

CSEF

Centre for Studies in Economics and Finance

WORKING PAPER NO. 684

Employment Protection, Job Insecurity, and Job Mobility

Marco Bertoni, Simone Chinetti, and Roberto Nisticò

October 2023



University of Naples Federico II



University of Salerno



Bocconi University, Milan

CSEF - Centre for Studies in Economics and Finance
DEPARTMENT OF ECONOMICS AND STATISTICS – UNIVERSITY OF NAPLES FEDERICO II
80126 NAPLES - ITALY

Tel. and fax +39 081 675372 – e-mail: csef@unina.it
ISSN: 2240-9696

WORKING PAPER NO. 684

Employment Protection, Job Insecurity, and Job Mobility

Marco Bertoni^{*}, Simone Chinetti[†], and Roberto Nisticò[‡]

Abstract

This study leverages the Italian Jobs Act reform as a natural experiment to examine the impact of reduced employment protection on job insecurity and job mobility. The reform significantly lowered protection for open-ended contract workers in large firms hired after March 7, 2015, and introduced a sharp discontinuity in severance pay at 2-year tenure. Treated employees exhibit increased fear of job loss and higher termination rates. The higher job insecurity prompts workers in low-pay sectors and in low-quality firms to actively pursue job mobility, transitioning towards higher-paying positions. Conversely, workers in high-paying sectors respond by intensifying their efforts to secure their existing jobs. Crucially, all effects disappear for workers above the 2-year tenure threshold, when they become entitled to a 50% higher severance pay. These findings emphasize a complex trade-off behind the design of employment protection systems, as addressing early-stage insecurity with tailored social insurance may counteract upward mobility effects.

JEL Classification: J22; J28; J41; J65.

Keywords: employment protection; job insecurity; job mobility; on-the-job search.

Acknowledgments: We thank Sonia Bhalotra, Tito Boeri, Giorgio Brunello, Pierre Cahuc, Lorenzo Cappellari, Andrew Clark, Conchita D'Ambrosio, Francis Kramarz, Cristina Lafuente, Anthony Lepinteur, Monica Langella, Giovanni Mastrobuoni, Lorenzo Pandolfi, Giovanni Pica, Francesco Principe, Enrico Rubolino, Raffaele Saggio, Olmo Silva, Kostantinos Tatsiramos, and participants at the ifo institute seminar and at the University of Naples Federico II Workshop on "Human Capital, Labour Markets and Public Policies", the Bordeaux School of Economics "1st Welfare & Policy Conference", the 2023 EALE conference, the 38th AIEL conference, and the Badolato Labour and Organization Workshop (BLOW) for valuable comments. Roberto Nisticò acknowledges funding from a FRA grant of the University of Naples Federico II. The usual disclaimers apply.

^{*} University of Padova, IZA and NETSPAR. Email: marco.bertoni@unipd.it

[†] University of Naples Federico II. Email: simone.chinetti@unina.it

[‡] University of Naples Federico II, CSEF and IZA. E-mail: roberto.nistico@unina.it (*Corresponding author*)

1 Introduction

The efficiency of the labor market plays a crucial role in determining how well an economy can respond to aggregate shocks. Labor market institutions, such as employment protection legislation, can introduce frictions that restrain the smooth functioning of the labor market by limiting its ability to swiftly allocate workers to firms where they can be most productive. Theoretical considerations suggest that higher firing costs may hinder both layoffs and new hirings (Bentolila and Bertola, 1990; Postel-Vinay and Turon, 2013; Pries and Rogerson, 2005), and there is abundant empirical evidence indicating that employment protection can have negative impacts on worker flows (Autor et al., 2004; Behaghel et al., 2008; Blanchard and Portugal, 2001; Boeri and Garibaldi, 2007; Cahuc et al., 2019; Daruich et al., 2023; Kugler and Saint-Paul, 2004; Martins, 2009).¹

The low efficiency of European labor markets has often been attributed to their high levels of employment protection. This observation has prompted efforts to deregulate labor markets and enhance efficiency. However, this shift has contributed to the proliferation of precarious forms of employment and to the emergence of dual labor markets, where workers on permanent contracts enjoy high levels of employment protection, while those with temporary contracts bear a disproportionate share of frictional costs (Berton and Garibaldi, 2012; Boeri and Garibaldi, 2007; Cahuc et al., 2016). Against this backdrop, a unified labor contract with graded security – i.e., with severance payments increasing with tenure – has been advocated as a valuable strategy to reduce duality (see e.g. Cahuc and Kramarz, 2005, Blanchard and Tirole, 2008, Cahuc, 2012 for France; Boeri and Garibaldi, 2008, Boeri et al., 2017 for Italy; and Bentolila et al., 2008, Bentolila et al., 2012, Pérez and Osuna, 2014 for Spain).

Employment protection may also influence workers' decisions regarding on-the-job search

¹See also Boeri and Garibaldi (2019), Boeri and Jimeno (2005), Hijzen et al. (2017), Kugler and Pica (2008), Schivardi and Torrini (2008) and Sestito and Viviano (2018) for additional evidence on Italy, as well as Fraise et al. (2015), Pérez and Osuna (2014) and Von Below and Thoursie (2010) for evidence on France, Spain and Sweden, respectively. In addition, see Cahuc and Postel-Vinay (2002), Gómez-Salvador et al. (2004) and Messina and Vallanti (2007) for evidence from cross-country analyses.

and job-to-job mobility. Entitlement to severance pay may lower workers' incentive to change jobs, as they might be reluctant to lose accrued benefits (Garcia-Louzao, 2022; Kettemann et al., 2017). Conversely, reforms that reduce employment protection can heighten workers' perceptions of job insecurity, potentially affecting their effort provision and willingness to seek better job opportunities (Gielen and Tatsiramos, 2012). Surprisingly, however, there is little empirical evidence on the effects of employment protection on workers' job search efforts and on their mobility.

This paper exploits the 2015 Italian "Jobs Act" as a natural experiment to estimate the effect of reduced employment protection on job insecurity, job search and job-to-job mobility. This reform changed two important features of employment protection. First, it abolished the compulsory reinstatement of workers in case of unfair dismissals, that was in place in Italy since the 1970s. Second, it mandates that unfair layoffs must be compensated by a severance payment that is flat and equal to 4 months of pay for the first 2 years of tenure, and that increases by two months of pay for every additional year of tenure, up to 24 months' pay at a 12-year tenure level. This "graded security" progression generates a sharp discontinuity in severance pay at 2 years of tenure, when treated workers become entitled to a 50% higher severance pay.

Importantly, both prescriptions only apply to workers hired on open-ended contracts after March 7, 2015 in firms with more than 15 employees - "large firms" hereafter. Instead, they do not apply to large firm employees hired before March 7, 2015, who continue to be protected by the reinstatement clause, and to open-ended hires in small firms irrespective of the hiring data. This feature of the reform, combined with the sharp discontinuity in severance pay at 2-year tenure, allows us to use a difference-in-differences approach to estimate not only how employment protection affects workers' perceived and actual job insecurity as well as their job search and mobility behaviors, but also to explore how these effects varies by tenure.

We base our analysis on a combination of survey and administrative data. We gather information on perceived job insecurity and other self-reported outcomes from the Italian

Labour Force Survey (LFS), which is administered quarterly by the Italian National Statistical Office (ISTAT) and encompasses approximately 50,000 individuals. We supplement the survey data with information obtained from administrative matched employer-employee data sourced from the Italian Social Security Institute (INPS).

The main findings of our study can be summarized as follows. First, we observe that, for workers with up to 2 years of tenure, the share of those fearing job loss is 2-percentage point higher among workers hired with the graded security contract compared to those hired with a pre-reform open-ended contract. This corresponds to an increase in perceived job insecurity of 17% with respect to the control group mean value. However, we find no significant effect for workers with 3 years of tenure, likely due to their increased severance pay. The estimated effects on perceived job insecurity align well with the patterns of job separations observed in the administrative data. Our findings reveal that graded security workers are initially less likely to remain employed in the same firm compared to workers with pre-reform open-ended contracts. However, as time progresses, this difference gradually diminishes. Specifically, the disparity in employment retention between the two groups reduces to zero after 2 years, when the graded security workers become eligible for increased severance pay.

Second, we examine workers' job search behavior using the LFS data, and show that – during the first 2 years of employment – graded security workers mitigate the higher risk of layoffs by actively conducting more on-the-job search compared to their counterparts on pre-reform permanent contracts. In addition, when analyzing job mobility outcomes using the administrative data, we consistently find an increase in job-to-job mobility. Interestingly, we show that it is mostly workers with up to 2 years of tenure in lower-paying sectors or lower-quality firms who exhibit a greater likelihood of voluntarily quitting their jobs to transition towards higher-paying positions. Even in these cases, the effects on job search and job-to-job mobility turn to zero for workers who reached the third year of tenure.

Third, we also investigate responses along the intensive margin of labor supply, and find that workers with 0-2 years of tenure employed in high-paying sectors with a graded security

contract increase their number of yearly workdays, as recorded in the administrative data. This evidence is in keeping with the hypothesis that workers employed in high paying firms might have hard times finding a higher-paying job, and thus react to insecurity by exhibiting a proactive response to secure their current one. Again, these differences in behavior vanish in the third year of tenure, when severance pay increases by 50%.

Taken together, our results provide nuanced perspectives on how graded security contracts affect workers at different levels of tenure, leading to higher levels of job insecurity for those at the initial stages of their careers.

Higher insecurity can ultimately impact workers' behavior and well-being (Böckerman et al., 2011; Clark and Postel-Vinay, 2009; Georgieff and Lepinteur, 2018; Origo and Pagani, 2009). Previous studies have in fact shown that job uncertainty may affect workers' labor market behavior (Akerlof et al., 1988; Clark, 2001; Freeman, 1978). Importantly, the adverse effects of job instability may extend into various domains of workers' life: it is associated with reduced well-being and life satisfaction (Carr and Chung, 2014; De Cuyper and De Witte, 2007; Drobnič et al., 2010), with lower physical and mental health (Buffel et al., 2015; Lepinteur, 2021; László et al., 2010), and with a large range of personal and family problems (Larson et al., 1994; Lim and Loo, 2003; Mauno and Kinnunen, 2002; Rook et al., 1999). Workers with higher job insecurity are also more likely to delay long-term commitments such as getting married, having children, or buying a house (Clark et al., 2023; Clark and Lepinteur, 2022; De Paola et al., 2021; Mistrulli et al., 2023). Finally, there is evidence pointing to the negative consequences for the economy as well as for social policy, as job instability is associated with lower children's educational outcomes (Ruiz-Valenzuela, 2020), with cuts in consumer and household spending (Barceló and Villanueva, 2016; Benito, 2006; Clark et al., 2022; Guariglia, 2001; Lugalde et al., 2018), and with lower public support for redistribution (Marx, 2014; Paskov and Koster, 2014).

Moreover, our analysis reveals a great deal of heterogeneity in workers' behavioral responses to the higher layoff risk caused by reduced employment protection: while workers in

high-paying sectors tend to exert more effort on the job to secure their positions, those in low-paying sectors or in low-quality firms instead increase job search and are more likely to transition to better-paid jobs.

Our paper contributes to two strands of literature. First, we advance the literature on the effects of wrongful discharge laws on workers' labour supply. While most existing studies examined the effects on employment and wages (Autor et al., 2004; Butschek and Sauermann, 2022; Leonardi and Pica, 2013; MacLeod and Nakavachara, 2007; Martins, 2009) and on productivity (Autor et al., 2007; Bassanini et al., 2009; Bjuggren, 2018; Cappellari et al., 2012; Cingano et al., 2010, 2016), we look at workers' mobility and effort provision choices in response to higher insecurity.

Second, our analysis also contributes to the literature on the effects of severance pay (Boeri et al., 2017; Cahuc et al., 2019; Garibaldi and Violante, 2005; Jappelli and Padula, 2016; Kugler, 2005; Lazear, 1990; Marinescu, 2009) by investigating how its generosity affects job insecurity and labour market efficiency. To our knowledge, the evidence on this topic is very limited. Gielen and Tatsiramos (2012) provide cross-country evidence of a negative relationship between job protection and workers' quit behaviour using data from the European Community Household Panel, while Kettemann et al. (2017) show that an Austrian reform replacing tenure-based severance payments at layoff with payments into workers' pension accounts increased workers' mobility in the eve of a mass layoffs. Similarly, Garcia-Louzao (2022) illustrates that a Spanish reform reducing severance pay led more workers to quit their job when exposed to a mass layoff. With respect to these studies, we exploit causal variation in job insecurity that is not related to mass layoffs - a very specific type of job risk - but is generated by a comprehensive labour market reform. This feature of our study allows us to draw specific policy implications for the design of employment protection reforms. Our study is the first to focus on graded security contracts and study their effects on workers' job insecurity and mobility. Our results on job insecurity bring relevant implications for the targeting of social security schemes by tenure levels. In addition, we provide first evidence

on the heterogeneous effects on mobility and effort provision by the quality of the previous job.

Overall, our paper builds a bridge between these two strands of literature on employment protection, as we show that the impact on workers' job insecurity and mobility decisions of eliminating protection against wrongful discharge is substantially weakened by the provision of higher severance pay.

In conclusion, our findings underscore a challenging trade-off inherent in the design of employment protection systems. On one hand, the need to mitigate the insecurity experienced during the initial stages of an employment contract calls for the implementation of targeted social insurance tools. On the other hand, the provision of such tools may inadvertently curtail the positive effects on upward mobility and labor market efficiency that stem from a reduction in employment protection. This emphasizes the need for nuanced, well-informed policies that account for the multifaceted impact of employment protection on both individual job security and the broader functioning of the labor market.

2 Employment protection legislation in Italy and the 2015 Jobs Act

Historically, permanent employees in the Italian labour market have been strongly protected against individual and collective dismissals. Under Article 18 of the 1970 Labour Rights Charter, layoffs were allowed only in case of just cause. However, in the event of an unfair dismissal confirmed by a court ruling, the employer was obligated to either reinstate the worker or provide compensation equivalent to 15 months' pay. Such regulation only applied to large firms, those above the 15-employee threshold, as the employment protection legislation did not apply to smaller firms. This dualism in size-contingent firing costs was addressed by a modification of Article 18 in 1990. Law N.108/1990 introduced several restrictions on dismissals and provided the possibility of reinstatement alternatively to a lower severance

payment for firms with less than 15 employees. However, different from large firms, the employer could decide its preferred option between reintegration and a compensation package. More recently, law N.92/2012 (the Monti-Fornero social security reform) reduced firing costs for workers employed in large firms in the case of unfair dismissals. This reform limited judges' discretion between reinstatement and monetary compensation, reduced the amount of the severance payment, and shortened the litigation duration. However, in many cases the reinstatement clause was preserved.

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. Law N.183 of December 20, 2014, commonly known as the Jobs Act, introduced a new permanent contract for employees hired in firms above 15 employees, the *graded security contract*, to ease the employment protection legislation. The new open-ended contract limits reinstatement to discriminatory and very specific disciplinary dismissals and prescribes that also unfair layoffs must be compensated by a monetary payment defined by law. Importantly, the compulsory severance payment in case of employment termination is flat and equal to 4 months of pay for the first 2 years after hiring, and then increases with tenure by two months of salary for every year of tenure, up to 24 months' wages at a 12-year tenure level. In addition, the reform introduced a new out-of-court procedure to settle down layoff disputes. In this case, the worker renounces any appeals to courts and the employer pays an allowance equal to 2 monthly wages in the first two years of tenure and then an additional 1 monthly wage per year of service, up to 18 monthly wages after 18 years of service.

As a result, this new compensation schemes pre-set by law not only reduced the firing costs faced by large firms, but also the uncertainty related with employment termination, since dismissal disputes were no longer dependent on the arbitrary nature of court rulings.

The new firing regulation only applies to permanent workers hired in large firms from March 7, 2015. Open-ended workers employed in firms above the 15-employee threshold before the Jobs Act implementation maintained the reinstatement clause. In addition, the

new legislation states that all workers (not only new hires) employed in small firms surpassing the size threshold would be subject to the new dismissal rules. Finally, workers in firms below 15 employees do not experience any change in employment protection, as their permanent contract was already flexible with a maximum severance payment of 6 months. Importantly, the “graded security” component of the new employment contract was substantially weakened by a decision of the constitutional court dating 26 September 2018 and published on 8 November 2018. The court deemed the determination of compensation in case of unjust dismissal based solely on tenure as unconstitutional, and re-instated judges’ freedom in determining the amount of this compensation within the bands prescribed by the Jobs Act.

The 2015 Budget Law also introduced a sizeable hiring subsidy for new hires on an open-ended basis from January 1, 2015. The subsidy consisted of a 3-year reduction in social contributions paid by the firm, with a cap at €8,060 per year and worker. Only workers without a permanent position in the previous 6 months were eligible for the tax rebate. Moreover, the subsidy applies to both large and small firms.²

3 Data

3.1 Italian Labour Force Survey (LFS)

We rely on repeated cross-sectional data drawn from the Italian Labour Force Survey (LFS) conducted by the Italian National Institute of Statistics (ISTAT). The dataset provides quarterly detailed information on labour market status and other socio-economic characteristics for a representative sample of the Italian population (about 95,000 observations per quarter).

As the new graded security contract applies only to permanent full-time employees in the private sector, our sample only comprises workers hired with these contractual arrangements between 2010 and 2018 - a nine-year window around the introduction of the Jobs Act - and

²The size and availability of the subsidy also changed over time. See [Ardito et al. \(2020\)](#) for details.

surveyed between the first quarter of 2013 and the third quarter of 2018. We need to start in 2013 as questions on perceived job insecurity, our main outcome variable, were not included in the survey before that year. In order to test the effects of increasing severance pay as tenure increases, we focus on employees with a tenure level of up to 3 years, and distinguish between workers with up to two years of tenure - who are in the first step of the graded security contract and entitled to a severance pay equivalent of four months of wage - and those with three years - who are in the second step and entitled to a severance pay equal to six months of wage. We stop at 3 years because this is the duration of the hiring subsidy introduced by the Jobs Act, and dismissals may increase after the subsidy is over, thereby leading to selective attrition from the sample. Table A1 reports the minimum and maximum tenure of workers observed in our final sample by hiring year and interview year. In addition, we limit our sample to 2018q3 because of the Constitutional Court ruling that weakened the ex-ante determination of the severance payment in case of layoff for those hired in large firms after the Jobs Act introduction.

Our main measure of perceived insecurity concerns workers' fear of job loss, that is measured in the LFS using the following question: "*Do you think it is likely that your job will end within the next six months?*". Workers can answer Yes or No.³ The LFS also provides us with self-reported information on on-the-job search, that is surveyed with this question: "*Are you looking for another job?*", to which workers can answer yes or no. We also study effect on other labour market outcomes that may be influenced by a perceived increase in job insecurity, such as hours worked, overtime and weekend work, and monthly net earnings.

According to the Jobs Act, the new graded security contract only applies to workers hired from March 7, 2015 in firms employing more than 15 workers. Workers in our sample hired in small firms or in large firms before the Jobs Act have open-ended contracts, as described in Section 2. We differentiate between small and large firms using a dummy labeled "Large",

³As an alternative measure we also use workers' job stability perceptions, that is surveyed in the LFS using the following question: "*How satisfied are you with job stability?*". Workers can assign a score ranging between 0 (not satisfied at all) and 10 (extremely satisfied).

that takes value 1 for those working in firms above the 15-employees threshold at the time of interview, and 0 otherwise⁴. Unfortunately, the LFS does not disclose the exact hiring date. In addition, although the data recorded by the LFS refer specifically to the week before the interview, information on the interview week is not released. Hence, we reconstruct the time discontinuity implied by the Jobs Act by adopting the following procedure. First, we impute the reference week to the middle of the quarter of the interview (i.e., mid-February for Q1, mid-May for Q2, etc). Then, we recover the approximate hiring date by computing the difference between the reference week and the number of months since the worker began the current job. Finally, we create a dummy “Jobs Act” that is equal to 1 if the approximate hiring date falls after March 7, 2015, and to 0 otherwise.

Our sample comprises 43,356 observations and includes respondents with no missing values for the following variables: gender, age, education (in years), tenure (in years), immigrant status, marital status, job position (blue or white collar), 1-digit ATECO sector of employment, region of residency, quarters, and years.

Table A2 reports the main descriptive statistics for the sample. One-third of workers are women, their average age is almost 39 years, they have a relatively low level of education (about 11 years), are married in one out of two cases, roughly 24% are immigrants (non-Italian citizens), 79% resides in Northern regions, and most of them are employed in blue-collar jobs (68%), with an average monthly earnings of €1,244.3.

Roughly 44% of workers are employed in large firms (above the 15-employee threshold) and close to 32% are hired after the introduction of the new permanent contract. In total, 14% of the sample is employed in large firms and hired after March 7, 2015, and thus subject to the new graded security contract introduced with the Jobs Act. This contract envisages that severance pay is increasing with tenure. In the full sample, workers’ average tenure in the job is 1.7 years. Specifically, 59% of workers have a tenure level of at least 2 years, and

⁴We drop from our sample workers declaring that “*she does not know the exact number of employees*”, “*she does not know the exact number of employees, but this number is greater than 10*”, and “*she does not know the exact number of employees, but this number is smaller than 10*”.

41% have a seniority of 3 years.

Concerning our main outcome variables, 9% fear losing their job over the next 6 months and 4% are currently searching for a new job. On average, employees work for slightly more than 40 hours a week, only 1% are on sick leave during the reference week, 5% do overtime, and 42% work during weekends (Saturday and Sunday).

3.2 LoSai (Longitudinal Sample INPS) administrative records

We complement the analysis on perceptions and self-reported behaviours using administrative data from the Italian Social Security Institute (INPS). We gather data on workers from LoSai (Longitudinal Sample INPS), a matched employer-employee dataset that contains information on a random sample of the universe of workers in the Italian non-agricultural private sector, covering approximately 7% of the universe of employees over the period 1985–2018⁵.

LoSai contains individual-level information on employment and earnings histories. It records annual gross earnings, the number of days worked in a year, the type of contract (full-time or part-time and permanent or temporary), and broad occupation categories (apprentice, blue-collar, white-collar, middle manager, manager). The data also report demographic information, including year of birth, gender, and region of residence. Information on firms includes firm size in discrete brackets⁶ and the firm’s 2-digit industry, based on the NACE Rev. 2 classification.

We use LoSai to measure separations, job-to-job mobility, and yearly days worked - that we take as our main measure of workers’ effort.

Consistent with the analysis on the LFS, we consider all permanent full-time contracts started between March and December 2014 and between March and December 2015. Given that our data end in December 2018, we can observe the careers of these workers up to three

⁵Sampled workers are those born on the 1st and 9th day of each month.

⁶Firm size brackets are defined as follows: 0-5, 6-10, 11-15, 16-20, 21-35, 26-30, 31-40, 41-50, 51-100, 101-200, 201-300, 301-400, 401-500, ≥ 500 .

years after hiring without incurring in issues related with right censoring. As we have done with the LFS data, we limit our sample to September 2018 because of the Constitutional Court ruling that weakened the ex-ante determination of the severance payment in case of layoff. From this dataset, we build three different samples devoted to investigating the effect of the Jobs Act on separations, job-to-job mobility, and yearly days worked.

First, we measure separations using information on causes of job termination, and only include those instances mentioning dismissal or collective layoff. Using this definition, in the data we observe 19,055 separations out of 106,532 recorded employment spells for 76,145 workers. 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 compute separations as the probability of being still employed in firm f at 6, 12, 18, 24, 30, and 36 months of tenure. Descriptive statistics for this sample are reported in the first panel of Table [A3](#).

Second, we construct the sample on job-to-job mobility following a similar logic. We measure mobility considering workers with permanent positions who voluntarily resign from their current employer and find a new open-ended job, and exclude instead workers experiencing any other motivation behind an early termination of their contract. Like in the case of separations, we then construct a monthly panel and estimate the probability of job-to-job mobility at 6, 12, 18, 24, 30, and 36 months of tenure. This sample includes 79,579 permanent employment spells for 63,619 workers, and we observe 25,084 transitions across jobs. The second panel of Table [A3](#) reports the corresponding descriptive statistics.

Finally, the sample used for the analysis on workers' effort comprises workers hired full-time and with a permanent contract between March and December of 2014 and between March and December of 2015, who worked for at least one day and earned at least 1€. Since the number of days worked, our proxy for workers' effort, is recorded on a yearly basis, we cannot construct a monthly panel for this analysis. We compute tenure as the difference between the hiring date and the end of each calendar year (2015-2018), measured in months.

If a termination date is available, tenure is measured as the actual difference between the hiring date and the effective termination date. Hence, although our measure of days worked varies only on a yearly basis, we effectively take into account that the potential number of days worked depends on the exact duration of the contract by carrying out our analysis by different levels of tenure. For this analysis, we observe 105,927 employment spells for 102,054 employees. The descriptive statistics of this sample are depicted in the third panel of Table [A3](#).

4 Empirical strategy

Our research design exploits the Jobs Act reform as a source of quasi-experimental variation in employment protection to estimate its causal effect on workers' outcomes. The new legislation implies lower employment protection for workers hired in large firms after March 7, 2015, and thanks to the graded security mechanism it grants severance payments that increase with tenure. Within each level of tenure, the difference-in-differences approach identifies the effect of interest by comparing the average job insecurity reported by workers hired in large vs. small firms and after vs. before the Jobs Act.

Let us exemplify our strategy by focusing on the LFS data. We estimate with Ordinary Least Squares (OLS) the following model:

$$Y_{i,t} = \alpha + \beta \text{LARGE FIRM}_{i,t} + \gamma \text{JOBS ACT}_{i,t} + \delta \text{LARGE FIRM}_{i,t} \times \text{JOBS ACT}_{i,t} + \theta \mathbf{X}_{i,t} + \lambda_t + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is an outcome of interest observed for worker i at the survey interview carried out in time period t ; $\text{LARGE FIRM}_{i,t}$ is a dummy equal to 1 for employees in large firms (more than 15 employees) and to 0 otherwise; $\text{JOBS ACT}_{i,t}$ is a dummy equal to 1 for employees hired after 7 March 2015 and to 0 otherwise; $\text{LARGE FIRM}_{i,t} \times \text{JOBS ACT}_{i,t}$ is an interaction term whose coefficient δ measures the treatment effect of interest. The vector of controls $\mathbf{X}_{i,t}$ considers individual characteristics including age, age squared, sex, education, marital status, immigrant, region of residence and 1-digit ATECO sector of employment.

Moreover, λ_t is a vector of interview time (quarter-by-year) dummies that allow us to eliminate time effects that are constant for workers in small and large firms and interviewed at the same time period. Finally, $\varepsilon_{i,t}$ is an error term. In all specifications, standard errors are clustered at Large \times Year-Quarter level to take into account the intra-cluster correlation of error terms within groups defined by treatment group and interview time (Bertrand et al., 2004). Equation (1) is estimated separately for two different tenure levels: [0-2] years, and (2-3] years, and we use seemingly unrelated estimation to test the equality of the treatment effect of interest across the two samples. 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 threats challenge the internal validity of our identification strategy. First, by lowering the firing costs sustained by large firms, the implementation of the Jobs Act may have led to a change in the distribution of workers by tenure across small and large firms. If this was the case, the detection of heterogeneous effects of the Jobs Act by tenure would be complicated by the impact of the reform on tenure itself. We verify this by estimating Equation (1) in the full sample, using both the level of tenure and the low tenure probability as dependent variable. Table 1 reports the results. Reassuringly, we do not detect any significant change in the probability of observing workers with low (0-2 years) vs. high (3 years) tenure in small and large firms around the implementation of the Jobs Act. Most likely, the 3-year hiring subsidies introduced by the Jobs Act has attenuated firms' interest in terminating employment contract before the end of the incentive, despite the lower firing cost introduced by the Jobs Act for workers with low tenure. While this is reassuring in terms of internal validity, the extrapolation of our findings to other graded-security schemes that do not involve a hiring subsidy may be difficult.⁷

⁷We further assess the robustness of our findings to the potential selection strategically implemented by employers at the first step of the graded security mechanism by re-estimating Equation 1 after fictitiously 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 A4 reassure us on this matter, as the effect of interest is not only insignificant and small in magnitude - as in Table 3 - but it also turns negative.

Second, our strategy relies on the assumption that trends in outcomes would have been the same in both groups in the absence of the policy change (i.e., the “common trends” assumption). Unfortunately, the usual dynamic Difference-in-Differences specification with lags and leads à-la Autor (2003) is unfeasible in our setting because, conditional on tenure, hiring time and interview time are collinear (see Table A1). This mechanical relationship implies that, for workers with a given tenure level and conditional on interview time dummies, we can only identify a pooled effect across workers hired before vs. after the introduction of the Jobs Act. As an alternative to a dynamic specification, we rely on a series of placebo exercises. We construct some fictitious reforms spanning from 2013Q2 to 2014Q3, that we also interact with the $LARGE_{i,t}$ dummy. These results, available in Table A5, show that only one out of six interaction terms is significant at the 10 percent level, suggesting the absence of differential pre-trends.⁸

Another potential concern about our design is that the sorting of workers into large and small firms could have changed as a result of the introduction of the Jobs Act. Although we control for a large battery of observable characteristics of workers, we assess the robustness of the results to selection on unobservables with an instrumental variables 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.

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.

⁸These estimates also rule out concerns on potential impacts on workers’ perceived job stability of the previous 2012 EPL reform introduced by the Monti-Fornero government.

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. The exclusion restriction is instead based on the assumption that the pre-reform share of large-firm employees in a given sector and region affects jobs security perceptions only through its effect on the probability of being employed in a large firm.

On top of strengthening our findings with respect to selection on unobservables, our IV strategy also alleviates concerns about measurement error in the self-reported firm size. Indeed, in our main analysis a firm is considered large if it has more than 15 employees as stated by the respondent, while in our IV analysis we use 50+ employees as definition for large firms. Finally, since our instrument is defined in a pre-Jobs Act period, this analysis also deals with the potential sorting of firms around the 15-employee threshold induced by the reform (see also [Garicano et al., 2016](#)).⁹

The approach we adopt to investigate the effects of the graded security contract 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.

When investigating the effects on job-to-job mobility, we also analyse whether the effects of interest vary according to the quality of workers' origin and destination firm. We measure firm quality following [Casarico and Lattanzio \(2023\)](#), and use firm fixed effects from an AKM two-way fixed effects regressions ([Abowd et al., 1999](#)). To this end, we consider all workers that have ever been employed at one of the firms in our sample and all firms that have ever hired one of the workers in the sample.¹⁰ Specifically, we estimate the following regression:

⁹By 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.

¹⁰For workers holding multiple contracts in a year, in the main analysis we only retain the information on the job with the longest duration or the highest earnings. Furthermore, we estimate AKM firm effects using

$$w_{ijt} = \alpha_i + \phi_j + \beta X_{i,t} + \eta_{i,t} \quad (2)$$

where w_{ijt} is log weekly wages of worker i working in firm j in year t . α_i and ϕ_j are worker and firm fixed effects, respectively, $X_{i,t}$ contains time-varying observables (cubic polynomials in age and tenure, type of contract dummies, occupation dummies, part-time dummy, and their interaction with a gender dummy) and year fixed effects, and $\eta_{i,t}$ is an error term. We then define as high quality firms those with above-median estimated firm effects $\widehat{\phi}_j$.

5 Results

5.1 Effects on perceived and actual layoff risk

5.1.1 Perceived job insecurity

Descriptive evidence and OLS estimates Descriptive evidence on the effect of the Jobs Act on fear of job loss is displayed in Table 2, where we report in a 2×2 matrix the share of workers fearing job loss by firm size and hiring date. To account for the differences in severance pay by tenure that come with the graded security contract, we do this separately for workers with low and high tenure level. We see that, 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

both permanent and temporary workers, even if the main analysis only focuses on the former. Finally, as information on hiring dates is only recorder from January 1, 2005, we only retain contracts started from this date onwards.

tenure, for whom severance pay is increased.

The OLS estimates of Equation (1) are reported in Table 3. 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 2. This is a reassuring finding in terms of the internal validity of the design. Table 3 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.

Robustness tests Table A6 presents a series of exercises aiming at probing the robustness of our main results, reported in Table 3. 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. Panel C illustrates instead that the inclusion of region-specific linear time trends does not alter our estimated effects, thus ruling out the possibility that local-level trends may confound our identification strategy. Next, in Panels D and E we respectively show that results not only hold when we exclude workers reporting to be employed in firms with 10-19 or 10-49 employees, but they also become larger in magnitude. For these workers, misclassification of firm size may be a salient issue, and this potential measurement error may lead to an attenuation bias in our estimated effects in Table 3. Additionally, Panel F 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. Finally, Table A7 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. We thus conclude that our main findings are robust to the inclusion of different sets of controls and trends, to using alternative estimators, sample selection criteria, and measures of the outcome of interest.

IV estimates We further assess the validity of our findings to selection into small or large firms along unobserved variables by adopting the IV strategy described in Section 4. The top panel of Table 4 reports the first-stage coefficients. Our chosen instrument - the percentage 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.¹¹ 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 implies that a 1SD increase in the value of the instrument would increase a worker's probability of being employed in a large firm by 6.9 percentage points, or 16% of the sample mean (43.7%).

The second-stage coefficients are reported in the bottom panel of Table 4. Consistent with our main OLS specification of Table 3, we find a negative and significant effect 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).¹² 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

¹¹Table A8 and Table A9 report 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.

¹²Table A10 replicates for the IV estimates the robustness tests presented in Table A6 for the OLS estimates (excluding of course the imputation estimator). The estimates are stable across different specifications.

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.¹³

Heterogeneous effects We next investigate whether the effects on fear of job loss are heterogeneous depending on several observable characteristics of workers, including gender, age, previous employment contract, area of residence, occupation and earnings level. The OLS estimates, reported in Table A11, 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.

5.1.2 Actual continuation rates

Do the impacts on perceived job insecurity mirror hard evidence on layoffs? To test this, we investigate whether employment continuation rates every semester - i.e., the probability of being still employed in the same firm - differ across workers hired in large vs small firms and before vs after the Jobs Act using our administrative data and the separation definition presented in Section 3.2. According to Figure 1 and Table A12 - that report the difference-in-differences estimates of the effects of the graded security contract on continuation rates at different levels of tenure - 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. 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

¹³This 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.

impact on continuation rates declines from -9.2% to -0.4% as tenure increases from 6 to 24 months.

5.2 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.

5.2.1 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 5, 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 1.2 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 21% 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 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.

5.2.2 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 2 and of Table A13 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 2 shows that higher mobility is observed for low-tenure graded security workers in low-paying sectors (see Table A14).

In Figure 3 we investigate whether mobility allows workers to achieve better wages or job positions. Panel **a** of Figure 3 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 mean group (see panel B of Table A13). 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 3 and Table A13 (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 3 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.

Finally, in Figure 4 and Table A15 we document heterogeneous effects on job-to-job mobility depending on the quality of the origin and destination firms. As described in Section 4, we define as high quality firms those with above-median estimated firm effects $\widehat{\phi}_j$, obtained through equation (2). Panel **a** of Figure 4 shows that workers employed in low quality firms - who are characterized by lower job attachment and more intense job searching - are more likely to voluntarily resign and transition to a new graded security contract than those in higher quality firms. Even in this case, the estimated effects are large in magnitude, and range between 36% and 20% of the control group mean - depending on tenure. Furthermore, Panel **b** of Figure 4 distinguishes by the quality of the destination firm and displays positive, though statistically insignificant, effects on mobility towards high-quality firms and significant negative effects on mobility towards low-quality firms.

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 and in low-quality firms, are more likely to voluntarily quit the job and move to better-paid jobs and to higher-quality firms.¹⁴

5.2.3 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.¹⁵ Considering that workers who are paid relatively

¹⁴Even in this case, we find no evidence of heterogeneous effects either by age or by gender.

¹⁵We have also considered annual gross earnings. However, we find no effects.

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 A16 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.¹⁶

Finally, in Table A17, 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 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 €25/month or 2% of the average monthly earnings

¹⁶Even in this case, we fail to detect significant heterogeneous effects by age and gender.

observed among control group workers.¹⁷

6 Conclusions

In this paper, we investigate the effects of a reduction in employment protection on workers' perceived and actual job security as well as on their job search and mobility behavior. We exploit the variation induced by the 2015 Italian Jobs Act, which reduced employment protection and introduced a sharp discontinuity in severance payments at the 2-year tenure for all new hires on a permanent contract in large firms, while leaving comparable workers in small firms unaffected. We combine LFS data with administrative records, and use a difference-in-differences strategy to estimate the effects of interests.

We can summarise our findings as follows. First, we document that the new graded security contract introduced by the Jobs Act significantly increased job insecurity for workers with 0-2 years of tenure, for whom severance payment is flat and low, but the effect fades once they reach 3 years of tenure and gain the entitlement to a 50% higher severance payment.

Second, our analysis reveals that, as a result of the higher insecurity, low-tenure graded security employees exhibit a higher likelihood of searching for and transitioning to a new job compared to their counterparts hired before the Jobs Act. Notably, we document that workers in low-paying sectors and low-quality firms are more likely to transition to higher-quality firms and better-paying jobs. Again, these effects vanish for higher tenure workers, who enjoy greater severance pay.

Third, for workers in high-paying sectors, we find that low-tenure treated employees tend to exert more effort than comparable workers in the control group. Specifically, we show that the effect on days worked is much larger for workers with higher-than-median sector specific wages. This evidence suggests that high-pay workers face greater incentives to signal their attachment to the firm to secure their jobs.

¹⁷We 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.

By reporting evidence of a trade-off between job security and job mobility induced by the Italian Jobs Act, our findings have important policy implications that shall be accounted for in the design of future employment protection reforms in Europe. Specifically, our findings underscore the need to strike a delicate balance between addressing job insecurity through the enhancement of more effective or generous unemployment insurance and social safety nets - especially in the earliest stages of the employment contract - while concurrently fostering labor market efficiency and promoting increased job mobility.

References

- Abowd, J. M., F. Kramarz, and D. N. Margolis**, “High wage workers and high wage firms,” *Econometrica*, 1999, *67* (2), 251–333.
- Akerlof, G. A., A. K. Rose, J. L. Yellen, L. Ball, and R. E. Hall**, “Job Switching and Job Satisfaction in the U.S. Labor Market,” *Brookings Papers on Economic Activity*, 1988, *2*, 495–594.
- Ardito, C., F. Berton, and L. Pacelli**, “Jobs Act e incentivi alle assunzioni: una visione d’insieme,” *Etica ed Economia*, 2020, *117*, 1–4.
- Autor, D. H.**, “Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing,” *Journal of Labor Economics*, 2003, *21* (1), 1–42.
- Autor, David H., John J. Donohue, and Stewart J. Schwab**, “The Employment Consequences of Wrongful-Discharge Laws: Large, Small, or None at All?,” *American Economic Review*, 2004, *94* (2), 440–446.
- , **William R. Kerr, and Adriana D. Kugler**, “Does Employment Protection Reduce Productivity? Evidence From US States,” *The Economic Journal*, 2007, *117* (521), F189–F217.
- Barceló, C. and E. Villanueva**, “The response of household wealth to the risk of job loss: Evidence from differences in severance payments,” *Labour Economics*, 2016, *39*, 35–54.
- Bassanini, A., L. Nunziata, and D. Venn**, “Job protection legislation and productivity growth in OECD countries,” *Economic Policy*, 2009, *24* (58), 349–402.
- Behaghel, L., B. Crépon, and B. Sédillot**, “The perverse effects of partial employment protection reform: The case of French older workers,” *Journal of Public Economics*, 2008, *92* (3-4), 696–721.

- Benito, A.**, “Does job insecurity affect household consumption?,” *Oxford Economic Papers*, 2006, *58*, 157–181.
- Bentolila, S. and G. Bertola**, “Firing Costs and Labour Demand: How Bad is Eurosclerosis?,” *The Review of Economic Studies*, 1990, *57* (3), 381–402.
- , **J. J. Dolado**, and **J. F. Jimeno**, “Two-tier employment protection reforms: The Spanish experience,” *CESifo DICE Report*, 2008, *6* (4), 49–56.
- , – , and – , “Reforming an insider-outsider labor market: the Spanish experience,” *IZA Journal of European Labor Studies*, 2012, *1* (1), 1–29.
- Berton, F. and P. Garibaldi**, “Workers and firms sorting into temporary jobs,” *The Economic Journal*, 2012, *122* (562), F125–F154.
- Bertrand, M., E. Duflo, and S. Mullainathan**, “How Much Should We Trust Differences-In-Differences Estimates?,” *The Quarterly Journal of Economics*, 02 2004, *119* (1), 249–275.
- Bjuggren, C. M.**, “Employment protection and labor productivity,” *Journal of Public Economics*, 2018, *157*, 138–157.
- Blanchard, O. and P. Portugal**, “What Hides behind an Unemployment Rate: Comparing Portuguese and U.S. Labor Markets,” *American Economic Review*, 2001, *91* (1), 187–207.
- Blanchard, O. J. and J. Tirole**, “The Joint Design of Unemployment Insurance and Employment Protection: A First Pass,” *Journal of the European Economic Association*, 2008, *6* (1), 45–77.
- Boeri, T. and J. F. Jimeno**, “The effects of employment protection: Learning from variable enforcement,” *European Economic Review*, 2005, *49* (8), 2057–2077.

- **and P. Garibaldi**, “Two tier reforms of employment protection: a honeymoon effect?,” *The Economic Journal*, 2007, *117*, 357–385.
 - **and** — , *Un nuovo contratto per tutti*, Chiarelettere Milano, 2008.
 - **and** — , “A tale of comprehensive labor market reforms: evidence from the Italian jobs act,” *Labour Economics*, 2019, *59*, 33–48.
 - , — , **and E. R. Moen**, “Inside severance pay,” *Journal of Public Economics*, 2017, *145*, 211–225.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess**, “Revisiting event study designs: Robust and efficient estimation,” *arXiv preprint arXiv:2108.12419*, 2021.
- Buffel, V., R. Dereuddre, and P. Bracke**, “Medicalization of the uncertainty? An empirical study of the relationships between unemployment or job insecurity, professional care seeking, and the consumption of antidepressants,” *European Sociological Review*, 2015, *31*, 446–459.
- Butschek, S. and J. Sauermann**, “The effect of employment protection on firms’ worker selection,” *Journal of Human Resources*, 2022, pp. 0919–10433R1.
- Böckerman, P., P. Ilmakunnas, and E. Johansson**, “Job security and employee well-being: Evidence from matched survey and register data,” *Labour Economics*, 2011, *18* (4), 547–554.
- Cahuc, P.**, “For a unified contract,” *European Labour Law Journal*, 2012, *3* (3), 191–206.
- **and F. Kramarz**, “From insecurity to mobility: towards professional social security,” *Report for the Ministry of the Economy, Finance, Employment*, 2005.
 - **and F. Postel-Vinay**, “Temporary jobs, employment protection and labor market performance,” *Labour economics*, 2002, *9* (1), 63–91.

- , **F. Malherbet**, and **J. Prat**, “The detrimental effect of job protection on employment: Evidence from France,” *CEPR Discussion Paper No. 13767*, 2019.
- , **O. Charlot**, and **F. Malherbet**, “Explaining the spread of temporary jobs and its impact on labor turnover,” *International Economic Review*, 2016, *57* (2), 533–572.
- Cappellari, L., C. Dell’Aringa, and M. Leonardi**, “Temporary employment, job flows and productivity: A tale of two reforms,” *The Economic Journal*, 2012, *122* (562), F188–F215.
- Carr, E. and H. Chung**, “Employment insecurity and life satisfaction: the moderating influence of labour market policies across Europe,” *Journal of European Social Policy*, 2014, *24*, 383–399.
- Casarico, A. and S. Lattanzio**, “Behind the child penalty: understanding what contributes to the labour market costs of motherhood,” *Journal of Population Economics*, 2023, pp. 1–23.
- Cingano, F., M. Leonardi, J. Messina, and G. Pica**, “The effects of employment protection legislation and financial market imperfections on investment: evidence from a firm-level panel of EU countries,” *Economic Policy*, 2010, *25* (61), 117–163.
- , – , – , and – , “Employment protection legislation, capital investment and access to credit: evidence from Italy,” *The Economic Journal*, 2016, *126* (595), 1798–1822.
- Clark, A. and F. Postel-Vinay**, “Job security and job protection,” *Oxford Economic Papers*, 2009, *61* (2), 207–239.
- , **C. D’Ambrosio**, and **A. Lepinteur**, “Job insecurity, savings and consumption: an Italian experiment,” *Paper prepared for the 37th IARIW General Conference*, 2022.
- , – , and – , “Marriage as insurance: job protection and job insecurity in France,” *Review of Economics of the Household*, 2023.

- Clark, A. E.**, “What really matters in a job? Hedonic measurement using quit data,” *Labour Economics*, 2001, 8 (2), 223–242.
- Clark, Andrew E. and Anthony Lepinteur**, “A Natural Experiment on Job Insecurity and Fertility in France,” *The Review of Economics and Statistics*, 2022, 104 (2), 386–398.
- Daruich, D., S. Di Addario, and R. Saggio**, “The effects of partial employment protection reforms: Evidence from Italy,” *Review of Economic Studies*, 2023.
- De Cuyper, N. and H. De Witte**, “Job insecurity in temporary versus permanent workers: associations with attitudes, well-being, and behaviour,” *Work and Stress*, 2007, 21, 65–84.
- De Paola, M., R. Nisticò, and V. Scoppa**, “Employment protection and fertility decisions: The unintended consequences of the Italian Jobs Act,” *Economic Policy*, 2021, 36 (108), 735–773.
- Drobnič, S., B. Beham, and P. Präg**, “Good Job, Good Life? Working Conditions and Quality of Life in Europe,” *Social Indicator Research*, 2010, 99 (2), 205–225.
- Fraisse, H., F. Kramarz, and C. Prost**, “Labor disputes and job flows,” *ILR Review*, 2015, 68 (5), 1043–1077.
- Freeman, R. B.**, “Job satisfaction as an economic variable,” *American Economic Review*, 1978, 68, 135–141.
- Garcia-Louzao, J.**, “Workers’ job mobility in response to severance pay generosity,” *Labour economics*, 2022, 75 (102143).
- Garibaldi, P. and G. L. Violante**, “The employment effects of severance payments with wage rigidities,” *The Economic Journal*, 2005, 115 (506), 799–832.

- Garicano, Luis, Claire Lelarge, and John Van Reenen**, “Firm size distortions and the productivity distribution: Evidence from France,” *American Economic Review*, 2016, *106* (11), 3439–3479.
- Georgieff, A. and A. Lepinteur**, “Partial employment protection and perceived job security: evidence from France,” *Oxford Economic Papers*, 2018, *70* (3), 846–867.
- Gielen, Anne C and Konstantinos Tatsiramos**, “Quit behavior and the role of job protection,” *Labour Economics*, 2012, *19* (4), 624–632.
- Guariglia, A.**, “Saving behaviour and earnings uncertainty: Evidence from the British Household Panel Survey,” *Journal of Population Economics*, 2001, *14*, 619–634.
- Gómez-Salvador, R., J. Messina, and G. Vallanti**, “Gross job flows and institutions in Europe,” *Labour Economics*, 2004, *11* (4), 469–485.
- Hijzen, A., L. Mondauto, and S. Scarpetta**, “The impact of employment protection on temporary employment: Evidence from a regression discontinuity design,” *Labour Economics*, 2017, *46*, 64–76.
- Ichino, A. and R. T. Riphahn**, “The effect of employment protection on worker effort: Absenteeism during and after probation,” *Journal of the European Economic Association*, 2005, *3* (1), 120–143.
- Jappelli, T. and M. Padula**, “The Consumption and Wealth Effects of an Unanticipated Change in Lifetime Resources,” *Management Science*, 2016, *62* (5), 1458–1471.
- Kettemann, Andreas, Francis Kramarz, and Josef Zweimüller**, “Job mobility and creative destruction: flexicurity in the land of Schumpeter,” 2017.
- Kugler, A.**, “Wage-shifting effects of severance payments savings accounts in Colombia,” *Journal of public Economics*, 2005, *89* (2-3), 487–500.

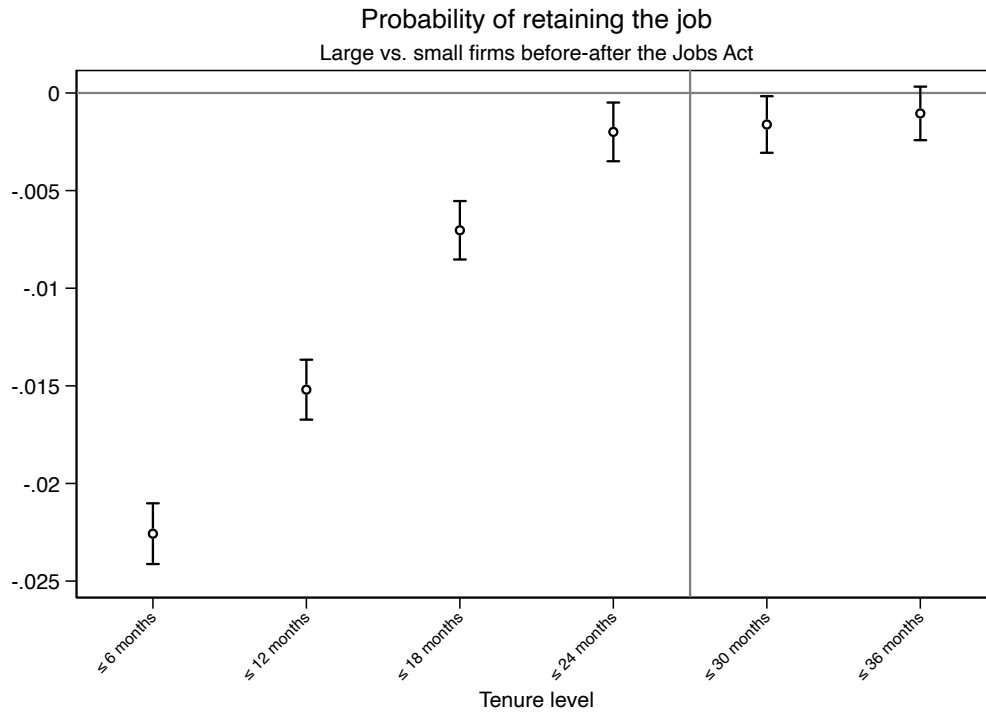
- **and G. Pica**, “Effects of employment protection on worker and job flows: Evidence from the 1990 Italian reform,” *Labour Economics*, 2008, *15* (1), 78–95.
 - **and G. Saint-Paul**, “How Do Firing Costs Affect Worker Flows in a World with Adverse Selection?,” *Journal of Labor Economics*, 2004, *22* (3), 553–584.
- Larson, J. H., S. M. Wilson, and R. Beley**, “The Impact of Job Insecurity on Marital and Family Relationships,” *Family Relations*, 1994, *43* (2), 138–143.
- Lazear, Edward P.**, “Job Security Provisions and Employment*,” *The Quarterly Journal of Economics*, 1990, *105* (3), 699–726.
- Leonardi, M. and G. Pica**, “Who pays for it? The heterogeneous wage effects of employment protection legislation,” *The Economic Journal*, 2013, *123* (573), 1236–1278.
- Lepinteur, A.**, “The asymmetric experience of gains and losses in job security on health,” *Health Economics*, 2021, *30* (9), 2217–2229.
- Lim, V. K. G. and G. L. Loo**, “Effects of parental job insecurity and parenting behaviors on youth’s self-efficacy and work attitudes,” *Journal of Vocational Behavior*, 2003, *63*, 86–98.
- Lugilde, A., R. Bande, and D. Riveiro**, “Precautionary saving in Spain during the great recession: Evidence from a panel of uncertainty indicators,” *Review of Economics of the Household*, 2018, *16*, 1151–1179.
- László, K. D., H. Pikhart, M. S. Kopp, M. Bobak, A. Pajak, and S. Malyutina**, “Job Insecurity and Health: A Study of 16 European Countries,” *Social Science & Medicine*, 2010, *70* (6), 867–874.
- MacLeod, W B. and V. Nakavachara**, “Can wrongful discharge law enhance employment?,” *The Economic Journal*, 2007, *117* (521), F218–F278.

- Marinescu, I.**, “Job security legislation and job duration: Evidence from the United Kingdom,” *Journal of Labor Economics*, 2009, *27* (3), 465–486.
- Martins, P. S.**, “Dismissals for cause: The difference that just eight paragraphs can make,” *Journal of Labor Economics*, 2009, *27* (2), 257–279.
- Marx, P.**, “The Effect of Job Insecurity and Employability on Preferences for Redistribution in Western Europe,” *Journal of European Social Policy*, 2014, *24* (4), 351–366.
- Mauno, S. and U. Kinnunen**, “Perceived job insecurity among dual-earner couples: do its antecedents vary according to gender, economic sector and the measure used?,” *Journal of Occupational and Organizational Psychology*, 2002, *75*, 295–314.
- Messina, J. and G. Vallanti**, “Job flow dynamics and firing restrictions: evidence from Europe,” *The Economic Journal*, 2007, *117* (521), F279–F301.
- Mistrulli, P. E., T. Oliviero, Z. Rotondi, and A. Zazzaro**, “Job Protection and Mortgage Conditions: Evidence from Italian Administrative Data,” *Oxford Bulletin of Economics and Statistics*, 2023.
- Origo, F. and Laura Pagani**, “Flexicurity and job satisfaction in Europe: The importance of perceived and actual job stability for well-being at work,” *Labour economics*, 2009, *16* (5), 547–555.
- Paskov, M. and F. Koster**, “Institutions, Employment Insecurity and Polarization in Support for Unemployment Benefits,” *Journal of European Social Policy*, 2014, *24* (4), 367–382.
- Pérez, J. I. G. and V. Osuna**, “Dual labour markets and the tenure distribution: Reducing severance pay or introducing a single contract,” *Labour Economics*, 2014, *29*, 1–13.

- Postel-Vinay, F. and H. Turon**, “The Impact of Firing Restrictions on Labour Market Equilibrium in the Presence of On-the-job Search,” *The Economic Journal*, 2013, *124* (575), 31–61.
- Pries, M. and R. Rogerson**, “Hiring Policies, Labor Market Institutions, and Labor Market Flows,” *Journal of Political Economy*, 2005, *113* (4), 811–839.
- Rook, K., D. Dooley, and R. Catalano**, “Stress transmission: the effects of husband’s job stressors on the emotional health of their wives,” *Journal of Marriage and the Family*, 1999, *53*, 165–177.
- Ruiz-Valenzuela, J.**, “Intergenerational effects of employment protection reforms,” *Labour Economics*, 2020, *62*, 101774.
- Schivardi, F. and R. Torrini**, “Identifying the effects of firing restrictions through size-contingent differences in regulation,” *Labour Economics*, 2008, *15* (3), 482–511.
- Sestito, P. and E. Viviano**, “Firing costs and firm hiring: evidence from an Italian reform,” *Economic Policy*, 2018, *33* (93), 101–130.
- Von Below, D. and P. S. Thoursie**, “Last in, first out?: Estimating the effect of seniority rules in Sweden,” *Labour Economics*, 2010, *17* (6), 987–997.

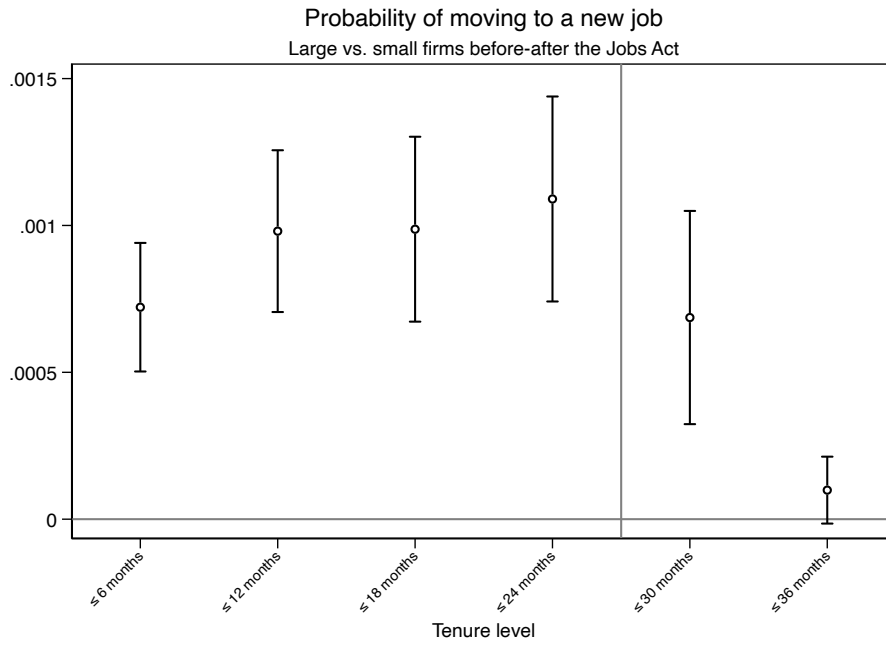
Figures and Tables

Figure 1: Continuation Rates

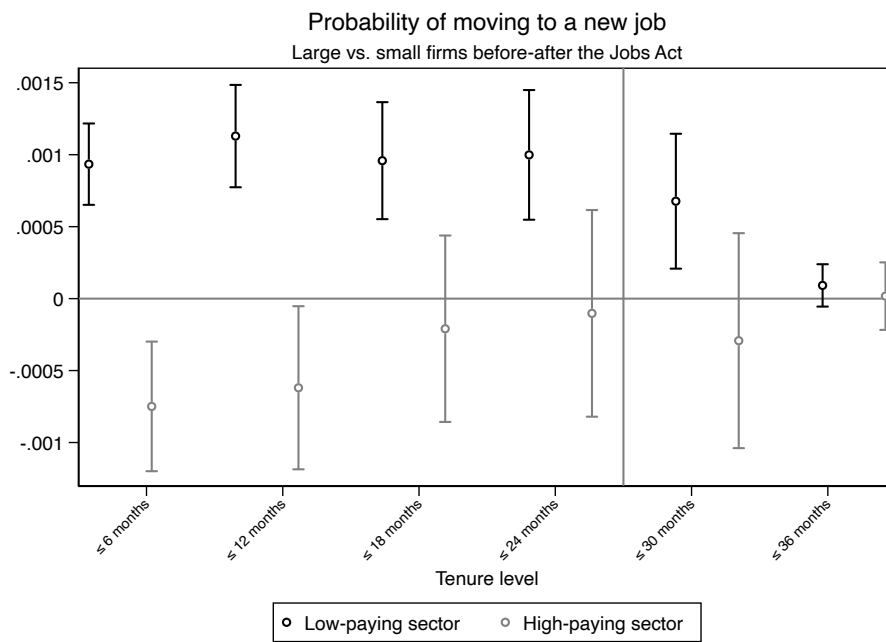


Notes: The Figure reports the difference-in-differences estimates available in Table [A12](#). The dependent variable is the probability of retaining the job in firm f at month tenure m .

Figure 2: Job-to-Job Mobility



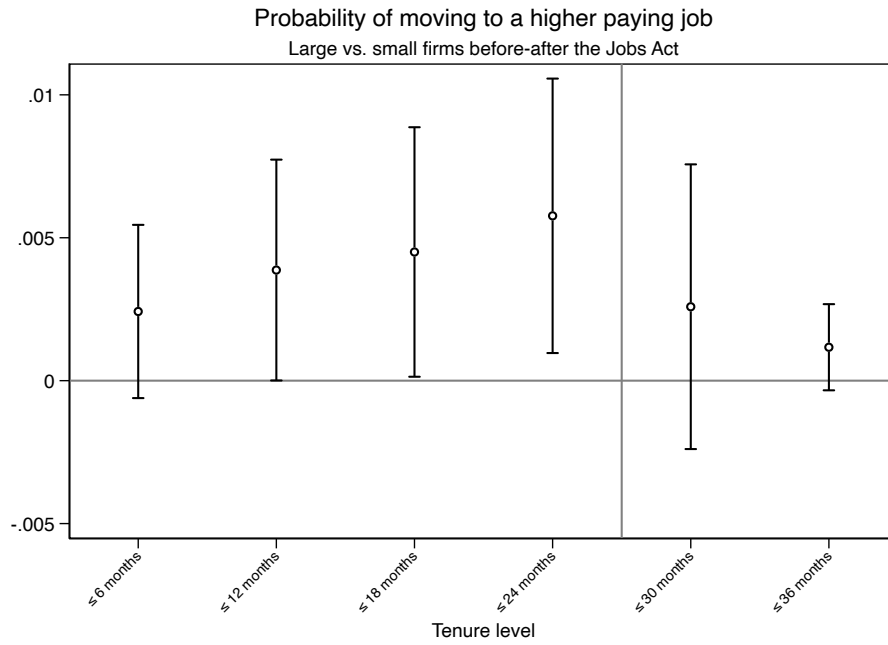
(a) Overall



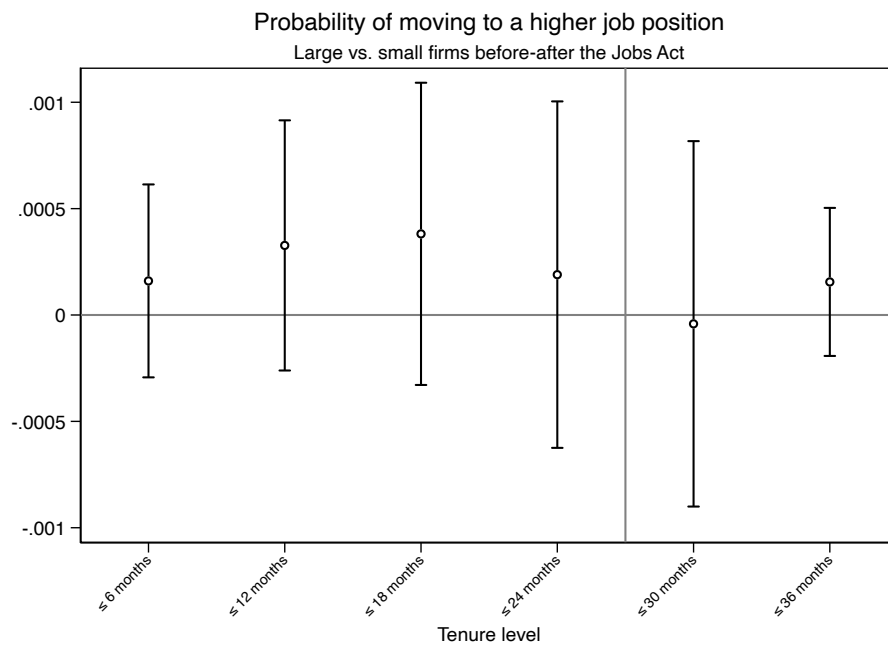
(b) By sector of employment

Notes: Panel **a** of the Figure reports the difference-in-differences estimates available in Panel A of Table [A13](#). The dependent variable is the probability of moving to a new job at month tenure m . Panel **b** reports the estimates available in Table [A14](#). High-paying sectors are defined as those with daily gross earnings above the yearly specific median.

Figure 3: Job-to-Job Mobility: Wage and Career Progressions



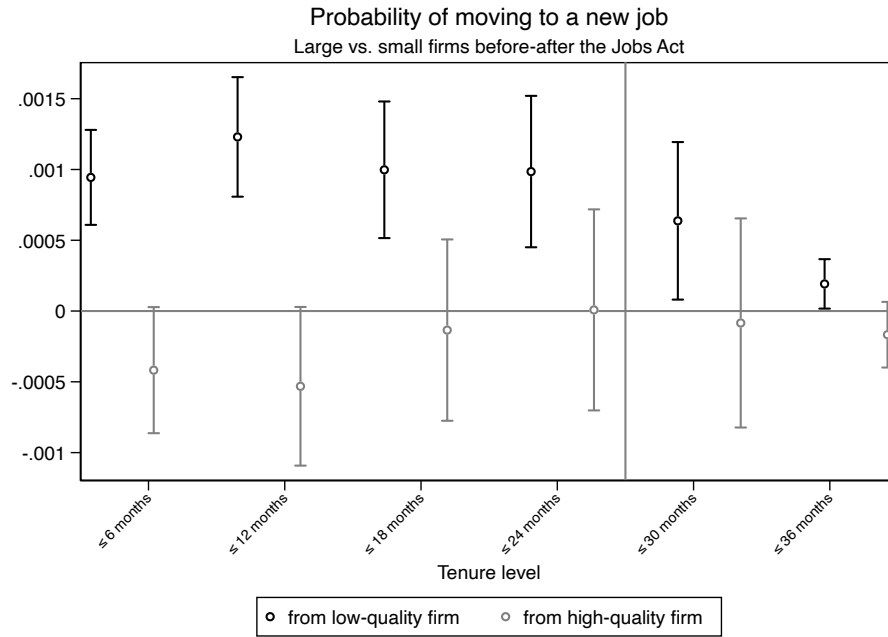
(a) Wage progression



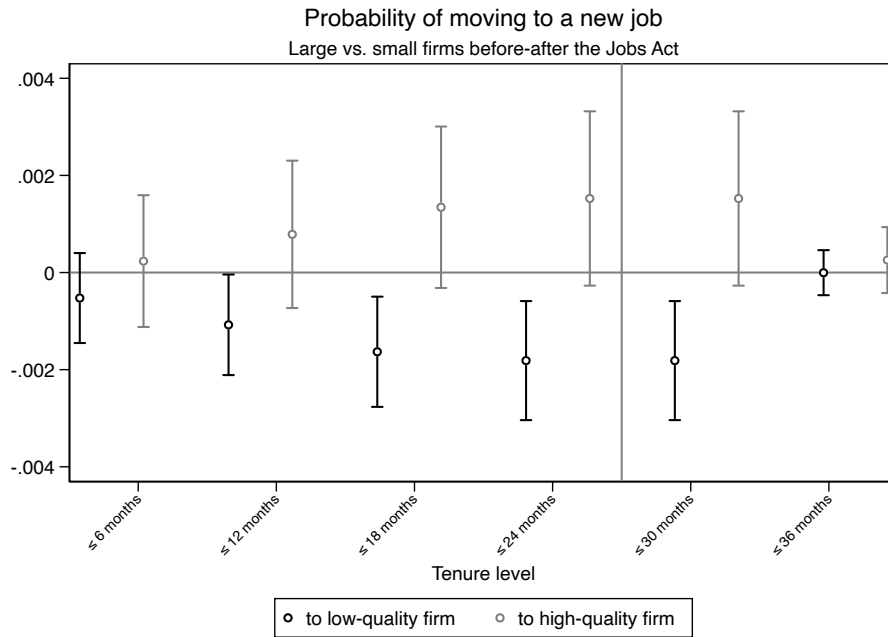
(b) Career progression

Notes: Panel **a** and **b** of the Figure report the difference-in-differences estimates available in Panel B and C of Table [A13](#), respectively. In Panel **a**, the dependent variable is the probability of moving to a higher paying job at month tenure m . In Panel **b**, the dependent variable is the probability of moving to a higher job position (from blue to white collar and from blue/white collar to manager).

Figure 4: Job-to-Job Mobility by Firm Quality



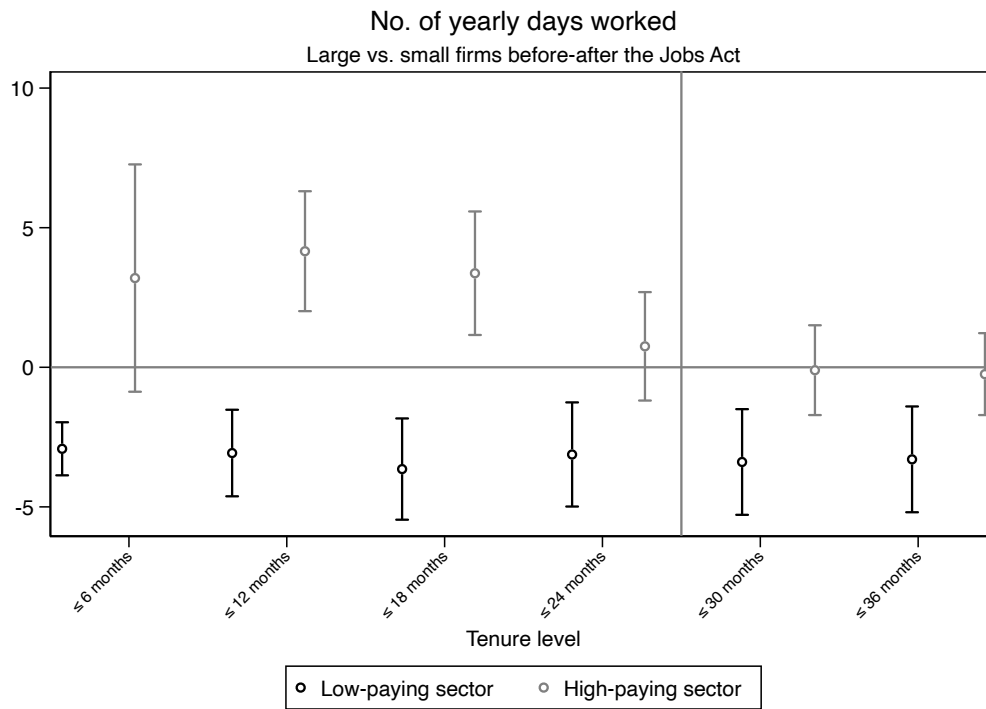
(a) Departing firm



(b) Destination firm

Notes: Panel **a** and **b** of the Figure report the difference-in-differences estimates available in Panel A and B of Table A15, respectively. The dependent variable is the probability of moving to a new job at month tenure m . High-quality firms are defined as those with above-median estimated firm effects $\hat{\phi}_j$, obtained through equation (2).

Figure 5: Worker Effort by Sector of Employment



Notes: The Figure reports the difference-in-differences estimates available in Table A16. 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.

Table 1: The Effect of the Jobs Act on Worker Tenure.

	Tenure (in years) (1)	Pr. tenure [0-2] (2)
Large Firm	0.193* (0.098)	-0.083* (0.046)
Jobs Act	-0.809*** (0.161)	0.334*** (0.069)
Large Firm*Jobs Act	-0.070 (0.229)	0.045 (0.104)
Observations	43356	43356

Notes: Each column reports estimates from OLS regression. The sample includes workers with up to 3 years of tenure. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

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

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

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
Large Firm	-0.032***	-0.009*
	(0.005)	(0.005)
Jobs Act	0.006	0.008
	(0.010)	(0.007)
Large Firm*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
<i>Chi</i> ²	5.80	
<i>p-value</i>	0.016	

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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The Effect of the Jobs Act on Fear of Job Loss. IV Estimates.

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
Second-stage:		
Large Firm*Jobs Act	0.051**	0.017
	(0.022)	(0.022)
First-stage for Large Firm:		
% Large-Firm Employees	0.009***	0.008***
	(0.002)	(0.002)
Kleibergen-Paap rk Wald F-statistic	26.583	11.838
Observations	23385	16247

Notes: Each column reports estimates from 2SLS 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. The instrumental variable, % of Large-Firm Employees, refers to the percentage 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. 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. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: The Effect of the Jobs Act on On-the-Job Search.

	Tenure level:			
	[0-2] (1)	(2-3) (2)	[0-2] (3)	(2-3) (4)
Large Firm*Jobs Act	0.012** (0.005)	-0.018*** (0.005)	0.024*** (0.008)	-0.007 (0.010)
Large Firm*Jobs Act*High-paying Sectors			-0.016 (0.010)	-0.018 (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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix: Additional tables and figures

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

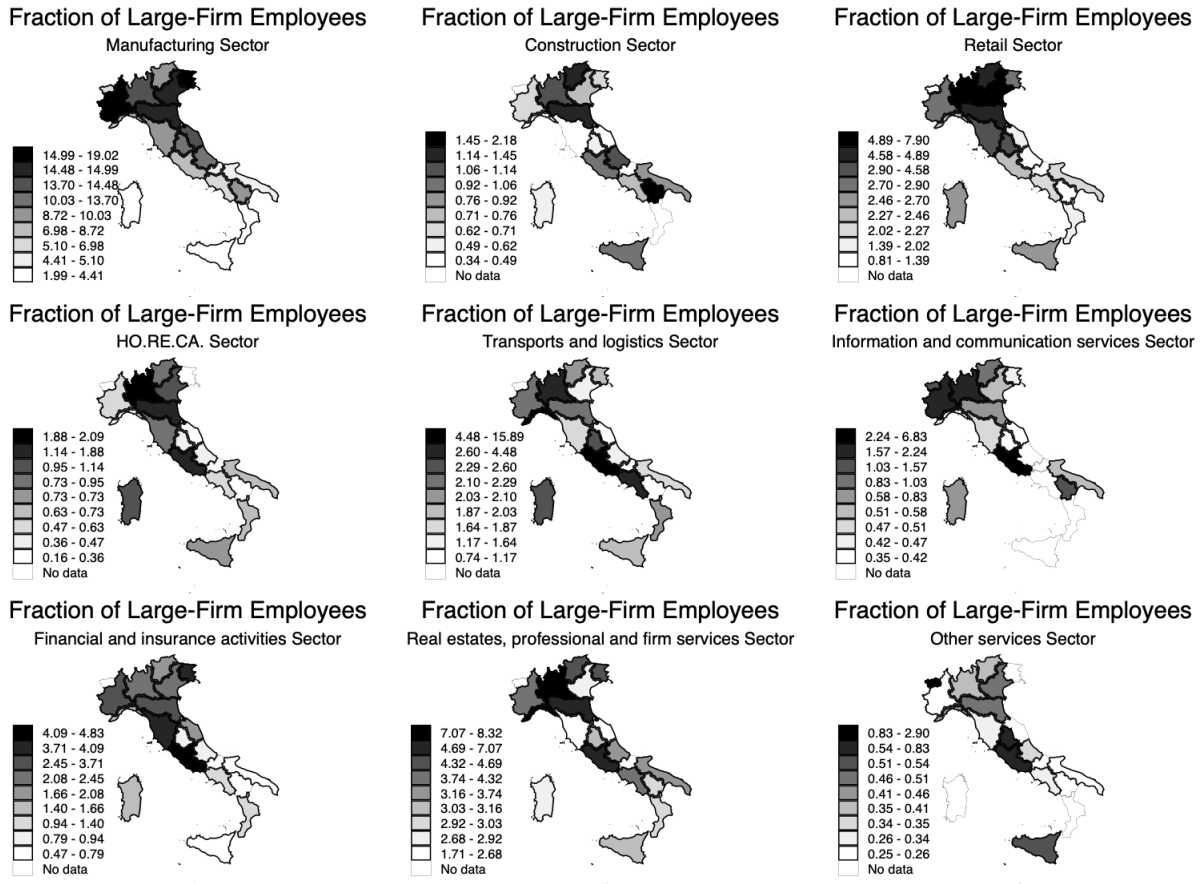


Table A1: Worker Tenure by Hiring Year and Interview Year in the Final Sample.

Hiring year:	Interview year:											
	2013		2014		2015		2016		2017		2018	
	min	max	min	max	min	max	min	max	min	max	min	max
2010	2.2	3										
2011	1.2	1.8	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 Firm*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 percentage 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 Firm*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 Firm*Jobs Act	2056475	0.372	0.483	0	1
White collar	2056475	0.342	0.474	0	1
From high quality firm	2056475	0.603	0.489	0.000	1.000
To high quality firm	25084	0.465	0.499	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 Firm*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. Selection Test

	(1)
Large Firm	-0.041*** (0.012)
Jobs Act	0.463*** (0.079)
Large Firm*Jobs Act	-0.037 (0.061)
Observations	20180
Mean Dep. Var. Large Firm=0, JA=0	0.215

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. The sample includes workers with 0-2 years of tenure reporting fear of job loss and workers with (2-3] years of tenure either reporting fear of job loss or not. The specification includes 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: The Effect of the Jobs Act on Fear of Job Loss. Placebo Hiring Dates.

	(1)	(2)	(3)	(4)	(5)	(6)
Large Firm	-0.027*	-0.027*	-0.027*	-0.028*	-0.029*	-0.029*
	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)
Fake Jobs Act 2013Q2	-0.043**					
	(0.017)					
Large Firm*Fake Jobs Act 2013Q2	-0.015					
	(0.023)					
Fake Jobs Act 2013Q3		-0.040*				
		(0.019)				
Large Firm*Fake Jobs Act 2013Q3		-0.025				
		(0.024)				
Fake Jobs Act 2013Q4			-0.039*			
			(0.022)			
Large Firm*Fake Jobs Act 2013Q4			-0.030			
			(0.028)			
Fake Jobs Act 2014Q1				-0.039*		
				(0.022)		
Large Firm*Fake Jobs Act 2014Q1				-0.027		
				(0.032)		
Fake Jobs Act 2014Q2					-0.039	
					(0.026)	
Large Firm*Fake Jobs Act 2014Q2					-0.029	
					(0.044)	
Fake Jobs Act 2014Q3						-0.027
						(0.028)
Large Firm*Fake Jobs Act 2014Q3						-0.066*
						(0.035)
Observations	16612	16612	16612	16612	16612	16612

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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: The Effect of the Jobs Act on Fear of Job Loss. Robustness Tests.

		Tenure level:	
		[0-2]	(2-3]
		(1)	(2)
A. No individual-level controls:	Large Firm*Jobs Act	0.022***	0.002
		(0.007)	(0.005)
	Observations	25596	17760
B. Imputation estimator (Borusyak et al., 2021):	Large Firm*Jobs Act	0.025***	0.003
		(0.008)	(0.005)
	Observations	25596	17760
C. Region specific trends:	Large Firm*Jobs Act	0.018**	0.000
		(0.007)	(0.005)
	Observations	25596	17760
D. No workers in firms with 10-19 employees:	Large Firm*Jobs Act	0.032***	-0.002
		(0.008)	(0.005)
	Observations	21372	14900
E. No workers in firms with 10-49 employees:	Large Firm*Jobs Act	0.031***	0.005
		(0.009)	(0.008)
	Observations	17523	11862
F. Same cohorts in the low-/high-tenure group:	Large Firm*Jobs Act	0.024***	-0.003
		(0.008)	(0.005)
	Observations	25024	14334

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. Unless otherwise stated, all specifications include Large and Jobs Act dummies and also 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: The Effect of the Jobs Act on Perceived Job Stability.

	Tenure level:	
	[0-2] (1)	(2-3] (2)
Large Firm*Jobs Act	-0.029*** (0.007)	0.005 (0.010)
Observations	25379	17620
Mean Dep. Var. Large Firm=0, JA=0	0.630	0.701

Notes: Each column reports estimates from OLS regression. The dependent variable, Perceived Job Stability, is a dummy taking value 1 for workers reporting satisfaction above-median satisfaction with job stability. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: The Effect of the Jobs Act on Fear of job loss. IV estimates - 2013 IV Definition.

	Tenure level:	
	[0-2] (1)	(2-3] (2)
Second-stage:		
Large Firm*Jobs Act	0.049** (0.024)	0.007 (0.021)
First-stage for Large Firm:		
% Large-Firm Employees	0.009*** (0.002)	0.008*** (0.002)
Kleibergen-Paap rk Wald F-statistic	24.838	11.394
Observations	23189	16115

Notes: Each column reports estimates from 2SLS 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. The instrumental variable, % of Large-Firm Employees, refers to the percentage of employees in large firms (50 or more employees) operating in 2013 in the same sector and in the same region in which the worker is currently employed. 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. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: The Effect of the Jobs Act on Fear of Job Loss. IV estimates - 2012 IV Definition.

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
Second-stage:		
Large Firm*Jobs Act	0.053** (0.022)	0.015 (0.021)
First-stage for Large Firm:		
% Large-Firm Employees	0.009*** (0.002)	0.009*** (0.002)
Kleibergen-Paap rk Wald F-statistic	28.282	13.228
Observations	23443	16279

Notes: Each column reports estimates from 2SLS 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. The instrumental variable, % of Large-Firm Employees, refers to the percentage of employees in large firms (50 or more employees) operating in 2012 in the same sector and in the same region in which the worker is currently employed. 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. Standard errors clustered at Large Firm*Year-Quarter level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: The Effect of the Jobs Act on Fear of job loss. IV estimates. Robustness Tests.

		Tenure level:	
		[0-2]	(2-3]
Added control:		(1)	(2)
A. No individual-level controls:	Large Firm*Jobs Act	0.069**	0.011
		(0.028)	(0.024)
	Observations	23385	16247
B. Region specific trends:	Large Firm*Jobs Act	0.034	0.008
		(0.022)	(0.023)
	Observations	23385	16247
C. No workers in firms with 10-19 employees:	Large Firm*Jobs Act	0.047**	0.015
		(0.021)	(0.019)
	Observations	19491	13647
D. No workers in firms with 10-49 employees:	Large Firm*Jobs Act	0.034	0.032
		(0.031)	(0.024)
	Observations	15840	10747
E. Same cohorts in the low-/high-tenure group:	Large Firm*Jobs Act	0.050**	0.017
		(0.023)	(0.022)
	Observations	22861	13135

Notes: Each column reports estimates from 2SLS 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. Unless otherwise stated, all specifications include Large and Jobs Act dummies and also 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: The Effect of the Jobs Act on Fear of job loss. Heterogeneous Effects.

	Tenure level:	
	[0-2] (1)	(2-3] (2)
Large Firm*Jobs Act	0.035*** (0.009)	0.013* (0.007)
Large Firm*Jobs Act*Woman	-0.031* (0.016)	-0.026 (0.016)
Large Firm*Jobs Act	0.025** (0.010)	-0.007 (0.007)
Large Firm*Jobs Act*Under35	-0.003 (0.013)	0.024** (0.010)
Large Firm*Jobs Act	0.038*** (0.013)	-0.177 (0.127)
Large Firm*Jobs Act*Permanent 1y before	-0.020 (0.016)	0.182 (0.129)
Large Firm*Jobs Act	0.019** (0.008)	0.004 (0.006)
Large Firm*Jobs Act*South	-0.002 (0.020)	-0.015 (0.029)
Large Firm*Jobs Act	0.027*** (0.008)	-0.002 (0.008)
Large Firm*Jobs Act*White Collar	-0.018 (0.011)	0.020 (0.018)
Large Firm*Jobs Act	0.026*** (0.009)	-0.013 (0.014)
Large Firm*Jobs Act*(Wage>median)	-0.012 (0.013)	0.028 (0.020)
Observations	28918	19954

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 Large Firm, Jobs Act and the relevant heterogeneity dummies as well as 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: The Effect of the Jobs Act on Continuation Rates

	Tenure level:					
	≤ 6 (1)	≤ 12 (2)	≤ 18 (3)	≤ 24 (4)	≤ 30 (5)	≤ 36 (6)
Large Firm*Jobs Act	-0.023*** (0.001)	-0.015*** (0.001)	-0.007*** (0.001)	-0.002*** (0.001)	-0.002** (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 A13: The Effect of the Jobs Act on Job-to-Job Mobility

	Tenure level:					
	≤ 6 (1)	≤ 12 (2)	≤ 18 (3)	≤ 24 (4)	≤ 30 (5)	≤ 36 (6)
Panel A: Any Mobility						
Large Firm*Jobs Act	0.0007*** (0.0001)	0.0010*** (0.0001)	0.0010*** (0.0002)	0.0011*** (0.0002)	0.0007*** (0.0002)	0.0001* (0.0001)
Observations	2086139	2086139	2086139	2086139	2086139	2086139
Mean Dep. Var.	0.0025	0.0038	0.0045	0.0051	0.0055	0.0056
Panel B: Mobility to a Higher Paying Job						
Large Firm*Jobs Act	0.0024 (0.0015)	0.0039** (0.0020)	0.0045** (0.0022)	0.0058** (0.0024)	0.0026 (0.0025)	0.0012 (0.0008)
Observations	63379	63379	63379	63379	63379	63379
Mean Dep. Var.	0.0136	0.0201	0.0233	0.0261	0.0274	0.0289
Panel C: Mobility to a Higher Job Position						
Large Firm*Jobs Act	0.0002 (0.0002)	0.0003 (0.0003)	0.0004 (0.0004)	0.0002 (0.0004)	-0.0000 (0.0004)	0.0002 (0.0002)
Observations	63379	63379	173712	63379	63379	63379
Mean Dep. Var.	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 . 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 A14: The Effect of the Jobs Act on Job-to-Job Mobility by Low- vs. High-Paying Sector

	Tenure level:					
	≤ 6 (1)	≤ 12 (2)	≤ 18 (3)	≤ 24 (4)	≤ 30 (5)	≤ 36 (6)
Large Firm*Jobs Act	0.0009*** (0.0001)	0.0011*** (0.0002)	0.0010*** (0.0002)	0.0010*** (0.0002)	0.0007*** (0.0002)	0.0001 (0.0001)
Large Firm*Jobs Acts*High-Paying Sector	-0.0007*** (0.0002)	-0.0006*** (0.0003)	-0.0002 (0.0003)	-0.0001 (0.0004)	-0.0003 (0.0004)	0.0000 (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 . 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 A15: The Effect of the Jobs Act on Job-to-Job Mobility by Firm Quality

	Tenure level:					
	≤ 6 (1)	≤ 12 (2)	≤ 18 (3)	≤ 24 (4)	≤ 30 (5)	≤ 36 (6)
Panel A: Departing Firm						
Large Firm*Jobs Act	0.0009*** (0.0002)	0.0012*** (0.0002)	0.0010*** (0.0002)	0.0010*** (0.0003)	0.0006** (0.0003)	0.0002** (0.0001)
Large Firm*Jobs Act*From High-Quality Firm	-0.0004* (0.0002)	-0.0005* (0.0003)	-0.0001 (0.0003)	0.0000 (0.0004)	-0.0001 (0.0004)	-0.0002 (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
Panel B: Destination Firm						
Large Firm*Jobs Act	-0.0006 (0.0004)	-0.0011** (0.0005)	-0.0016*** (0.0005)	-0.0018*** (0.0006)	-0.0018*** (0.0006)	-0.0000 (0.0002)
Large Firm*Jobs Acts*To High-Quality Firm	0.0003 (0.0006)	0.0008 (0.0007)	0.0014* (0.0007)	0.0015** (0.0008)	0.0015** (0.0008)	0.0003 (0.0003)
Observations	25084	25084	25084	25084	25084	25084
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 . All specifications include Large Firm and Jobs Act dummies, year-month fixed effects and worker fixed effects. (from (to)) high-quality firms is defined as whether the firm from which the worker is moving (is starting a new permanent contract) is above the median AKM firm effect. Standard errors clustered at the worker level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: The Effect of the Jobs Act on Number of Yearly Days Worked

	Tenure level:					
	≤ 6 (1)	≤ 12 (2)	≤ 18 (3)	≤ 24 (4)	≤ 30 (5)	≤ 36 (6)
Large Firm*Jobs Act	-2.9200*** (0.4850)	-3.0715*** (0.7909)	-3.6460*** (0.9256)	-3.1234*** (0.9515)	-3.3924*** (0.9656)	-3.2977*** (0.9668)
Large Firm*Jobs Act*High-Paying Sector	3.1950 (2.0766)	4.1579*** (1.0959)	3.3691*** (1.1286)	0.7504 (0.9906)	-0.1034 (0.8206)	-0.2454 (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 A17: The Effect of the Jobs Act on Other Outcomes.

	Tenure level:	
	[0-2]	(2-3]
	(1)	(2)
<i>Hours Worked:</i>		
Large Firm*Jobs Act	0.211**	-0.068
	(0.095)	(0.178)
Observations	25578	17743
Mean Dep. Var. Large Firm=0, JA=0	40.983	41.044
<i>Sick Leave:</i>		
Large Firm*Jobs Act	-0.001	0.005*
	(0.003)	(0.002)
Observations	25929	17917
Mean Dep. Var. Large Firm=0, JA=0	0.010	0.009
<i>Overtime Work:</i>		
Large Firm*Jobs Act	0.023***	-0.002
	(0.004)	(0.006)
Observations	25800	17835
Mean Dep. Var. Large Firm=0, JA=0	0.028	0.031
<i>Working During Weekends:</i>		
Large Firm*Jobs Act	0.011	0.023
	(0.008)	(0.015)
Observations	25874	17875
Mean Dep. Var. Large Firm=0, JA=0	0.470	0.492
<i>Monthly Earnings:</i>		
Large Firm*Jobs Act	24.476***	-4.212
	(7.018)	(7.508)
Observations	25929	17917
Mean Dep. Var. Large Firm=0, JA=0	1095	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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$