
Small Firm	0.094	8158	0.059	1177	-0.035***
	(0.003)		(0.007)		(0.008)
Large Firm	0.063	7293	0.031	1132	-0.032***
	(0.003)		(0.004)		(0.006)
Large-Small Firm Diff.	-0.031***		-0.028***		0.003
	(0.004)		(0.006)		(0.010)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Irrespective of tenure level, employees in large firms report lower job insecurity. Moreover, workers hired after March 2015 also perceive lower fear of job loss - most likely because of better macroeconomic conditions at the time of hiring. The resulting difference-in-differences estimate points to a positive effect of lower employment protection on fear of job loss for workers with 0-2 years of tenure, who are entitled to a low severance pay. We find an increase in the share of workers reporting that they fear job loss of 2.5 percentage points, or roughly

17% of the control group mean (equal to 14.5%). However, this effect disappears completely for workers with 3 years of tenure, for whom severance pay is increased.

The OLS estimates of the treatment effects from Equation (1) are reported in Table 2. Despite the inclusion of interview quarter-by-year dummies and a comprehensive set of individual level controls (gender, age and age squared, education, marital status, immigrant status, region dummies, occupation - white or blue collar - and 1-digit ATECO sector dummies), the estimated effects are virtually indistinguishable from those obtained from the simple comparison of sample means across hiring date and treatment groups, reported in Table 1. This is a reassuring finding in terms of the internal validity of the design. Table 2 also reports a test for the equality of the effects across the two sub-samples, that is rejected with a p-value below 0.05.

Table 2: The Effect of the Jobs Act on Fear of Job Loss.

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
Large×Jobs Act	0.024***	0.004
	(0.008)	(0.005)
Observations	25596	17760
Mean Dep. Var. Large Firm=0, JA=0	0.145	0.094
Chi^2	5.88	
p-value	0.015	

Notes: Each column reports estimates from OLS regression. The dependent variable, Fear of job loss, is a dummy taking value 1 for workers reporting fear of losing their job within the next 6 months. All specifications include the following controls: interview quarter-by-year dummies, gender, age and age squared, education, marital status, immigrant status, region dummies, occupation - white or blue collar - and 1-digit ATECO sector dummies. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Probing validity

Several threats challenge the internal validity of our identification strategy.

First, our empirical design hinges on the common-trends assumption, that is, in the absence of the Jobs Act, outcomes would have evolved similarly in both treatment and control groups. The standard event-study specification à la Autor (2003), which would

allow us to test this assumption directly, is unfortunately unfeasible in our setting. Once we condition on job tenure, hiring time and interview time are perfectly collinear (see Table A1), so that for a given tenure level and interview date we can only identify a single post-reform contrast between workers hired before and after the introduction of the Jobs Act.

To probe the validity of the common-trends assumption, we conduct a series of placebo exercises using only pre-reform data. Specifically, we simulate fictitious "Jobs Act" introductions between 2013Q2 and 2014Q4 and re-estimate Equation 1 by interacting each placebo reform with the $LARGE_{i,t}$ dummy.

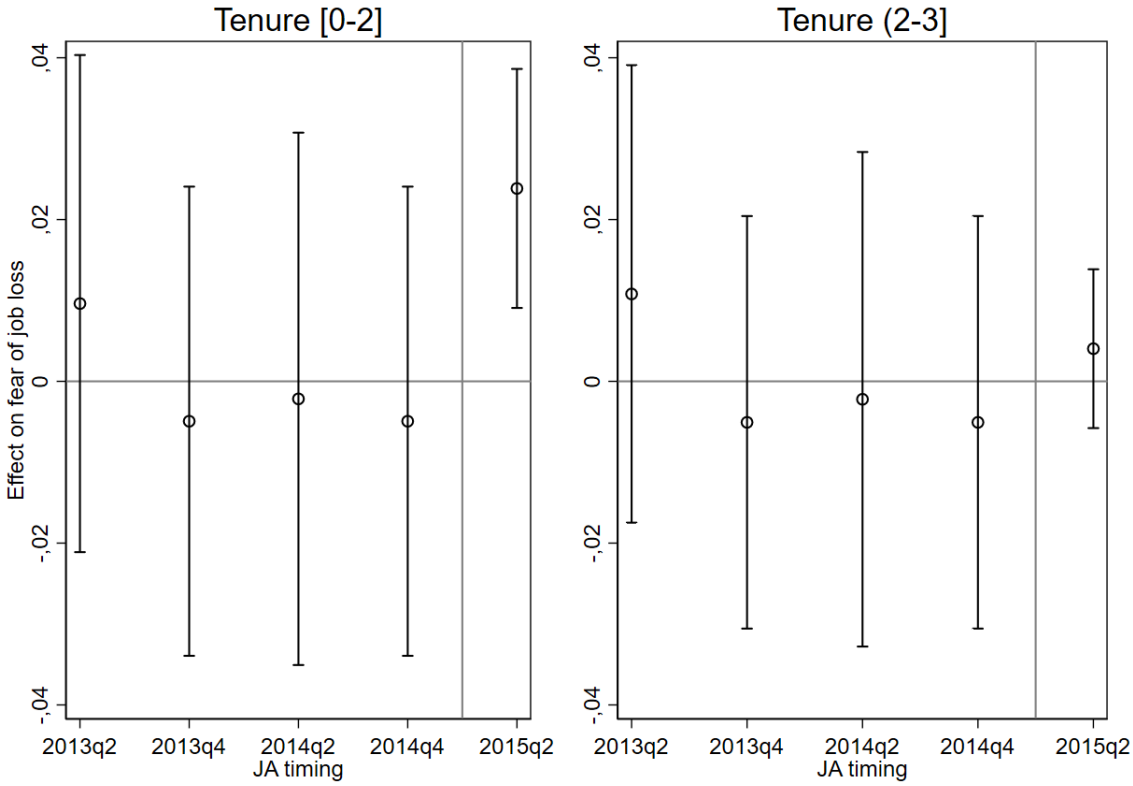
Figure 1 reports the resulting coefficients together with the baseline estimate for the actual reform (taken from Table 2) for comparison. This setup allows us to visually assess whether any differential trends between large and small firms emerged before the true policy change.[11] As shown in the Figure, none of the placebo interactions is statistically significant at conventional levels, supporting the absence of differential pre-trends between treated and control units.[12]

Second, selection into employment may also have changed in response to the introduction of the Jobs Act. On the one hand, the JA hiring subsidy boosted open-ended hirings, especially among small firms. At the same time, the reduction in firing costs brought about by the graded security contract stimulated hirings in large firms by lowering the expected cost of future separations (Boeri and Garibaldi, 2019 document both facts). These mechanisms likely induced composition changes in the pool of employed workers across firm sizes. The graded security contract may also have affected the distribution of workers by tenure, particularly between small and large firms. If separations were random, there would be no

[11] The narrower confidence intervals observed for the actual post-reform estimate simply reflect the larger effective sample size in that specification, where all workers hired before and after March 2015 are pooled together, rather than a genuine increase in precision over time. Furthermore, unlike standard event-study designs, we report a single post-treatment estimate. This choice follows directly from the structure of our data: conditional on tenure, hiring and interview timing move one-for-one, making it impossible to separately identify dynamic treatment effects. Our analysis thus focuses on the average post-reform contrast, while the pre-reform placebos serve to validate the identifying assumption.

[12] These estimates also rule out concerns on potential lingering effects of the previous 2012 EPL reform introduced by the Monti-Fornero government.

Figure 1: Placebo tests for common trends



Notes: the figure reports the interaction term between fictitious Jobs Act reforms spanning from 2013Q2 to 2014Q4, obtained from specifications akin to Equation 1, estimated in the pre-Jobs Act sample. Each reported estimate is from a different OLS model. The baseline estimate for the actual Jobs Act implentation - see Table 2 - is also reported for comparison. The dependent variable, Fear of job loss, is a dummy taking value 1 for workers reporting fear of losing their job within the next 6 months. All specifications include the following controls: interview quarter-by-year dummies, gender, age and age squared, education, marital status, immigrant status, region dummies, occupation - white or blue collar - and 1-digit ATECO sector dummies. 95% confidence intervals clustered at Large Firm*Year-Quarter level are also reported.

issue with our strategy, as survivors would be a representative sample of the workforce. But separations are not random, as employers tend to retain the best matches.

We assess the robustness of our findings to changes in composition of the workforce across the four subgroups we compare, and by tenure, by leveraging information on workers' observable characteristics. The results are reported in Table A4. First, Panel A shows that our results are robust to dropping individual-level controls, suggesting that selection along these observed variables is not a concern for the internal validity of our design. Given that our model includes several covariates, we also verify in Panel B that the results are

robust when, instead of using OLS, we rely on the robust imputation estimator developed by Borusyak et al. (2021), that only uses pre-intervention information to estimate coefficients related to the covariates. Finally, in Panel C we re-weight within firm size each cell defined by the interaction of $JOBSACT$ and tenure (0-2 vs. 2-3) so that they are comparable to the pre-Jobs Act group with 0-2 tenure years on the basis of their observed characteristics, using the entropy balancing (Hainmueller, 2012). The stability of the results of these tests are reassuring about the irrelevance of compositional changes.[13]

Another potential concern about our design is related to firm size. On the one hand, the sorting of workers into large and small firms could have changed as a result of the introduction of the Jobs Act. On the other hand, by making the firing regulations for large firms more similar to those in place for small firms, the Jobs Act may have induced firms to pass the 15-employee threshold(see also Boeri and Garibaldi, 2019; Garicano et al., 2016).

We first deal with these concerns by trimming our sample. Internal validity considerations may lead researchers to zoom-in around the relevant size cutoff. At the same time, marginal firms are the more likely to pass the threshold as a result of the Jobs Act introduction, suggesting a donut-hole approach. Results for both strategies are reported in Table A6, and are comparable to our baseline.

As a complement, we also adopt an Instrumental Variable (IV) strategy. Following De Paola et al. (2021), we use the fraction of workers employed in large-firms (with 50 or more employees) operating in the previous year (for workers interviewed in 2013-2014) or in 2014 (for those interviewed in 2015-2019) in the same sector and the same region in which the worker is currently employed as an instrument for working in a large firm. The interaction term $LARGE_{i,t} \times JOBS\ ACT_{i,t}$ is also instrumented by the interaction between the instrument and the $JOBS\ ACT_{i,t}$ dummy.

[13]We further assess the robustness of our findings to potential selection at the first step of the graded security mechanism by re-estimating Equation 1 after including in the 2-3 years tenure sample all workers in the 0-2 years tenure group reporting fear of job loss. We then assess whether the $LARGE_{i,t} \times JOBS\ ACT_{i,t}$ coefficient is altered or not by the inclusion of low-tenured workers perceiving job insecurity. The estimates in Table A5 reassure us, as the effect of interest is insignificant and, if anything, it turns negative.

The data we use come from ISTAT's ASIA ("Archivio Statistico delle Imprese Attive") Archive for years 2012–14. As reported in Table A2 the average value of the instrument is close to 5.5%. Figure A1 shows the distribution of the IV across economic sector of activity and regions, displaying a large amount of variability along both dimension.

The first stage of the strategy leverages the fact that whether a worker is employed by a small or a large firm depends in part on the structure of the firms in the area in which he/she lives, proxied by the fraction of large-firm employees that were active in previous years in the region and in the sector in which the worker is employed.[14] Concerning the exclusion restriction, the instrument effectively measures a feature of the local labor market that may matter for workers' ability to keep the current job or finding a new one if displaced conditional on the size of the current firm, with a direct effect on feelings of job insecurity. So long as this effect is not altered by the introduction of the Jobs Act, however, it should not matter for the identification of the difference-in-difference effect.[15]

The top panel of Table 3 reports the fist-stage coefficients. Our chosen instrument - the fraction of employees in large firms (50 or more employees) operating in the previous year (for workers interviewed in 2013-2014) or in 2014 (for those interviewed in 2015-2019) in the same sector and region in which the worker is currently employed - is strongly predictive of whether workers are currently employed in a large firm.[16] For both sample, the first-stage coefficient for working in a large firm is roughly equal to 0.8 percentage points and is strongly significant, with a value of the Kleibergen-Paap F statistic above 10 in both samples. Considering that in our final sample the standard deviation of the instrumental variable is equal to 8.6 percentage points, a first stage coefficient of 0.8 percentage points implies that a 1SD increase in the value of the instrument would increase a worker's probability of being

[14] In our main analysis a firm is considered large if it has more than 15 employees as stated by the respondent, while due to data limitations in our IV analysis we use 50+ employees as definition for large firms.

[15] Eventually, this strategy also dispels concerns related to errors in the measurement of firm size, that in the LFS is self-reported by workers.

[16] Results are comparable results when, instead of adopting a time varying definition, we define the IV using the 2013 or 2012 shares of employees in large firms for all workers.

employed in a large firm by 6.9 percentage points, or 16% of the sample mean (43.7 percentage points).

The second-stage coefficients are reported in the bottom panel of Table 3. Consistent with our main OLS specification of Table 2, we find a positive and significant effect on fear of job loss of being hired with the graded security contract at the lower tenure level (0-2 years), that becomes insignificant at the higher tenure level (3 years). In the low-tenure sample, where we find significant treatment effects, the magnitude of the IV effect is larger than the OLS one, 0.051 vs. 0.025. This is consistent with the attenuation bias due to measurement error in self-reported firm size, as well as with the possibility of a positive change in the composition of workers employed in large firms after the Jobs Act along unobservable determinants of job security. As a result, we deem OLS as more conservative (as well as more precise) than IV, and use it as a benchmark in what follows.[17]

Table 3: The Effect of the Jobs Act on Fear of Job Loss. IV Estimates.

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
Second-stage:		
Large×Jobs Act	0.051**	0.017
	(0.023)	(0.022)
First-stage for Large Firm:		
% Large-Firm Employees	0.009***	0.008***
	(0.002)	(0.002)
Kleibergen-Paap rk Wald F-statistic	25.43	11.34
Observations	23385	16247

Notes: Each cell reports estimates from 2SLS regression. The instrumental variable, % of Large-Firm Employees, refers to the fraction of employees in large firms (50 or more employees) operating in the previous year (for workers interviewed in 2013-2014) or in 2014 (for those interviewed in 2015-2019) in the same sector and in the same region in which the worker is currently employed. We adopt the specification used in Table 2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7 presents more exercises aimed at probing the robustness of our main results in Table 2. Panel A illustrates instead that the inclusion of region-specific linear time trends does not alter our estimated effects, ruling out the possibility that local-level trends may

[17]This choice is also motivated by the results of a cluster-robust Hausman test for the exogeneity of the $LARGE_{i,t}$ and $LARGE_{i,t} \times JOBS\ ACT_{i,t}$ variables, that fails to reject the null hypothesis with p-values above 0.05 for both the high- and the low-tenure samples.

confound our identification strategy. Panel B shows that the exclusion from our sample of workers hired in 2010 and 2018 - that we only observe with tenure up to two years and above 2 years, respectively (see Table A1) - does not alter the results. Next, to address the potential confounding effect of residual variation in tenure within the (0,2] and [2,3] samples, Panel C shows that the results remain unchanged when we include month-of-tenure dummies. Panel D excludes workers hired within a 45-day window around the March 7, 2015 threshold marking the implementation of the Jobs Act, thereby mitigating concerns about possible misclassification of workers across this threshold. Finally, Panel E illustrates that we obtain comparable results when we use as an alternative dependent variable a dummy equal to 1 for workers who report a satisfaction with job stability above the median value, that is equal to 7/10.

A final concern is that the effects on fear of job loss may differ by gender, age, previous employment contract, area of residence, occupation and earnings level. We report heterogeneous effects in Table A8. Results show that the higher job insecurity induced by the lower severance pay is concentrated only among males. However, as we are exploring heterogeneous effects across several observable dimensions, "false positive effects" are a non-negligible concern for statistical inference. Since the significance of the interaction terms is never above 5%, we take this evidence as suggestive more than conclusive.

5.3 Effects on actual employment continuation rates

Do perceived-insecurity effects mirror actual layoffs? To test this, we use administrative data to investigate whether employment continuation rates every semester - i.e., the probability of not having been yet laid off by the firm - differ across workers hired in large vs small firms and before vs after the Jobs Act. According to Figure 2 and Table A9, the patterns observed for continuation rates match those for perceived job insecurity. Our evidence consistently shows that low-tenure graded security workers are less likely to be retained during the first 2 years of the contract relative to those with a pre-reform open-ended contract. In other words,

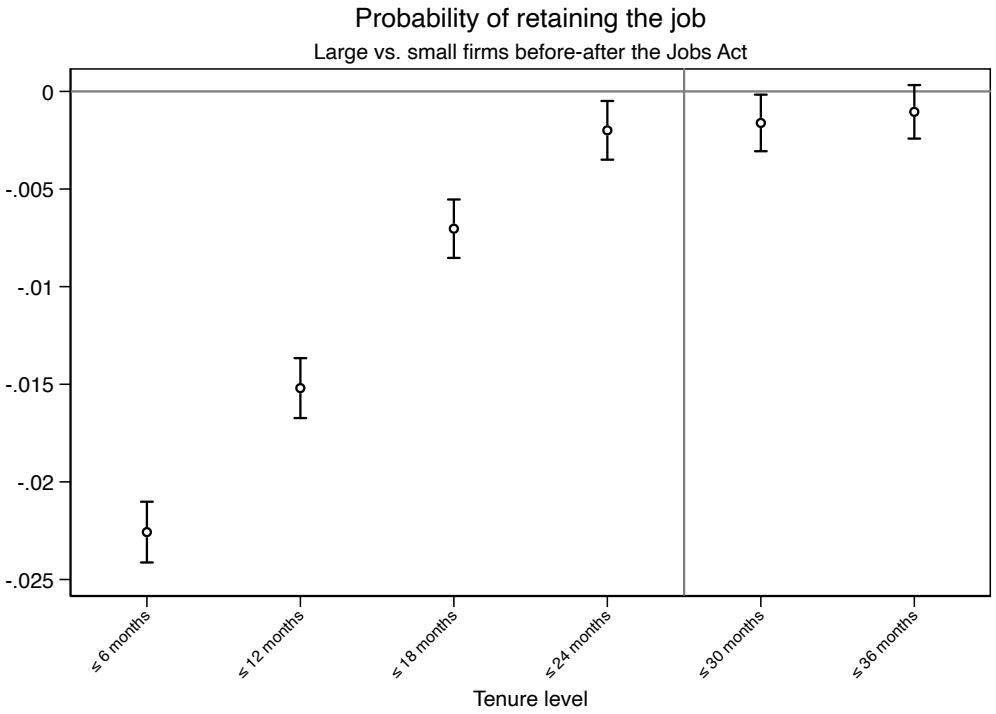
the share of workers laid off during the first two years of tenure is 2% lower in the treated than in the control group. This effect declines in magnitude with tenure, and vanishes after two years from hiring, when workers achieve the second step of the graded security contract. When compared to the control group mean for each tenure level, the impact on continuation rates declines from -9.2% to -0.4% as tenure increases from 6 to 36 months. A possible interpretation of these findings is that the loss in EPL from lower reinstatement rights is totally offset by gains in severance pay for high-tenure workers, whereas this loss is greater for lower-tenure workers.

The gradient in Figure 2 can be interpreted as revealing firms' willingness to anticipate layoffs in order to avoid higher severance payments. Comparing average effects one year before and one year after the two-year tenure threshold indicates a difference in the layoff probability of about 0.25 percentage points for a two-month increase in severance pay, corresponding to 0.125 percentage points per month. Given that one month of severance pay equals one month of salary, on average €1,515 (see Appendix Table A3), this slope implies that, by anticipating layoffs before the two-year cutoff instead of paying the higher severance, firms save on average about €2 per worker (0.00125 × 1,515).

5.4 Effects on job search, mobility and effort

Our results on the effect of the Jobs Act on perceived and actual job security suggest that, with respect to workers hired with a permanent contract, those hired with the graded security contract initially perceive and face a higher layoff risk. However, this effect disappears when treated workers achieve the second step of the graded security scale and are entitled to a 50% higher severance pay. Does the heightened layoff risk experienced by early-stage treated workers trigger responses on other labour market outcomes, such as on-the job search, job-to-job mobility and effort provision? In what follows, we assess this matter from several perspectives.

Figure 2: Continuation Rates



Notes: The Figure reports the difference-in-differences estimates available in Table A9. The dependent variable is the probability of retaining the job in firm f at month tenure m.

5.4.1 On-the-job search

A potential response to a higher layoff risk is to increase job search effort while employed. We investigate this matter in Table 4, where we present our OLS estimates using self-reported on-the-job search from the LFS data as the outcome. We estimate effects in the full sample and after differentiating between sectors where workers earn average yearly earnings above vs. below the median. Looking at Column (1), we find that - on average - graded security workers with 0-2 years of tenure are 1.3 percentage points more likely than workers on pre-reform contracts to report on-the-job search activities. This effect is large, and is equivalent to an increase of 23% with respect to the control group mean outcome. As shown in Column (2), the effect flips sign in the higher tenure group, endowed with the higher severance pay. Furthermore, Column (3) shows that the effect on low-tenure workers is smaller for those employed in high paying sectors - defined as those sectors whose hourly wage is above the

Table 4: The Effect of the Jobs Act on On-the-Job Search.

	Tenure level:			
	[0-2]	(2-3]	[0-2]	(2-3]
	(1)	(2)	(3)	(4)
Large×Jobs Act	0.013**	-0.018***	0.025***	-0.007
	(0.005)	(0.005)	(0.008)	(0.010)
Large×Jobs Act*High-paying Sectors			-0.016	-0.018
			(0.010)	(0.013)
Observations	25929	17917	25929	17917
Mean Dep. Var. Large Firm=0, JA=0	0.057	0.037	0.057	0.037

Notes: Each column reports estimates from OLS regression. The dependent variable, On-the-job search, is a dummy taking value 1 for workers reporting any on-the-job search activity. All specifications include Large and Jobs Act dummies and the following controls: interview quarter-by-year dummies, gender, age and age squared, education, marital status, immigrant status, region dummies, occupation - white or blue collar - and 1-digit ATECO sector dummies. Column (3)-(4) also includes the dummy High-Paying Sector and its interaction with the dummies Large and Jobs Act. High-paying sectors are defined as those those with hourly wage above the yearly specific median. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

yearly specific median. Although the difference between workers in the two sectors is not statistically significant, the resulting effect for workers in high paying sectors is much smaller than the one for worker in low-paying ones, and significantly indistinguishable from zero. For the high-tenure group, the effects are of the same sign for both sectors.

5.4.2 Job-to-job mobility

Does the higher job search effort exerted by graded security employees pay off? We investigate this using our administrative data to estimate effects on job-to-job mobility from a permanent full-time contract to a new one. The evidence in Panel **a** of Figure 3 and of Table A10 shows that job-to-job mobility is higher for graded security workers compared to those employed on open-ended contracts with up to 2 years of tenure, and this effect vanishes later on, when severance pay increases. These effects are large and amount to 22-28% of the control group mean outcomes, depending on tenure. Consistently with the evidence on job-search, Panel **b** of Figure 3 shows that higher mobility is observed for low-tenure graded security workers in low-paying sectors (see Table A11).

A potential concern for the interpretation of these results is that pre-JA workers may

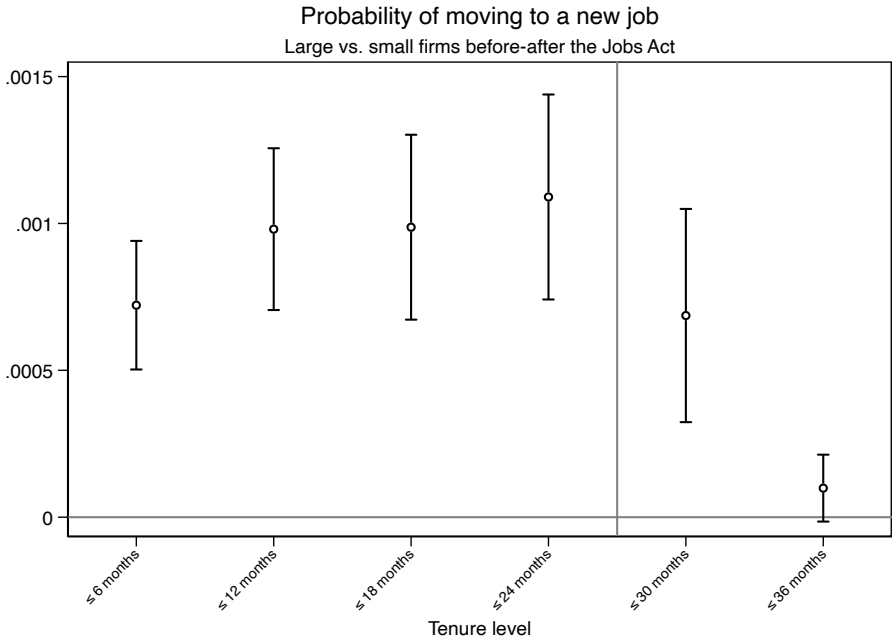
have been discouraged from changing jobs after the reform, since new jobs would have been under graded security. We sidestep this complication in Figure A2, where we show that our results still hold even if, instead of considering as controls workers hired between March and December 2014, we use workers hired between March and December 2012, who were already beyond the 2-year tenure threshold when the Jobs Act was introduced.

In Figure 4 we investigate whether mobility allows workers to achieve better wages or job positions. Panel **a** of Figure 4 shows that low-tenure graded security workers transition towards a higher paying job - defined as whether the new permanent full-time contract's gross daily earnings is higher than that of the previously resigned open-ended contract. Again, this probability is larger for those in the first step of the graded security contract, and irrespective of the tenure amounts to an increase of 22% with respect to the control group mean (see panel B of Table A10). This result is important as it would make little sense for workers in graded security contract to voluntarily move towards jobs that have the same employment protection level - where they would have to start climbing the tenure/severance pay ladder again - but lower pay than the current one. Furthermore, in Panel **b** of Figure 4 and Table A10 (see panel C) explore whether mobility allows the workers to achieve a career progression (i.e., a higher job position) - defined as whether the new permanent full-time contract's allows the worker to move from blue- to white-collar jobs or from blue/white-collar jobs to managerial positions. The evidence in Figure 4 suggests that mobility improves career trajectories during the first 2 years of employment, although the estimates are never statistically significant at the conventional levels. Again, this positive effect fades out when achieving the second step of the graded security mechanism, reflecting also lower mobility for these workers.

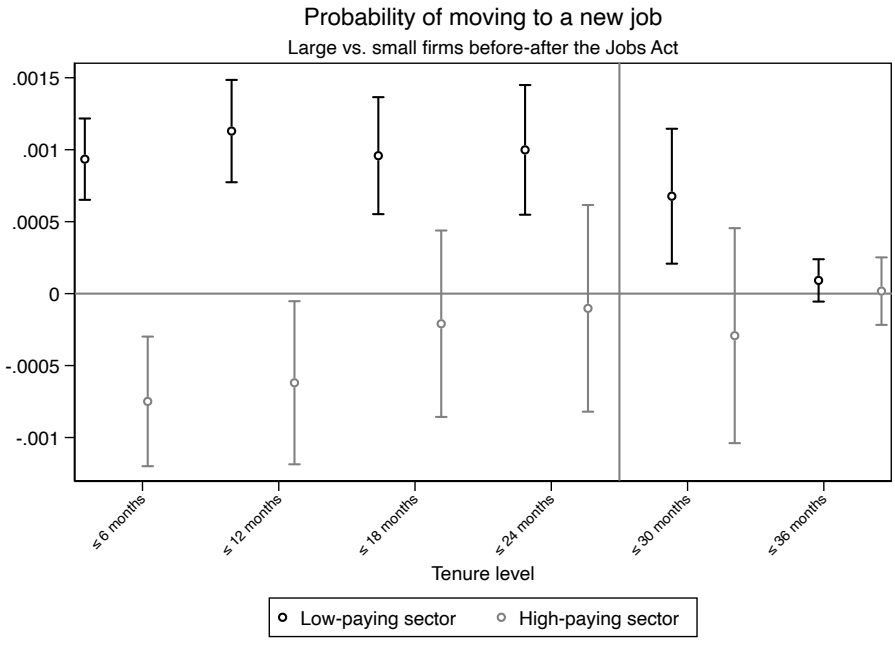
Overall, our results on mobility suggest that the job search effort induced by the initial insecurity related to graded security vs. pre-reform open ended contracts seems to be paying off, as low-tenure graded-security workers, especially in low-paying sectors, are more likely to voluntarily quit the job and move to better-paid jobs.[18]

[18]Even in this case, we find no evidence of heterogeneous effects either by age or by gender.

Figure 3: Job-to-Job Mobility



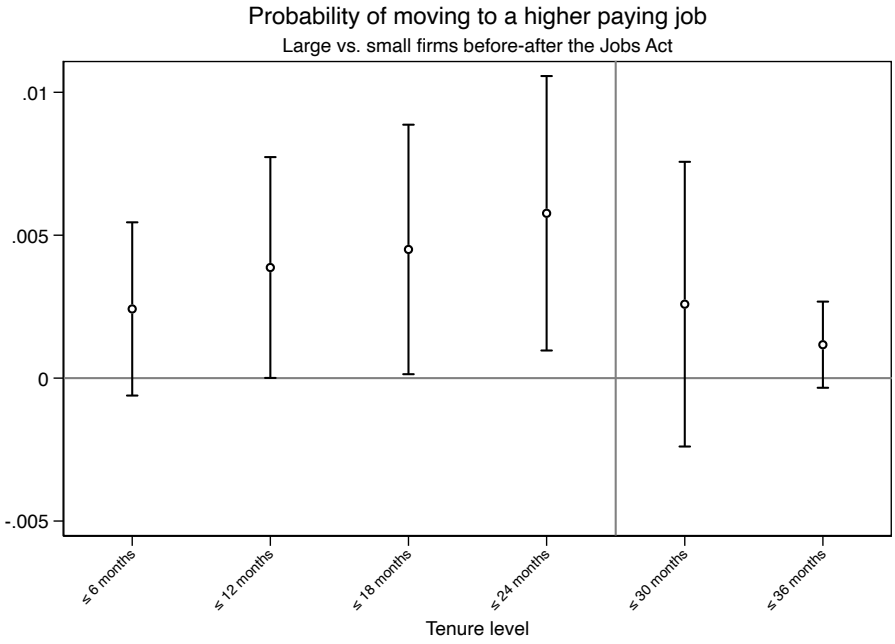
(a) Overall



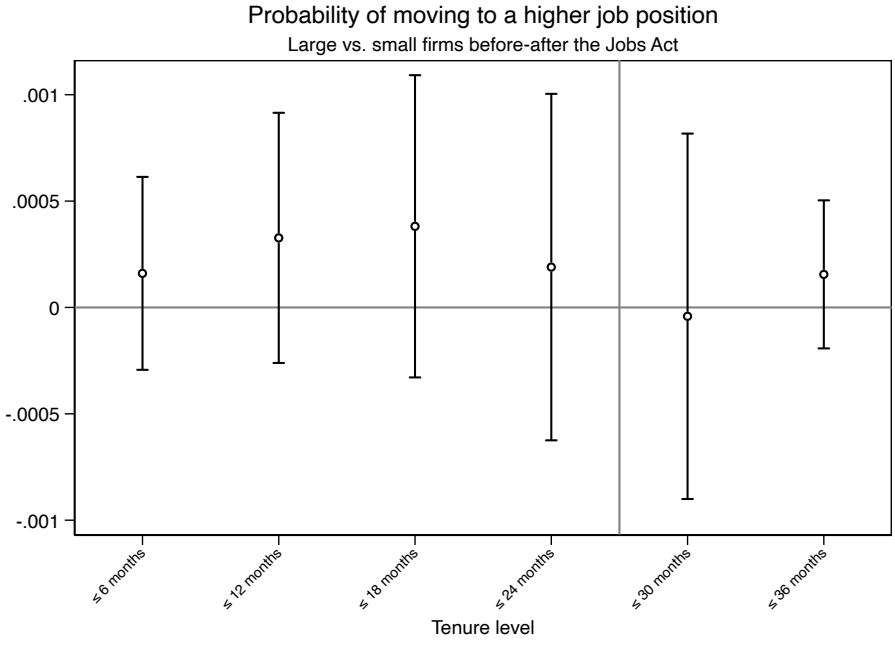
(b) By sector of employment

Notes: Panel **a** of the Figure reports the difference-in-differences estimates available in Panel A of Table A10. The dependent variable is the probability of moving to a new permanent job from a previously resigned one at month tenure m. Panel **b** reports the estimates available in Table A11. High-paying sectors are defined as those with daily gross earnings above the yearly specific median.

Figure 4: Job-to-Job Mobility: Wage and Career Progressions



(a) Wage progression



(b) Career progression

Notes: Panel **a** and **b** of the Figure report the difference-in-differences estimates available in Panel B and C of Table A10, respectively. In Panel **a**, the dependent variable is the probability of moving to a higher paying permanent job from previously resigned one at month tenure m. In Panel **b**, the dependent variable is the probability of moving to a new permanent contract with a higher job position (from blue to white collar and from blue/white collar to manager) with respect to the previous resigned open-ended contract.

5.4.3 Worker effort

Graded security workers may also respond to the higher layoff risk faced at the initial stage of their contracts by exerting more effort at work, with the hope of signalling to employers their attachment to the firm and secure their jobs.

We carry out our analysis on workers' effort using information on annual days worked reported in the LoSai administrative data.[19] Considering that workers who are paid relatively well have a higher incentive to show their attachment to the job by putting more effort, we here distinguish between workers employed in sectors whose daily gross earnings is above or below the yearly specific median. Figure 5 and Table A12 report the difference-in-differences estimates at different tenure levels. We find that workers hired with a graded security contract in high-paying sectors exert more effort during the first 18-24 months of employment as they work more days than their counterparts hired with the pre-reform permanent contract. In terms of magnitude, the effects range between 2.8% and 4.5% of the control group mean, depending on tenure. By contrast, we estimate that graded security workers in low-paying sectors work less days than comparable workers on pre-reform permanent contracts, a result that - in most cases - holds irrespective of tenure.

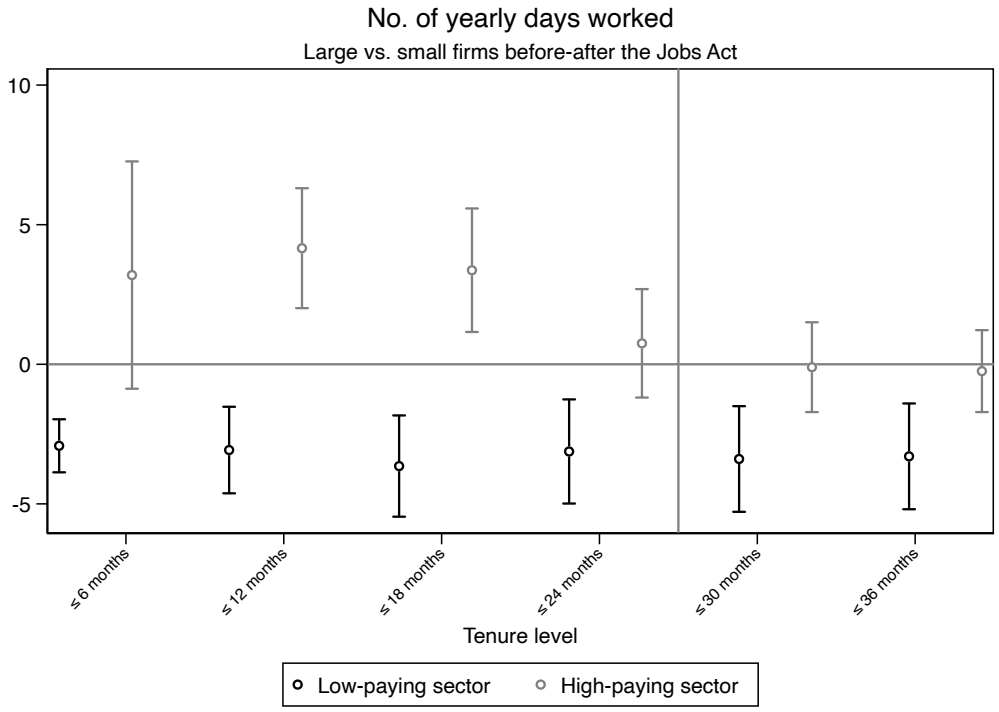
Overall, this evidence suggests that while workers in higher-paying sectors work harder to secure the second step of the graded security contract, i.e. higher employment protection, those in low-wage sectors are less willing to be committed and, possibly, less attached to their current employers.[20]

Finally, in Table A13, we investigate average responses on other measures of workers' effort that are present in the LFS - including hours worked, sick leave, overtime work, working during weekends - as well as on monthly earnings. We find that low-tenure graded security employees work more hours per week, with the size of the effect amounting to 0.5% of the control group mean (less than 41 hours). Unlike Ichino and Riphahn (2005), we find no

[19] We have also considered annual gross earnings. However, we find no effects.

[20] Even in this case, we fail to detect significant heterogeneous effects by age and gender.

Figure 5: Worker Effort by Sector of Employment



Notes: The Figure reports the difference-in-differences estimates available in Table A12. The dependent variable is annual days worked at month tenure m. High-paying sectors are defined as those with daily gross earnings above the yearly specific median.

effect on sick leave absences, but we do detect a positive effect on overtime work and on working during weekend, although the latter is small and not statistically significant. As a consequence of the higher number of hours worked and of the higher likelihood of working overtime and during weekends, we also find that low-tenure/low-severance pay graded security workers have marginally higher monthly earnings than comparable workers hired on pre-reform open-ended contracts. Although very significant from a statistical viewpoint, this effect is small in magnitude and equal to €22/month or 2% of the average monthly earnings observed among control group workers.[21]

[21] We assessed the presence of heterogeneous effects by sector wage, age or by gender, but found no evidence in favour of heterogeneous effects. Results are thus not reported.

6 Conclusions

In this paper, we investigate how a reduction in employment protection affects workers' perceived and actual job security, as well as their job search, mobility, and effort. We exploit the 2015 Italian Jobs Act, which lowered protection and introduced a sharp discontinuity in severance payments at the two-year tenure mark for new permanent hires in large firms, while leaving comparable workers in small firms unaffected. Using a difference-in-differences strategy and combining LFS data with administrative records, we estimate the causal effects of interest.

We can summarise our findings as follows. First, the new graded-security contract significantly increased job insecurity among workers with 0–2 years of tenure, for whom severance pay was flat and low; this effect fades once workers complete two years of tenure, when severance pay raises by 50%.

Second, higher insecurity induced low-tenure graded-security employees to search more intensively and to transition more often to new jobs than comparable pre-reform workers. These transitions were particularly pronounced in low-paying sectors, where workers moved to better-paying jobs, with likely beneficial effects on labour market efficiency. Again, the effects disappear once severance pay rises with tenure.

Third, in high-paying sectors, low-tenure treated employees responded to the heightened layoff risk by exerting more effort than their counterparts in the control group. In particular, the increase in days worked is concentrated among workers with above-median sectoral wages, suggesting stronger incentives to signal attachment to the firm in order to secure their jobs

Overall, our evidence highlights a trade-off between job security and job mobility induced by the Jobs Act, with important policy implications for the design of future employment protection reforms in Europe. In particular, our results underscore the need to strike a careful balance between reducing insecurity - through more effective or generous unemployment insurance and social safety nets, especially in the early stages of employment - and fostering labour market efficiency and mobility.

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Appendix 1: The LoSai Samples

1. **Separations**. We include only dismissals and collective layoffs. Out of 106,532 total spells recorded for 76,145 workers, we observe 19,055 separations. We then create a monthly panel that follows workers from hiring and until the minimum between the eventual termination date and the end of our observation period (36 months). We then estimate survival in the hiring firm at 6, 12, 18, 24, 30, and 36 months. Descriptive statistics are in Table A3, Panel A.

2. **Job-to-job mobility**. We focus on voluntary resignations of permanent employees leading to a new permanent job, excluding other separations. This sample covers 79,579 spells for 63,619 workers, with 25,084 transitions. We again build a monthly panel to estimate job-to-job mobility at 6, 12, 18, 24, 30, and 36 months. Descriptive statistics are in Table A3, Panel B.

3. **Effort (days worked)**. We include workers hired full-time on permanent contracts between March and December 2014 and between March and December 2015, who worked at least one day and earned at least €1. The data report the number of days worked on a yearly basis, which prevents us from constructing a monthly panel for this analysis. We thus compute tenure as the number of months between hiring and year-end (or actual termination if earlier) and analyse days worked in the year by tenure levels - effectively accounting for the fact that the potential number of working days depends on contract duration. Overall, we observe 105,927 spells for 102,054 employees. Descriptive statistics for this sample are reported in the third panel of Table A3.

Appendix 2: Additional tables and figures

Figure A1: Fraction of Large-Firm Employees Over 2012-2014 by Sector and Region

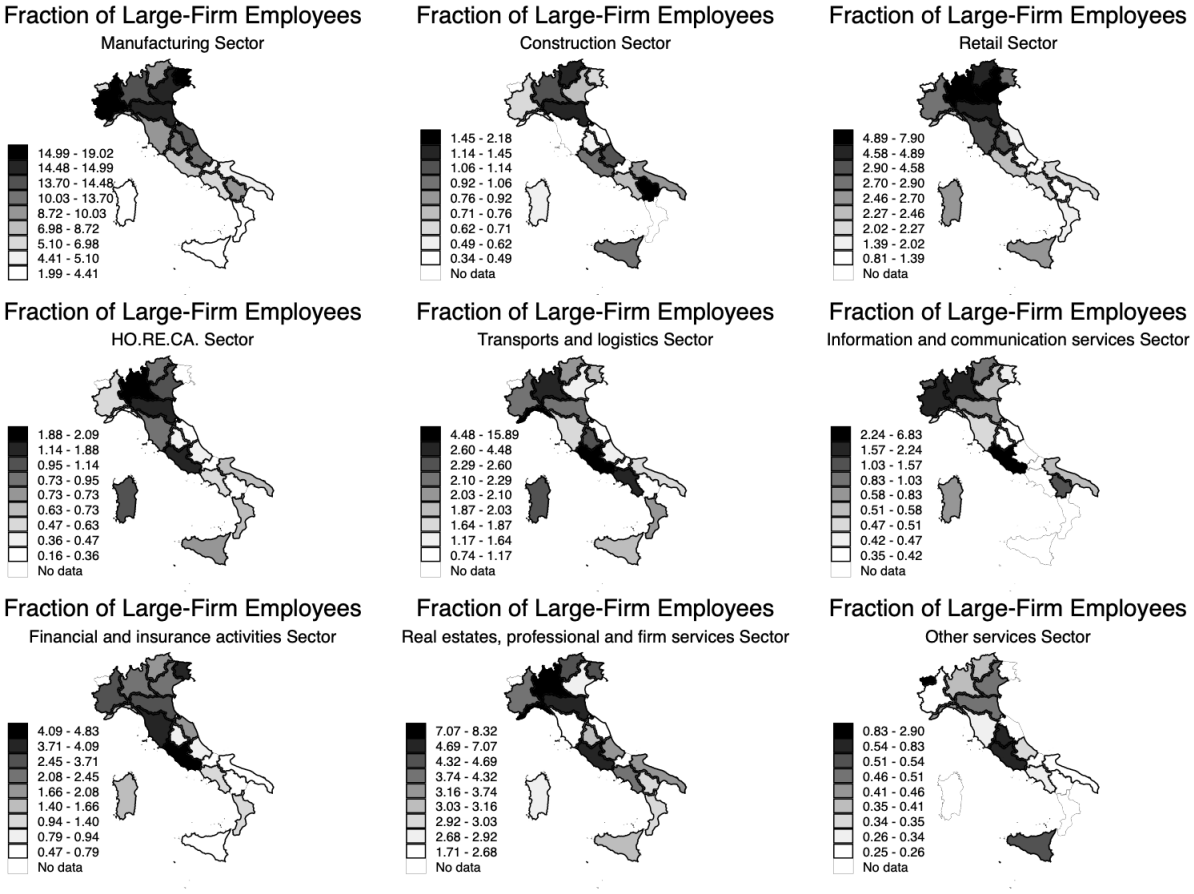
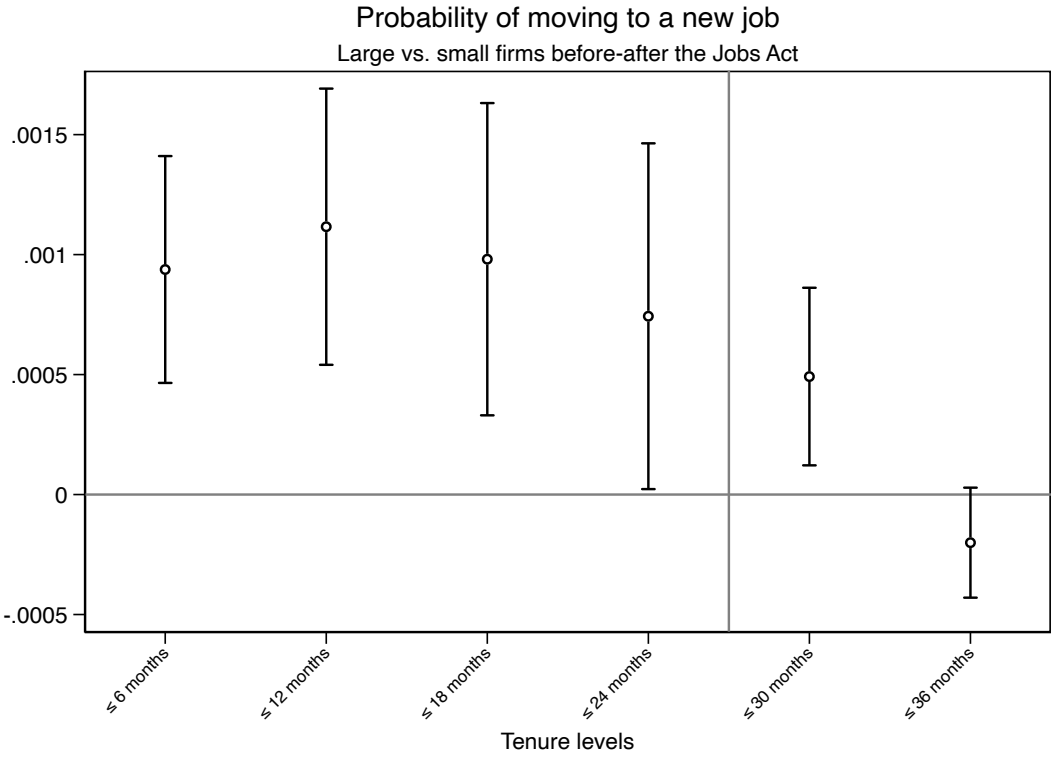


Figure A2: Job-to-Job Mobility - Permanent contracts between March and December 2012 as alternative control group



Notes: The dependent variable is the probability of moving to a new permanent job from a previously resigned one at month tenure m. The sample comprises workers hired full-time and with a permanent contract between March and December of 2015 (treated) and between March and December 2012 (alternative controls).

Table A1: Worker Tenure by Hiring Year and Interview Year in the Final Sample.

	Interview year:											
	2013		2014		2015		2016		2017		2018	
	Tenure (years)		Tenure (years)		Tenure (years)		Tenure (years)		Tenure (years)		Tenure (years)	
	min	max	min	max	min	max	min	max	min	max	min	max
Hiring year:												
2010	2.2	3										
2011	1.2	2.8	2.2	3								
2012	0.2	1.8	1.2	2.8	2.2	3						
2013	0	0.8	0.2	1.8	1.2	2.8	2.2	3				
2014			0	0.8	0.2	1.8	1.2	2.8	2.2	3		
2015					0	0.8	0.2	1.8	1.2	2.8	2.2	3
2016							0	0.8	0.2	1.8	1.2	2.6
2017									0	0.8	0.2	1.6
2018											0	0.6

Table A2: Descriptive Statistics: Labor Force Survey

	Obs.	Mean	sd	min	Max
Fear of job loss (within the next 6 months)	43356	0.087	0.281	0	1
On-the-job search (any activity)	43356	0.042	0.200	0	1
Perceived job stability ≥ 7	42823	0.738	0.440	0	1
Hours worked	42868	40.805	5.672	18	105
Sick leave	43356	0.011	0.104	0	1
Overtime	43183	0.049	0.215	0	1
Working during weekend	43296	0.421	0.494	0	1
Monthly net earnings (€)	43356	1244.321	387.584	250	3000
Large Firm	43356	0.434	0.496	0	1
Jobs Act	43356	0.317	0.465	0	1
Large×Jobs Act	43356	0.138	0.345	0	1
% Large-Firm Employees	199	5.565	8.640	1.651	48.35
Woman	43356	0.334	0.472	0	1
Age	43356	38.686	11.156	16	67
Education (years)	43356	11.193	3.443	3	18
Tenure	43356	1.763	1.013	0	3
% Tenure [0-2] years	43356	0.590	0.492	0	1
% Tenure (2-3] years	43356	0.410	0.492	0	1
Immigrant	43356	0.238	0.426	0	1
Married	43356	0.503	0.500	0	1
White collar	43356	0.320	0.467	0	1
Lives in the South	43356	0.215	0.411	0	1

Notes: Italian Labor Force Survey (2013-2019), ISTAT. Monthly earnings are bottom- and top-coded. The fraction of large-firm employees varies by region and 1-digit ATECO sector of employment.

Table A3: Descriptive Statistics: INPS Administrative Records

	Obs.	Mean	sd	min	Max
Sample: Continuation rates					
Age	106532	39.982	10.766	15	67
Women	106532	0.234	0.423	0	1
Lives in South	106532	0.365	0.481	0	1
Retained the job at 6 months tenure	2292578	0.228	0.420	0	1
Retained the job at 12 months tenure	2292578	0.402	0.490	0	1
Retained the job at 18 months tenure	2292578	0.564	0.496	0	1
Retained the job at 24 months tenure	2292578	0.718	0.450	0	1
Retained the job at 30 months tenure	2292578	0.865	0.342	0	1
Retained the job at 36 months tenure	2292578	0.992	0.091	0	1
Tenure	2292578	16.478	10.598	0	36
Large Firm	2292578	0.550	0.497	0	1
Jobs Act	2292578	0.645	0.479	0	1
Large×Jobs Act	2292578	0.346	0.476	0	1
White collar	2292578	0.324	0.468	0	1
Sample: Job-to-job mobility					
Age	79579	39.118	10.421	15	67
Women	79579	0.241	0.428	0	1
Lives in South	79579	0.268	0.443	0	1
Job-to-job mobility at 6 months tenure	2056475	0.001	0.037	0	1
Job-to-job mobility at 12 months tenure	2056475	0.002	0.047	0	1
Job-to-job mobility at 18 months tenure	2056475	0.003	0.054	0	1
Job-to-job mobility at 24 months tenure	2056475	0.004	0.059	0	1
Job-to-job mobility at 30 months tenure	2056475	0.004	0.062	0	1
Job-to-job mobility at 36 months tenure	2056475	0.004	0.065	0	1
Tenure	2056475	16.943	10.545	0	36
Large Firm	2056475	0.598	0.490	0	1
Jobs Act	2056475	0.645	0.479	0	1
Large×Jobs Act	2056475	0.372	0.483	0	1
White collar	2056475	0.342	0.474	0	1
Higher paying job	63379	0.684	0.465	0	1
Higher job position	63379	0.045	0.207	0	1
Sample: Worker effort					
Age	105927	39.267	10.936	15	67
Women	105927	0.243	0.429	0	1
Lives in South	105927	0.314	0.464	0	1
No. yearly days worked	311994	196.394	114.951	1	312
Tenure	311994	19.679	10.481	0	36
Large Firm	311994	0.572	0.495	0	1
Jobs Act	311994	0.624	0.484	0	1
Large×Jobs Act	311994	0.347	0.476	0	1
White collar	311994	0.327	0.469	0	1
Earnings	311994	18181.963	15460.120	100	214300

Notes: LoSai archives (2014-2018), INPS. Age, Woman and Lives in South refer to individual characteristics observed at the time of hiring.

Table A4: The Effect of the Jobs Act on Fear of Job Loss. Addressing composition effects.

		Tenure levels: [0-2] (1)	(2-3] (2)
A. No individual-level controls	Large×Jobs Act	0.022***	0.002
		(0.007)	(0.005)
	Observations	25596	17760
B. Imputation estimator (Borusyak et al., 2021):	Large×Jobs Act	0.025***	0.003
		(0.008)	(0.005)
	Observations	25596	17760
C. Balanced observables over time and tenure groups within firm size	Large×Jobs Act	0.020**	0.006
		(0.008)	(0.006)
	Observations	25596	17760

Notes: We adopt the specification used in Table 2, but excluding controls in Panel A, using the imputation estimator by Borusyak et al. (2021) in Panel B, and balancing the samples involved in the difference-in-differences comparison on the basis of observable characteristics using entropy balancing in Panel C. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: The Effect of the Jobs Act on Fear of job loss. Selection test

	(1)
Large×Jobs Act	-0.037
	(0.061)
Observations	20180
Mean Dep. Var. Large=0, JA=0	0.215

Notes: We adopt the specification used in Table 2, but the sample includes workers with 0-2 years of tenure reporting fear of job loss (=1) and workers with (2-3] years of tenure either reporting fear of job loss or not (0/1). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: The Effect of the Jobs Act on Fear of Job Loss for different firm size selections.

		Tenure levels: [0-2] (1)	(2-3] (2)
A. Only workers in firms with less than 50 employees	Large×Jobs Act	0.023***	0.000
		(0.009)	(0.007)
	Observations	20438	13358
B. Excluding workers in firms with 10-19 employees	Large×Jobs Act	0.032***	-0.002
		(0.008)	(0.005)
	Observations	21372	14900
c. Excluding workers in firms with 10-49 employees	Large×Jobs Act	0.031***	0.005
		(0.009)	(0.008)
	Observations	17523	11862

Notes: We adopt the specification used in Table 2, but trim the sample differently in each panel. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: The Effect of the Jobs Act on Fear of Job Loss. Further robustness Tests.

		Tenure level: [0-2] (1)	(2-3] (2)
A. Region specific trends	Large×Jobs Act	0.018**	0.000
		(0.007)	(0.005)
	Observations	25596	17760
B. Same cohorts in the low-/high-tenure group	Large×Jobs Act	0.025***	-0.003
		(0.008)	(0.005)
	Observations	22526	14334
C. Including months of tenure dummies	Large×Jobs Act	0,024***	0,004
		(0,007)	(0,005)
	Observations	25596	17760
D. No hires in a 45-day donut around 7 March 2015	Large×Jobs Act	0,027***	0,010**
		(0,008)	(0,004)
	Observations	24337	16709
E. Perceived Job Stability	Large×Jobs Act	-0.029***	0.005
		(0.007)	(0.010)
	Observations	25258	17565

Notes: Unless otherwise stated, we adopt the specification used in Table 2. In Panel C the dependent variable is a dummy taking value 1 for workers reporting above-median satisfaction with job stability, and 0 otherwise. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: The Effect of the Jobs Act on Fear of job loss. Heterogeneous Effects.

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
Large×Jobs Act	0.036***	0.013*
	(0.009)	(0.007)
Large×Jobs Act*Woman	-0.031*	-0.026
	(0.016)	(0.016)
Large×Jobs Act	0.026**	-0.007
	(0.010)	(0.007)
Large×Jobs Act*Under35	-0.003	0.024**
	(0.013)	(0.011)
Large×Jobs Act	0.038***	-0.177
	(0.013)	(0.127)
Large×Jobs Act*Permanent 1y before	-0.020	0.182
	(0.016)	(0.129)
Large×Jobs Act	0.019**	0.004
	(0.008)	(0.006)
Large×Jobs Act*South	-0.003	-0.015
	(0.020)	(0.029)
Large×Jobs Act	0.027***	-0.002
	(0.008)	(0.008)
Large×Jobs Act*White Collar	-0.019	0.020
	(0.011)	(0.018)
Large×Jobs Act	0.026***	-0.014
	(0.009)	(0.014)
Large×Jobs Act*(Wage>median)	-0.012	0.028
	(0.013)	(0.020)
Observations	28918	19954

Notes: we adopt the specification used in Table 2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: The Effect of the Jobs Act on Continuation Rates

	Tenure level:					
	≤ 6	≤ 12	≤ 18	≤ 24	≤ 30	≤ 36
	(1)	(2)	(3)	(4)	(5)	(6)
Large×Jobs Act	-0.023***	-0.015***	-0.007***	-0.002***	-0.002**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	2292578	2292578	2292578	2292578	2292578	2292578
Mean Dep. Var. Large Firm=0, JA=0	0.250	0.422	0.577	0.721	0.856	0.994

Notes: Each column reports estimates from OLS regression. The dependent variable is the probability of retaining the job at month tenure m. All specifications include Large Firm and Jobs Act dummies, year-month fixed effects and worker fixed effects. Standard errors clustered at the worker level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: The Effect of the Jobs Act on Job-to-Job Mobility

	Tenure level:					
	≤ 6	≤ 12	≤ 18	≤ 24	≤ 30	≤ 36
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Any Mobility						
Large×Jobs Act	0.0007***	0.0010***	0.0010***	0.0011***	0.0007***	0.0001*
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Observations	2086139	2086139	2086139	2086139	2086139	2086139
Mean Dep. Var. Large Firm=0, JA=0	0.0025	0.0038	0.0045	0.0051	0.0055	0.0056
Panel B: Mobility to a Higher Paying Job						
Large×Jobs Act	0.0024	0.0039**	0.0045**	0.0058**	0.0026	0.0012
	(0.0015)	(0.0020)	(0.0022)	(0.0024)	(0.0025)	(0.0008)
Observations	63379	63379	63379	63379	63379	63379
Mean Dep. Var. Large Firm=0, JA=0	0.0136	0.0201	0.0233	0.0261	0.0274	0.0289
Panel C: Mobility to a Higher Job Position						
Large×Jobs Act	0.0002	0.0003	0.0004	0.0002	-0.0000	0.0002
	(0.0002)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0002)
Observations	63379	63379	173712	63379	63379	63379
Mean Dep. Var. Large Firm=0, JA=0	0.0007	0.0010	0.0012	0.0015	0.0017	0.0021

Notes: Each column reports estimates from OLS regression. In Panel A, the dependent variable is the probability of moving to a new job at month tenure m. In Panel B, the dependent variable is the probability of moving to a higher paying job at month tenure m. In Panel C, the dependent variable is the probability of moving to a higher job position (from blue to white collar or from blue/white collar to manager) at month tenure m. Mobility to a new permanent contract with respect to a previously resigned open-ended contract. All specifications include Large Firm and Jobs Act dummies, year-month fixed effects and worker fixed effects. Standard errors clustered at the worker level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: The Effect of the Jobs Act on Job-to-Job Mobility by Low- vs. High-Paying Sector

	Tenure level:					
	≤ 6	≤ 12	≤ 18	≤ 24	≤ 30	≤ 36
	(1)	(2)	(3)	(4)	(5)	(6)
Large×Jobs Act	0.0009***	0.0011***	0.0010***	0.0010***	0.0007***	0.0001
	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Large×Jobs Acts*High-Paying Sector	-0.0007***	-0.0006**	-0.0002	-0.0001	-0.0003	0.0000
	(0.0002)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0001)
Observations	2056475	2056475	2056475	2056475	2056475	2056475
Mean Dep. Var. Large Firm=0, JA=0	0.0025	0.0038	0.0045	0.0051	0.0055	0.0056

Notes: Each column reports estimates from OLS regression. The dependent variable is the probability of moving to a new job at month tenure m. Mobility to a new permanent contract with respect to a previously resigned open-ended contract. All specifications include Large Firm and Jobs Act dummies, year-month fixed effects and worker fixed effects. High-paying sectors are defined as those with daily gross earnings is above the yearly specific median. Standard errors clustered at the worker level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: The Effect of the Jobs Act on Number of Yearly Days Worked

	Tenure level:					
	≤ 6	≤ 12	≤ 18	≤ 24	≤ 30	≤ 36
	(1)	(2)	(3)	(4)	(5)	(6)
Large×Jobs Act	-2.9200***	-3.0715***	-3.6460***	-3.1234***	-3.3924***	-3.2977***
	(0.4850)	(0.7909)	(0.9256)	(0.9515)	(0.9656)	(0.9668)
Large×Jobs Act*High-Paying Sector	3.1950	4.1579***	3.3691***	0.7504	-0.1034	-0.2454
	(2.0766)	(1.0959)	(1.1286)	(0.9906)	(0.8206)	(0.7484)
Observations	70066	117129	167396	202278	235159	258685
Mean Dep. Var. Large Firm=0, JA=0	62.603	92.701	121.605	134.142	144.498	150.710

Notes: Each column reports estimates from OLS regression. The dependent variable is the yearly number of days worked at tenure months m. All specifications include Large Firm and Jobs Act dummies, year-month fixed effects and worker fixed effects. High-paying sectors are defined as those with daily gross earnings is above the yearly specific median. Standard errors clustered at the worker level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: The Effect of the Jobs Act on Other Outcomes.

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
Hours Worked:		
Large×Jobs Act	0.215**	-0.067
	(0.099)	(0.182)
Observations	25277	17591
Mean Dep. Var. Large Firm=0, JA=0	40.996	41.046
Sick Leave:		
Large×Jobs Act	-0.001	0.005**
	(0.003)	(0.002)
Observations	25596	17760
Mean Dep. Var. Large Firm=0, JA=0	0.010	0.009
Overtime Work:		
Large×Jobs Act	0.023***	-0.002
	(0.004)	(0.006)
Observations	25491	17692
Mean Dep. Var. Large Firm=0, JA=0	0.028	0.031
Working During Weekends:		
Large×Jobs Act	0.010	0.025
	(0.008)	(0.016)
Observations	25565	17732
Mean Dep. Var. Large Firm=0, JA=0	0.469	0.493
Monthly Earnings:		
Large×Jobs Act	22.081***	-4.260
	(6.843)	(7.066)
Observations	25596	17760
Mean Dep. Var. Large Firm=0, JA=0	1096	1126

Notes: Each column reports estimates from OLS regression. All specifications include Large Firm and Jobs Act dummies and the following controls: interview quarter-by-year dummies, gender, age and age squared, education, marital status, immigrant status, region dummies, occupation - white or blue collar - and 1-digit ATECO sector dummies. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$