Universitat Autònoma de Barcelona

ESSAYS ON JOB MOBILITY

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A thesis submitted to the *Departament d'Economia i d'Història Econòmica* of the *Universitat Autònoma de Barcelona* in partial fulfillment of the requirements for the degree of Doctor of Philosophy granted by the International Doctorate in Economic Analysis (IDEA) in May 2019.

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To my mom and sister.

Acknowledgements

When something comes to an end, it is good to look backwards and evaluate the path that brought you to that exact moment. During this journey until the completion of my Ph.D. thesis, I have undoubtedly benefited from a growing number of people who have helped me, made me laugh, and, most importantly, believed in me. The following lines are just a tiny symbol of my deep gratitude to those people.

First and foremost, I would like to thank the most important person for me during all these years, my supervisor, Ana Rute Cardoso. I am extremely grateful for her unlimited support, invaluable guidance, and continuous encouragement. I will be forever indebted with her for believing in me, especially in those moments I barely did. Ana, thanks for helping me to become a better person. I hope one day I will be able to pay you back.

I am also grateful to Paulo Guimaraes for opening me the door of the Microdata Lab of the Bank of Portugal in Porto. Thank you for all the suggestions and advice, and for sharing with me your optimism and pragmatism. Paulo, I have learned so much from you! I would also like to thank Marta Silva, my co-author in the third chapter of this thesis, and the rest of experts at the BPlim, Emma, Gustavo, Joana, Miguel, and Rita, for all your help and support during my days in Porto. Thanks to the *Cidade Invicta* for being the turning point of my life and of this dissertation.

I am particularly indebted with Joan Llull for all the generous time devoted to equipping me with my econometrics' toolbox. Joan, thank you for all the input, advice, and useful conversations we had during all these years. I am also grateful to Hannes Mueller for all his insightful feedback on my research. I highlight Nezih Guner, Yuliya Kulikova, Javier Fernández-Blanco, Michael Creel, and Ada Ferrer-i-Carbonell because in one way or another they shaped my research interests in a critical way. Thank also to Inés Macho-Stadler and

David Pérez-Castrillo for all your insights during lunch time. A special thanks to Àngels López García and Mercè Vicente for all the help with the bureaucracy and receiving me with a smile anytime I needed something.

I thank Xulia González, Consuelo Pazó, Jaime Carrera-Alonso, and all my professors at the Universidade de Vigo for encouraging me to enroll in a PhD program.

My sincere thanks also to all my current and previous colleagues who shared with me this journey. To Alessandro Ruggieri for his selfless help and useful conversations over these years. To Jesús Sánchez Ibrahim for being my friend and being always there to support me. To the rest, Codruta, Annalisa, Rubén, Adrian, Elena, Alberto, Tania, Davide, Lavinia, Bartosz, Danisz, Anna, Sergi, thank you.

I gratefully acknowledge the financial support from Fundaciò Bancaria "La Caixa" through grant LCF/BQ/DE15/1036001 that made my Ph.D. work possible.

Last but not least, I would like to thank my mother and sister for all their caring, patience, and support that gave me the strength to reach this day. I am also grateful to my friends who were always there to make me laugh, especially in the darker days.

Abstract

This doctoral thesis studies how labor market institutions, social contacts, and firm-ownership schemes determine workers and firms job mobility decisions. The first chapter investigates the effect of severance pay on workers' voluntary mobility out of the firm. The analysis exploits a major labor market reform of the employment protection legislation in Spain in 2012 to show that a decrease in mobility costs due to a reduction in severance pay induced by the reform made workers who may expect to be displaced in the near future more likely to voluntarily leave their employer. The second chapter adds to the empirical literature on the role of social contacts on the labor market. The findings in this chapter indicate that employers are more likely to hire workers who have a connection in the firm through a former coworker, and that having a connection in the hiring firm improves re-employment outcomes of workers relative to non-connected workers. The last chapter studies differences between conventional and worker-owned firms in their adjustment to the business cycle. It documents that both types of firms respond to changing macroeconomic conditions by adjusting employment, hours of work, and wages. However, worker-owned firms exhibit greater employment resilience than conventional enterprises. Hours of work and wages, instead, adjust to a similar extent across the two types of firms.

Resumen

Esta tesis doctoral estudia cómo las instituciones del mercado laboral, las relaciones sociales en el mercado, o la estructura de la propiedad de las empresas afectan la movilidad laboral de trabajadores y empresas. El primer capítulo investiga el papel de la indemnización por despido en las decisiones de los trabajadores de abandonar voluntariamente su empresa. Utilizando la reforma laboral de 2012 en España, que modificó la legislación sobre la protección del empleo, el análisis muestra que la reducción en la indemnización de despido consecuencia de la reforma provocó que trabajadores con expectativas de que pueden ser despedidos en un futuro próximo están más dispuestos a abandonar la empresa de forma voluntaria. El segundo capítulo se centra en el papel de las relaciones sociales en forma de antiguos compañeros de trabajo en el mercado laboral. Los resultados indican que las empresas tienden a contratar con mayor probabilidad trabajadores que tienen algún ex-compañero de trabajo en la empresa, y que estos contactos mejoran las características de los puestos de trabajo obtenidos en relación aquellos trabajadores contratados que no tienen ningún contacto en la empresa. El último capítulo estudia diferencias en el ajuste a lo largo del ciclo económico entre empresas convencionales y empresas cuyos propietarios son sus trabajadores. En el capítulo se documenta que ambos tipos de empresas reaccionan a cambios en las condiciones macroeconómicas ajustando empleo, horas trabajadas, y salarios. Sin embargo, en empresas cuyos propietarios son sus trabajadores el empleo es menos volátil comparado con empresas convencionales. Por su parte, las horas trabajadas y los salarios exhiben un ajuste similar a lo largo del ciclo económico en los dos tipos de organizaciones.

Preface

Modern labor markets are characterized by a sizeable amount of flows of jobs and workers across different firms and labor market states. The mobility decisions made by the agents populating the labor market, along with the prevalent institutional setting, determine the extent of labor reallocation, which arises as a key force underlying productivity growth. This dissertation adds to the understanding of how labor market institutions, social contacts, or firm-ownership schemes impact workers and firms labor mobility decisions.

In the first chapter, entitled *Workers' Job Mobility in Response to Severance Pay Generosity*, I investigate the impact of employment protection legislation, or more precisely its severance pay component, on workers' voluntary mobility decisions out of their firm. The identification strategy exploits a major labor market reform in Spain in February 2012, which reduced severance pay entitlements for permanent contracts, together with the exposure of some workers to a mass layoff or a plant closure. I rely on rich administrative data to estimate a discrete time duration model with dynamic treatment effects. The results show that a decrease in mobility costs induced by a reduction in severance pay made workers who may expect to be displaced in the near future more likely to voluntarily leave their employer. In fact, I find that the job quit hazard rate for these workers increased by 13% after the policy change relative to workers who did not experience the layoff shock due to future employer closure. The results highlight that a policy targeting employers may have side effects on workers behavior, which should be taken into account in the design of labor market policies.

In the second chapter, co-authored with Marta Silva and entitled *Coworker Networks and the Labor Market Outcomes of Displaced Workers: Evidence from Portugal*, we investigate the role of personal connections on the labor market outcomes of displaced workers. We rely on rich administrative data, covering all private firms and their workers in Portugal, to define personal connections that arise from interactions in the workplace. Our research design exploits firm closures to identify workers who are exogenously displaced. This strategy allows us to use workers displaced by the same event as counterfactuals to study the impact of former coworkers networks on hiring probabilities and re-employment outcomes. Our analysis of hiring probabilities indicates that displaced workers with a direct link to a former coworker are more than two times more likely to be hired by a firm relative to workers displaced from the same closing event who lack such a tie. Analyzing re-employment outcomes, we show that displaced workers benefit from having a connection in the hiring firm. Namely, they have higher initial earnings and enjoy greater job security. The current analysis indicates that social contacts play a decisive role in the labor market, by reducing information asymmetries and helping to reduce the costs of job displacement.

In the third chapter, entitled *Employment Resilience in Worker-Owned Firms: Evidence from Spain*, I analyze differences between conventional and worker-owned firms in their adjustment to changing aggregate conditions. I rely on rich Spanish administrative data to study the evolution of employment, hours of work, and wages over the business cycle. The results show that both types of firms promote adjustments on all the three margins. However, employment of individuals hired by worker-owned firms is less sensitive to changing macroeconomic conditions than in capitalist enterprises. Wages and working-time of wage-earners, instead, are equally responsive across the two types of firms. The current findings can be rationalized by the presence of similar labor regulations and differences in the objectives of both types of organizations. Namely, both firms are constrained by regulations, such as collective bargaining, on the adjustments that can impose on wages and working hours. However, the social nature of worker-owned firms mitigates employment volatility in these organizations.

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Chapter 1

Workers' Job Mobility in Response to Severance Pay Generosity

1.1 Introduction

Labor mobility is the outcome of the decisions of workers to change jobs and the decisions of firms to adjust their workforce. Labor market institutions affect this process by influencing agents' behavior, as they alter the costs and benefits of such decisions. Employment protection legislation is considered one of the most relevant institutions hindering labor reallocation (OECD, 2004, 2013b) and, ultimately, productivity growth (Autor et al., 2007; Bartelsman et al., 2016).

Following the seminal work of Bentolila and Bertola (1990) and Lazear (1990), an extensive literature has explored the impact of employment protection legislation on the performance of labor markets. These studies have mostly focused on how this regulation distorts employer behavior. Theoretically, employment protection acts as a tax on firing, which has a direct effect on firms' firing and hiring decisions (Bertola, 1992; Blanchard and Portugal, 2001; Pries and Rogerson, 2005; Postel-Vinay and Turon, 2014). By directly increasing adjustment costs, employment protection leads firms to inefficiently continue employment relationships (Kugler and Pica, 2008; Garibaldi and Pacelli, 2008; Marinescu, 2009). Hiring decisions may also be affected as employers anticipate future costs associated with workforce adjustments (Kugler and Pica, 2008).

In practice, a key component of employment protection legislation raising firms' firing costs is severance pay, i.e. lump-sum transfers from the firm to the worker upon dismissal (OECD, 2013b). This suggests that employment protection may also alter workers' mobility decisions out of the firm. Indeed, severance payments increase the opportunity cost of job quit, thereby making workers more reluctant to voluntarily leave their employer to avoid losing the benefits they will be entitled to (Burdett, 1978; Mitchell, 1982). Yet, the effect of severance pay generosity on workers' decisions compounds labor supply and demand forces, so that its magnitude is an empirical question that has not been widely studied in the literature. In this chapter, I exploit a major labor market reform in Spain, which took place in February 2012, to study the link between employment protection and workers' quit decisions.

The Spanish 2012 labor market reform aimed to boost labor market dynamism and to reduce the existing gap in employment protection levels between open-ended and fixed-term contracts. The most relevant legislative change for the purposes of this analysis was the change in the employment protection legislation. In particular, the reform reduced monetary compensations for unfair dismissals of workers under permanent contracts, reshaped the definition of economic redundancies, and removed the requirement of administrative authorization for collective dismissals. Garcia-Perez and Domenech (2017) study the impact of the reform on unemployment inflows and outflows. Their results indicate that the reform increased the probability of exiting unemployment through a permanent contract as well as the transitions from temporary to permanent contracts. They also find that the reform decreased the probability of dismissal for workers on a temporary contract, whereas it had no effect on the layoff patterns of permanent workers. Overall, their findings suggest a mild but positive effect of the reform fostering labor market fluidity and dampening the widespread segmentation of the Spanish labor market.

The goal of this chapter is then to exploit the reduction in severance pay entitlements for permanent contracts induced by the 2012 reform to directly investigate the responsiveness of workers' mobility decisions to severance pay generosity. An important feature of the legislative change is that it affected all permanent contracts from the reform date onwards. Workers hired after the reform date are subject to this new regulation, whereas for those already employed severance pay will be computed in proportion to the time employed before and after the policy change. Note that this feature of the reform rules out the possibility of a static comparison of quit decisions of workers hired before and after the reform, as all are potentially affected by the reform. My research design exploits instead the fact that the reform took place at a fixed point in time, which creates exogenous variation in severance pay entitlements at a given realized duration in the job spell beyond tenure and wages. Specifically, consider two identical workers who have the same wage and tenure. However, that situation occurs at different moments in calendar time, given that they were hired at different dates, hence one is affected by the reform and the other is not. As a result, they will have different severance payment entitlements. This variation in entitlements is used to identify the effect of severance pay generosity on workers' mobility decisions.

Voluntary mobility is not only determined by severance pay, but by other factors like labor demand or worker endogenous labor supply decisions. For instance, in periods of economic growth, job creation rates are higher (Davis et al., 2012) and job offers are generally better (Schmieder and von Wachter, 2010). Similarly, workers may decide to voluntarily switch jobs in order to climb up the job ladder or to find a job that better matches personal or family circumstances. To disentangle the reform effect from confounding factors, my empirical strategy must compare two groups of workers who face similar determinants driving mobility decisions, but one of them is affected by the reform (treatment) and the other is not (control). In such a setting, a differential response in the quit rate for treated workers would be indicative of the reform effect. Unfortunately, the reform applied to all individuals and, hence, it is not possible to identify a group of workers who is not affected by the policy change. To overcome this limitation, I exploit mass-layoffs and plant closings to identify firm-specific conditions affecting workers' expectations of losing their job. This strategy allows me to find a group of workers who experience a (plausible) exogenous layoff shock and for whom severance pay is the unique mobility cost (in the form of foregone income). One could then expect a larger behavioral response from these workers relative to those who do not experience such a shock, as the reform directly decreased the economic incentives to wait to be laid-off for the former group of workers.

Using Spanish Social Security data, my empirical strategy consists of modeling the event history of individuals during their job spells up to job quit. In the evaluation framework, I combine the 2012 Spanish labor market reform together with the layoff shock to identify the effect of severance pay generosity on workers' quit decisions. These events are introduced as time-varying covariates in a discrete-time duration model, exploiting the availability of the exact dates of occurrence of the relevant events in the data. At those dates, the job quit hazard rate is allowed to change. The size of this change provides an estimate of the effect of the event. Treatment effects are identified by comparing the job quit hazard rate for individuals who are and are not affected by a given event at the same stage in their job spells (Abbring and van den Berg, 2003, 2004). The main effect of interest is then given by the incremental effect on the job quit hazard rate after the occurrence of the policy change for workers who face the layoff shock relative to such change for workers who do not experience the layoff shock.

My results point to a behavioral response of workers to severance pay generosity in the face of a layoff shock. A reduction in mobility costs induced by the severance pay reform made workers who may expect to be displaced in the near future more likely to voluntarily leave their employer. In fact, the job quit hazard rate for workers who experience a layoff shock relative to workers not affected by the shock increased by roughly 13 percent after the policy change.

The effect of the stringency of employment protection on workers' mobility decisions has direct policy implications. The results highlight that policies targeting firms may have also an impact on workers' behavior, which should be taken into account for the design of labor market policies that alter the economic incentives embedded in the employment relationship of firms and workers. Thus, the findings indicate that understanding the behavioral response of both agents that determine the extent of labor mobility is crucial to improve the design of policies aimed to foster labor market performance. Additionally, the change in workers' mobility decisions induced by the reform suggests that severance pay generosity may affect the incidence of public insured unemployment, as only laid-off workers qualify for unemployment benefits. This result stresses the relevance of taking into account employment protection in the design of unemployment insurance (Blanchard and Tirole, 2008).

Closely related to this study are Gielen and Tatsiramos (2012) and Kettemann, Kramarz and Zweimüller (2017). Gielen and Tatsiramos (2012) study the effect of employment protection on quit rates in a cross-country analysis for the period 1992-2001. They provide evidence that job quality is an important determinant of quits, but the strictness of the employment protection affects this relation. Their findings suggest that workers in countries with lower employment protection are more likely to quit from low quality jobs. However, their analysis does not provide any causal evidence on this effect due to the lack of exogenous variation across countries.

The analysis of Kettemann et al. (2017) builds upon a regression discontinuity design to analyze a prominent reform in Austria that took place in 2003. The reform abolished the severance pay system and introduced an occupational pension scheme. Under the severance pay system, only layoffs were subject to severance pay, whereas quits were not eligible. In the the new occupational pension system, both types of mobility keep their accumulated separation payments on an individual account that is transferred across employers. Authors find that workers subject to the new system were 60 percent more likely to make a job-to-job transition, compared to workers in the old system.

This study contributes to the previous literature along three dimensions. Firstly, contrary to Kettemann et al. (2017) who analyze the effect of removing the severance pay system on worker mobility, this study provides direct evidence on the role of the stringency of employment protection on workers' quit behavior by exploiting exogenous variation in severance pay generosity across workers. This is relevant from a political point of view as completely removing the current severance pay system may not be a feasible labor market reform to implement in many countries.

Secondly, the dynamic nature of my empirical strategy allows to control not only for business cycle effects and workforce composition changes, but also time variation within the employment relationship that is disregarded in static approaches. Specifically, my strategy takes into account idiosyncratic changes in the nature of the relationship between employer and employee that may arise as key determinants of the decisions of firms and workers to dissolve the match (Jovanovic, 1979). In other words, my empirical strategy accounts for dynamic selection at different job durations. The dynamic approach also permits to consider only job spells that started before the reform moment. This is important when analyzing a labor market reform that affects firms' hiring decisions, as the matching process between firms and workers may change as a consequence of the reform and, hence, affect the future development of the employment relationship.

Thirdly, the dataset used in the analysis explicitly states the objective reason for the separation, i.e. quit vs layoff, as it is compulsory to be reported by the employer to the Social Security administration to determine workers' entitlements to severance pay and unemployment benefits. Thus, the strategy overcomes potential concerns related to the assumptions made in previous literature about whether a job-to-job transition implies that workers will not collect severance pay upon separation.

The rest of the chapter is organized as follows: Section 1.2 explains the institutional setting, describing the labor market reform and the economic situation. Section 1.3 describes the empirical strategy used to identify the effect of interest. Section 1.4 provides a description of the data and summary statistics. Section 1.5 presents the estimation approach. Section 1.6 discusses the results and Section 1.7 concludes.

1.2 Institutional setting

1.2.1 Employment protection legislation before the reform

The Spanish labor market is characterized by a strong segmentation between open-ended (permanent) regular contracts and fixed-term (temporary) contracts.[1] This segmentation stems from large differences in employment protection legislation after the labor market reform of 1984 that liberalized the use of temporary contracts.

Since 1984, several labor market reforms have been carried out in order to make the labor market more dynamic and less segmented, thereby increasing productivity growth and reducing the incidence of temporary jobs.[2] Most of the legislative changes taken in the last thirty years aimed to reduce the employment protection gap between permanent and temporary contracts —

[1]Around 90 percent of the contracts signed each month are temporary, and about one-fourth of the labor force is employed under this type of contracts.

[2]Countries with relatively high employment protection are typically found to have lower hiring and separation rates and a slower resilience to output shocks. Moreover, evidence suggests that in these countries stringent regulations stifle the allocation of labor to the most productive uses, thereby hindering productivity and economic growth (Boeri and Garibaldi, 2007). Additionally, the coexistence of stringent employment protection on permanent contracts with relatively easy access to fixed-term contracts leads firms to react by substituting fixed-term for permanent contracts —due to the smaller cost involved in the termination of the employment relationship at the end of a fixed-term contrac—with negative effects on employment volatility (Bentolila et al., 2012), productivity growth (Dolado et al., 2016), or on-the-job training (Cabrales et al., 2014), in particular if the likelihood of contract conversion is small, as it is the case in Spain (Güell and Petrongolo, 2007).

either through short-lived hiring incentives for permanent contracts or penalties on the use of temporary contracts with the introduction of termination costs (Conde-Ruiz et al., 2011a,b). However, in the midst of a double dip recession, a major reform was undertaken in February 2012 which, for the first time, substantially changed the provisions of the Spanish employment protection legislation for permanent contracts.[3]

Before the 2012 labor market reform, employment protection for permanent contracts was among the most stringent in Europe, with job security rules and strong mandatory severance payments contributing to a rigid system (OECD, 2004, 2013b). The Spanish labor law distinguishes two types of employer-initiated separations that lead to different severance pay entitlements: "unfair" and "fair" dismissals. In the case of "unfair" dismissals, severance payments amounted to 45 days of wages per year of seniority with a maximum of 42 monthly wages. Dismissals due to objective reasons, or "fair" dismissals, are entitled to 20 days of wages per year of tenure with a maximum of 12 monthly wages. The reasons for the dismissal to be considered as fair were based either on firms' objective grounds such as, economic, technical, productive or organizational reasons; or employees' grounds such as incompetence or failure to adapt to the technical modifications at the workplace. Collective dismissals were also subject to the same severance pay as individual "fair" dismissals. To carry out this type of collective redundancies, employers must obtain administrative authorization and have the obligation of good-faith negotiations with unions before undertaking them.[4] Importantly, a worker fired for a "fair" reason could subsequently sue the firm and a legal process would begin. Between the layoff date and the final court ruling, employers must pay interim wages to the worker. In practice, the ambiguity

[3] Current labor legislation is contained in the Worker's Statute (*Estatuto de los Trabajadores*) of 1980, which has since been modified on several occasions with the 1984, 1994, 1997, 2001, 2006, 2010 and 2012 reforms.

[4] In firms with less than 50 employees, negotiations about the possibility to avoid the collective dismissal or reduce the amount of workers displaced cannot last more than 15 days after the employer's notification. In case of firms with 50 or more employees, this negotation period cannot last more than 30 days.

of the definition in the legislation of what "negative conditions" meant gave judges a great deal of discretion and, effectively, they re-ruled most dismissals that ended up in court as "unfair".[5] This led most employers to often opt for the fast-track dismissal procedure (*despido expres*), i.e. declaring a dismissal "unfair" even before a conciliatory procedure took place, paying upfront the corresponding severance payment and avoiding additional costs.

In turn, temporary contracts may be signed for a maximum period between six months and three years, depending on the specific type of contract. Costs at termination of the temporary contract were equal to 12 days of wages per year of seniority.[6] The most important element is that the use of temporary contracts is not linked to the principle of causality, so that they could be applied to any activity, temporary or not, boosting the use of this type of contracts by employers to fill permanent positions (Aguirregabiria and Alonso-Borrego, 2014).[7]

1.2.2 The 2012 policy change

The 2012 Spanish labor market reform was approved by the government on February 12th, 2012 as *Real Decreto Ley 3/2012* and confirmed by the Spanish Parliament with no substantial modifications in the *Ley 3/2012*. The most comprehensive policy change, and also the most relevant for the purposes of this analysis, was the reform of the employment protection system. The reform reduced monetary compensations for unfair dismissal, reshaped the definition of fair economic dismissal, and eliminated the requirement of administrative

[5] Jimeno et al. (2015) document that, during the 1984-2010 period, around 70 percent of dismissal cases resolved by Labor Courts' rulings were declared "unfair".

[6] In 2001, the Spanish government passed a law that introduced termination costs for all temporary contracts equal to 8 days of wages per year of seniority. The 2010 reform increased termination costs for temporary contracts gradually from 8 days of wages per year of seniority for contracts signed before 2012 up to 12 days for those contracts signed from 2014 onwards.

[7] In September 2016, the Court of Justice of the European Union sentenced (Case C-596/14 of the High Court of Madrid) the termination of temporary contracts should apply the same principles of the dismissals of permanent contracts. The ruling clearly overturned the Spanish labor legislation that denied temporary workers the same benefits and redundancy pay as permanent workers, and is currently giving rise to increased conflicts and litigation regarding the differential severance pay.

authorization for collective redundancies.

Monetary compensation for "unfair" dismissal was reduced to 33 daily wages per year of tenure, with a maximum of 24 monthly wages, compared to the previous severance pay of 45 daily wages per year of tenure with a maximum of 42 monthly wages. Noteworthy, this reduction in severance pay was not discontinuous, affecting instead all individuals employed at the moment of the reform. More precisely, whereas workers hired after the reform date are subject to this new regulation, those hired before the policy change are under a dual regime where the amount of severance pay is proportional to the length of the employment spell before and after the legislative change.

Severance pay for "fair" dismissals remained unchanged, but the reform clarified the conditions under which a dismissal can be justified by the employer due to economic circumstances. The new legislation specifies that "a dismissal is always justified if the level of revenue or sales, over three consecutive quarters, was lower than in the same quarters of the previous year". In addition, the employer does not have to prove anymore that the dismissal is necessary for the future profitability of the organization.[8] Loosely speaking, the reform limited the intervention of judges to the verification of the existence of the objective causes for dismissal and the compliance with the procedural rules. The reform also removed the worker's right to interim wages between the effective date of layoff and the final court ruling, removing the incentives for firms to opt for the fast-track dismissal procedure. Consequently, the redefinition of objective causes is likely to lead to a larger reduction in severance payments if it increased the likelihood that employers carry out economic redundancies.[9]

Regarding collective dismissals, the reform abolished the requirement of

[8]Case law seems to confirm that the *de jure* relaxation of the definition of fair economic dismissal also holds *de facto*. See for example the decision of the *Sala de lo Social del Tribunal Supremo* (STS 20-9-13, Rec. 11/2013) that specified that judges have to establish that the economic reasons alleged by the employer are truthful and serious, but must not assess whether the employer's decision is an appropriate managerial decision.

[9]Jimeno et al. (2015) find mild evidence on the increase in proportion of dismissals being ruled as fair by Labor Courts after the labor market reform.

administrative authorization for collective redundancies, while maintaining the obligation of good-faith negotiations with unions before providing individual notice, in line with the current legislation in most OECD countries. Furthermore, the new legislation states more precisely the objective reasons under which an employer can undertake a collective redundancy and establishes that the firm also has to develop a special training and relocation plan for those workers who have been laid off if the collective dismissal affects more than 50 workers. The reform has also enlarged the set of cases in which the employer must pay a tax if the collective dismissal involves workers aged 50 or older.

The policy change also modified other aspects of the Spanish labor market legislation that can influence worker mobility. On the one hand, a new permanent contract (*Contrato de Apoyo a Emprendedores*) for firms with less than 50 employees was introduced. This new contract includes several hiring incentives and fiscal rebates, and allows for a probationary period of one year.[10] On the other hand, the reform aimed to align labor costs more closely with firm idiosyncratic productivity by giving priority to agreements at firm level over the existing sector or province level. Employers can now opt-out more easily from a collective agreement and exploit internal flexibility measures as an alternative to dismissals in the presence of firm-specific shocks.[11]

Unfortunately, as the latter provisions took place at the same time as the severance pay reform, estimating the effect of each element separately is not feasible given that most of these changes affected all workers and firms in the same way. Thus, the estimated effect arises as a combination of all the provisions included in the labor market reform. Yet, one could expect that the

[10] The reform also extended the existing subsidy equivalent to 40% of ordinary severance pay (8 days per year of service, paid by a wage guarantee fund – FOGASA) to all cases of fair dismissal in the case of firms with less than 25 workers. Therefore, for these firms, firing costs are shared by the employer and the government. The subsidy was removed at the end of 2013.

[11] For instance, firms may unilaterally introduce changes in working conditions (wages, working hours, work schedules) whenever there are objective economic, technical, production or organizational arguments.

effect of the severance pay reduction is likely to be the main driving force behind any behavioral change in terms of *voluntary* mobility of workers exposed to the reform.

1.2.3 The Spanish labor market situation, 2005-2017

The period under analysis embeds a full economic cycle (see Table 1.1). One can clearly differentiate three sub-periods of economic growth and contraction. Between 2005 and 2007 GDP growth was on average 3.9 percent per year, then it collapsed with the onset of the Great Recession exhibiting a negative annual rate of 1.3 percent during 2008-2013; and it started to grow again from 2014 onwards with an average yearly growth rate of 2.8 percent.[12] The deterioration of the economic activity translated into massive employment losses during the recessionary period that led the unemployment rate to skyrocket from a minimum of 8.23 percent in 2007 to 26 percent in 2013. By the end of 2013, economic conditions started to improve, leading to continuous employment gains. The quit rate, defined as the percentage of worker initiated separations among all separations observed in a given year, exhibits a marked pro-cyclical behavior. Firm exit rate refers to the share of organizations going out of business over the total number of business active in a given year. The numbers show that firms' exit increased during the recessionary period, but it exhibits a fairly stable evolution over the period compared to GDP or employment. The observed variation in aggregate conditions over the period under analysis must be accounted for in the empirical strategy to properly identify the reform effect.

[12]The Spanish Business Cycle Dating Committee dated the first recession from the second quarter of 2008 to the fourth quarter of 2009, and the second recession from the fourth quarter of 2010 to the second quarter of 2013.

Table 1.1: Spanish labor market, 2005-2017

Year	GDP Growth	Unemp. Rate	Emp. Growth	Quit rate	Firm exit
2005	3.72	9.15	4.65	38.01	6.07
2006	4.17	8.45	4.98	41.52	5.73
2007	3.77	8.23	3.84	41.74	6.44
2008	1.13	11.25	-1.23	29.78	10.56
2009	-3.57	17.86	-6.80	18.37	8.94
2010	0.02	19.86	-1.97	18.53	9.33
2011	-1.00	21.39	-1.66	16.57	7.75
2012	-2.93	24.79	-4.51	15.00	8.86
2013	-1.70	26.10	-4.03	15.91	8.29
2014	1.38	24.44	1.59	22.10	7.35
2015	3.65	22.06	3.80	25.93	6.70
2016	3.17	19.64	3.64	28.04	7.81
2017	2.98	17.22	4.46	31.29	6.92

Sources: GDP growth and unemployment rate (*Instituto Nacional de Estadistica*). Employment growth and quit rate (*Ministerio de Trabajo, Migraciones y Seguridad Social*). Firm exit rate (*Directorio Central de Empresas*).

1.3 Empirical strategy

To estimate the effect of severance pay generosity on workers' quit decisions, the research design models the event history of individuals during their job spell. My empirical strategy consists of exploiting the availability of the exact date of the occurrence of the relevant events in the data. At those dates, the job exit probability is allowed to change, and the size of this change provides an estimate of the treatment effect of interest (Abbring and van den Berg, 2003, 2004).[13]

[13] A similar approach has been largely used in the literature on dynamic treatment evaluation of job training programs (Richardson and van den Berg, 2013), unemployment benefits sanctions (van der Klaauw and van Ours, 2013), or job creation schemes (Bergemann et al., 2017).

1.3.1 Evaluation framework

Events of interest. Two main events are considered to estimate the effect of severance pay generosity on workers' quit behavior. The first event of interest is the February 2012 Spanish labor market reform that cut severance pay entitlements for all existing permanent contracts from that moment onwards. The second event of interest is a (plausible) exogenous information shock to workers about individual job loss probabilities. The goal is to identify a group of workers who experience an exogenous increase in their likelihood of being dismissed and for whom severance pay is the unique mobility cost. The shock should hence trigger a decision from this group of workers on whether to leave voluntarily, or wait to be laid-off and collect severance pay. To define this shock, my empirical strategy exploits mass-layoffs and plant closings to identify firm-specific conditions affecting workers' expectations of losing their job.[14] In light of evidence indicating that individual's expectations of losing a job are predictive of subsequent job loss, and behavioral responses occur around a year before actual displacement (Stephens, 2004; Hendren, 2017), I assume a worker receives the information shock regarding the upcoming event if she is still employed at least 12 months before the event occurs.[15] In other words, the earliest moment that a worker may be aware of the increased layoff probability is one year before the large employment contraction occurs. Throughout the chapter, I label this layoff information shock as a *layoff shock*.

The layoff shock and the policy change are taken together to identify the role of severance pay generosity on workers' mobility decisions. Specifically, the empirical strategy combines both events to isolate the effect of severance pay generosity from common labor demand and supply factors that can drive

[14]There is a large body of research using this type of firm events as quasi-experiments where workers lose their jobs for exogenous reasons. Examples of outcomes analyzed are re-employment and earnings (Jacobson et al., 1993; Davis and von Wachter, 2011; Lachowska et al., 2018), fertility (Del Bono et al., 2015), role of intra-household insurance (Halla et al., 2018), spillover effects on local labor markets (Gathmann et al., 2018), or the use of firms' internal labor markets (Cestone et al., 2018).

[15]This threshold also helps to separate normal turnover from the turnover directly related to the firm event (Schwerdt, 2011).

quit decisions. The idea is that all workers should exhibit the same mobility response to common determinants. Yet, for workers who face the layoff shock severance pay represents the main opportunity cost of job quit.[16] Thus, lower severance pay entitlements induced by the policy change should generate higher probability of voluntarily leaving their employer in this group of workers, as the opportunity cost of job quit is lower.

Individual event history. Consider an individual starting a job at a given calendar time (t_0). The individual may experience the events considered at any time during her spell. Specifically, she may be affected by the 2012 reform (r) and/or an exogenous layoff shock (s). At the moment of job quit, the worker may be potentially in four different states (r, s, $r \times s$, or none of the above).

Denote T_r the job duration up to the reform date, T_s the duration up to the start of the layoff shock time window, and T the overall job duration until quit. These durations are random variables, and r, s, and t denote their realizations. Individual differences in the distribution of T are assumed to be summarized by explanatory variables X and V, where X denotes observed characteristics and V is the unobserved component. Thus, $T|s, r, X, V$ represents the job quit duration of an individual who may face a layoff shock and/or be affected by the reform. The distribution of job quit duration is in turn characterized by its hazard rate, $h(t|s, r, X, V)$.[17]

Model specification. The job quit duration process is modeled following a mixed proportional hazard rate (MPH) structure with dynamic treatment effects.[18] The transition probability is assumed to vary with observed and unb-

[16]For instance, firm-specific capital may represent another mobility cost. However, for workers who face the layoff shock, this cost vanishes, as it will be ultimately realized independently of the individual decision.

[17]Somewhat loosely, the hazard function is the rate at which the spell is completed at a given job duration, conditional on that it has not been completed before, as a function of time since job start.

[18]Abbring and van den Berg (2003) show that identification of these models is provided under an MPH structure and mild regularity conditions. Heckman and Navarro (2007) dis-

served characteristics as well as the elapsed job duration t. Additionally, the probability of voluntary leaving the job depends on treatment status at time t. The reduced-form specification for the job quit hazard rate at date t conditional on (s, r, X, V) is

$$h(t|s, r, X, V) = \lambda(t) \cdot \phi(X) \cdot V \cdot \exp(\tau R + \gamma S + \delta R \times S) \tag{1.1}$$

λ and ϕ are functions for the baseline hazard and observed characteristics, respectively. γ measures the impact of the incidence of the layoff shock ($t > s - t_0$) on the quit hazard rate. τ captures the shift in the quit hazard rate due to the occurrence of the reform ($t > r - t_0$). These two parameters are identified by comparing the change in the hazard rate of workers at same realized durations, with some being affected by the incidence of a given event and others not (yet).[19] δ measures the shift in the quit hazard rate due to the labor market reform on the mobility response of workers to the layoff shock relative to those workers who are not affected by the layoff shock. Accordingly, the effect is identified by comparing, at the same realized duration, the quit hazard rate of individuals for whom the layoff shock is realized with the quit hazard rate for workers who are not affected by the layoff shock, before and after the occurrence of the policy change.

Equation 1.1 has a mixed proportional specification except for the components related to the incidence of a given event. More precisely, observed characteristics and the unobserved heterogeneity term act multiplicative on the baseline hazard, whereas the effect of a given event works on the hazard rate from the moment the event is realized onwards. The dataset used in the analysis contains a large set of time-varying covariates. With variation over time in observed characteristics the proportional hazards assumption is not crucial for identification, as time-varying covariates act as exclusion restric-

cuss identification of dynamic discrete time models.

[19] Individuals in the comparison group may experience any of the events at a later stage in their spells or not be affected by them at all.

tions in the sense that their past values have an impact on the current transition probabilities only through the selection process (Brinch, 2007; Gaure et al., 2007). Identification of the effect of the incidence of the events considered requires some further assumptions on the treatment assignment process and the forward-looking behavior of individuals. These assumptions are discussed below.

1.3.2 Identifying assumptions

No selection of treatment assignment. Individuals are assumed to be affected by the policy change or the layoff shock randomly. Specifically, there is not endogenous selection of workers into any of the events considered. Treated individuals are then a random sub-sample of the overall sample of interest. This assumption implies that the distribution of the unobserved heterogeneity of treated individuals is equal to that distribution for the whole sample. Arguably, as the incidence of the events considered occurs beyond any control of individuals, those affected must be equal to those who do not experience the same event. There could be a second reason for which the treated individuals are not a random sub-sample: to observe the incidence of a specific event, it is necessary that the individual did not exit her job before. However, if the incidence of a specific event occurs randomly across individuals' job histories, differences in job exit rates that lead some individuals not to experience the event because they exit before their incidence should be accounted for by individual heterogeneity. The lack of selectivity rules out the need to model the duration until treatment and link this process with the job exit hazard rate through the unobserved heterogeneity distribution, as it is commonly done in the applications of this type of approach.[20] In sum, this assumption implies that treatment effects can be captured by exogenous time-

[20]For instance, in the analysis of unemployment benefit sanctions on unemployment duration, sanctions are not imposed randomly across individuals (e.g., van der Klaauw and van Ours, 2013). The endogeneity in the assignment process is accounted for by estimating the duration until the sanction is imposed and correlate the unobserved heterogeneity in this hazard rate with unobserved heterogeneity in the general unemployment exit rate.

varying covariates in an ordinary univariate duration model (van den Berg, 2001).

Random moment of treatment assignment. The moment when workers are affected by a specific event during their spells is random. Randomness in the moment of treatment assignment is necessary to distinguish the effect of a given event from the duration dependence in the exit rate.[21] In the case of the reform, this randomization is generated by the fact that the policy change affected all spells from February 2012 onwards. As workers start their jobs at different dates, the reform creates variation in the moment when workers are affected in their spells. This feature of the reform allows to separate the reform effect from duration dependence (van den Berg, Bozio and Costa-Dias, 2018) and wage levels.[22] The randomness in the moment the layoff shock is realized arises due to variation in the moment at which the layoff shock time period starts. Similar to the case of the reform, the fact that the layoff shock is realized at different elapsed job durations enables to separate the layoff shock effect from duration dependence. Independence of both of the events considered guarantees that the incidence of one event does not determine the incidence of the other (i.e. the distribution of one duration given the other is not degenerate).

No anticipation effects. A fundamental assumption for identification of dynamic treatment effects is that future entry into treatment does not have an effect on the job quit rate prior to the realization of the treatment, i.e. there is no anticipation about the future occurrence of the event (Abbring and van den Berg, 2003). Conceptually, the assumption implies that the hazard paths coincide for two (potential) counterfactuals up to the occurrence of the treatment,

[21]Note that spells starting after a given event is realized are assigned to treatment from moment zero. Thus, the estimation only considers spells starting before the event moment to guarantee that the moment of treatment assignment is random.

[22]Two individuals with the same tenure and wage may have different severance pay entitlements based on their exposure to the reform. In other words, depending on the relative time employed after the legislative change relative to overall time employed.

conditional on observables and unobservables. Intuitively, the condition implies that individuals do not have private information on the moment when treatment starts (or that they do not act on such information). In other words, the no-anticipation assumption does not exclude that individuals know the probability distribution of future events conditional on observable and unobservable characteristics. In my setting, anticipation of the policy change is unlikely to occur, as the reduction in severance pay for permanent contracts was unanticipated. Anticipatory effects regarding mass-layoffs or plant closings could be a concern: workers may anticipate the upcoming event and adapt their behavior accordingly, e.g. reduce/increase on-the-job search, which may have an impact on the individual job quit hazard rate. My empirical design uses the 12 months prior to the employer event to mitigate concerns regarding the anticipation of a worker's own job loss (Hendren, 2017). Identification of the main effect of interest relies thus on the assumption of no pre-treatment effects, i.e. in the absence of the reform the differences in the job quit hazard rate between workers who are and are not affected by the layoff shock would have remained constant.

1.4 Data, employer events, and estimation sample

The main data source is the Spanish Continuous Sample of Employment Histories (*Muestra Continua de Vidas Laborales* or MCVL), an administrative dataset collected annually by the Spanish Social Security administration since 2005 up to 2017. The MCVL is a representative 4 percent random sample of individuals who had any relationship with the Social Security system at any time in the reference year.[23] Each MCVL wave is typically extracted in March/April of the year following the reference period.

The MCVL has a longitudinal design, since an individual present in a year

[23]This includes employed and self-employed workers, recipients of unemployment benefits and pension earners, but excludes individuals registered only as medical care recipients, or those with a different social assistance system (civil servants, such as the armed forces or the judicial power).

who subsequently remains registered with the Social Security administration stays as a sample member. Additionally, since the dataset is refreshed each year, it remains representative of both the stock and flow of individuals in the Social Security system.[24]

For each sample member, the MCVL retrieves all relationships with the Social Security system since the date of the first job spell, or 1967 for earlier entrants.[25] All the spells are followed from their start up to their end or to the 31st of the December of 2017. Importantly, the MCVL provides precise information on the reason of termination of each labor relationship. This information allows to differentiate between employer and employee initiated separations, i.e. layoff and quits, respectively. Worker, job, and employer characteristics are also observed for each employment spell.[26]

1.4.1 Plant closures, mass-layoffs, and layoff shock

The MCVL includes longitudinal records of the employers of the randomly selected workers. Two levels of employer identifiers are observed: plant and firm. The plant identifier refers to the Social Security contribution account.[27] The second identifier is based on the tax ID and is common to all plants within a firm. This study considers as the unit of analysis the plant since employer information is observed at that level of disaggregation.[28] In particular, for each of the plants in the MCVL, plant-size is observed annually at the data

[24]Individuals who stop working remain in the sample while they receive unemployment benefits or other welfare benefits (e.g. retirement pension). Then, individuals leave the sample when they pass away or leave the country permanently. Likewise, each wave adds individuals who enter the labor market for the first time.

[25]Since 1980 including information on earnings.

[26]Appendix 1.A provides a detailed description of the variables.

[27]According to the Social Security administration, around 85 percent of the firms are single unit organizations, i.e. there have just one contribution account per firm. Each firm has one account for each treble province-Social Security regime-type of employment relation. Thus, the Social Security Administration identifies within a province different groups of employees of a given firm. By restricting the sample to standard labor relationships (e.g. traditional wage-employment workers) and the General Regime of the Social Security (e.g. no peculiarities in welfare entitlements)as will be described in Section 1.4.2, contribution accounts can be thought of as establishments.

[28]Throughout the analysis employer, firm and plant will be used indistinctly.

extraction moment, y.[29] Then, I organize the plant records in a yearly panel to exploit plant-size changes between two consecutive years to identify plant closures and mass-layoffs.[30]

Plant closure. To be coded as a plant closing in my analysis, the employer must meet the following criteria: (i) plant-size is equal or larger than 5 in y, (ii) employment collapses to zero between y and $y + 1$, and (iii) plant-size is also zero in $y + 2$. Condition (iii) prevents the inclusion of temporary inactivity periods of the Social Security account. To further minimize the inclusion of "fake" plant deaths, I refine the closure definition by looking at workers' job spells. A closing plant is recoded as non-closing if there are jobs still active after the moment when plant size is observed to be zero. Likewise, I redefine the employer as non-closing if the reason for the end of a worker's job spell is associated with an employer's merge.[31] The closure month is then defined as the first month of the closing year.

Mass-layoff. Mass-layoff events are defined following the Spanish collective dismissals regulation.[32] To fall within the scope of this legislation, employers must plan to dismiss or make redundant between at least 10 and 30 employees within a period of 90 days. The minimum thresholds for collective dismissals depend on the size of the company: at least 10 employees in plants with fewer than 100 employees; 10% of the workforce in firms employing between 100 and 299 employees, and 30 employees in companies employing 300 or more employees.[33] Thus, an employer experiences a mass-layoff event if: (i) plant-

[29] If the plant is no longer in operation, the observed plant-size is zero.

[30] There are some cases in which plant size observations exhibit holes. Specifically, I observe the plant in $y - 1$ and $y + 1$, but not in y. In such cases, I recover plant size at y, by linearly interpolating employment stock between $y - 1$ and $y + 1$. In addition, I recover plant-size for 2005 from the 2004 file.

[31] To minimize the impact of this reclassification, recoded plants are left out from the analysis (roughly 2.31 percent of all plant closings).

[32] In a sensitivity analysis (Table 1.B2) I use alternative definitions of mass layoffs based on employment contractions typically used in the job displacement literature.

[33] A dismissal of the entire workforce also falls under the collective dismissal rules if more than 5 employees are affected and this redundancy leads to the shut down of the business.

size is larger than 10 in y, (ii) the employment contraction between y and $y+1$ falls within the threshold determined by the collective dismissal regulation, (iii) plant-size in y is not more than 110 percent of its level in $y-1$, and (iv) plant-size in $y+2$ is at most 90 percent of plant-size in y. Condition (iii) and (iv) rule out temporary fluctuations in plant-level employment.[34] Similarly to closing plants, I filter the mass-layoff definition by looking at job spells. In particular, I exclude from the mass-layoff definition those organizations that create 10 or more jobs during the mass-layoff year.[35] The mass-layoff month corresponds to the first month of the mass-layoff year.

Layoff shock. The layoff shock time window is considered to start one year before the firm event period begins. Hence, the firm event might take place any time between the 13th to the 24th month after the layoff shock time period starts.[36] My research design then assumes that a worker becomes aware of the upcoming employer event at any moment after the onset of the layoff shock period. Namely, the earliest moment that a worker may perceive the increase in her individual layoff probability is one year before the large employment drop occurs.

1.4.2 Estimation sample and descriptive characteristics

Sample restrictions

The starting sample includes the continuous history of job spells of workers observed in the MCVL born after 1950 whose first job spell in the data is

[34] In the Spanish context it is important to minimize the impact of these fluctuations, especially in large employers, given the widespread use of temporary contracts. As the plant-size refers to the total number of employees in the plant at a fixed point in time, these conditions seek to minimize variations in plant-size driven by the amount of fixed-term contract workers.

[35] This implies a re-coding of about 4.87 percent of plants which exhibit a mass-layoff.

[36] There are some cases when a mass-layoff event occurs immediately before the closure event. In such situations, events are treated as a unique event and, hence, the event month corresponds to the month when the first event takes place.Note that the time-length between the beginning of the layoff shock period and the end of the employer event year in those cases is longer than 24 months.

observed at age 16.[37] From this sample, I exclude job spells with inconsistencies in relevant information.[38] This starting sample comprises 992,250 workers observed over 7,465,393 job spells. The following constraints are imposed to select the analysis sample.[39]

Job spells linked to the public sector are removed from the sample as labor relationships clearly differ from those in the private sector (635,602). Job spells in special regimes of the Social Security that typically cover the primary sector and household activities are also excluded, as these regimes exhibit distinctive labor regulation in terms of welfare entitlements (954,010).[40] Likewise, I discard job spells with any peculiarity in terms of the labor relationship between employer and employee, such as workers provided by temporary work agencies (663,683). The last two constraints also ensure that contribution accounts identify establishments of a given firm in a province. These restrictions reduce the sample to 883,971 workers observed over 5,212,098 job spells. These restrictions reduce the sample to 883,971 workers observed over 5,212,098 job spells.

The analysis focuses on all job starters between 2005 and 2017 in plants with at least two years of positive plant-size (2,265,702 spells removed). At the moment of separation, workers must be (i) age 50 or younger, and (ii) hold a permanent contract with more than six months of tenure (2,501,612 spells dropped). Condition (i) prevents the influence of early/partial retire-

[37]The minimum legal age to work in Spain is 16 years old and the MCVL provides information on job spells back to 1967. Thus, I restrict to workers born after 1950 to be able to track their complete labor market careers up to the moment of each job start. Additionally, I exclude workers whose first job spell is observed before being 16 years old. These constraints excludes 1,772,709 job spells from the original sample.

[38]3,678,958 of job spells were discarded. The largest share of the spells removed are those that lack information on the type of contract (51 percent). Before 1991 was not compulsory to fill this information, and only after 1997 is fully reliable. The other relevant variables with missing information are: re-coded plant events (30 percent) entry wages (6 percent), education (5 percent), reason for termination (3 percent), , occupation group (2 percent), and plant creation and location (2 percent), occupation group (1 percent).

[39]Table 1.C1 in the appendix presents an overview of the effect of each of the constraints on relevant variables.

[40]Remaining jobs in agriculture, fishing, mining and other extraction industries as well as household employees activities are also excluded.

ment schemes on mobility decisions. Condition (ii) guarantees that workers exceed the probationary period and are affected by the reform. These constraints yield a sample of 311,582 workers observed over 444,784 spells.

Finally, only workers who already qualify for severance pay the moment they are affected by a given event are considered in the analysis. This implies that the estimation sample only includes job starters between January 2005 and July 2011 (184,210 job spells dropped). In the case of workers in closing or mass-layoff plants, only those hired at least 6 months before the start of the layoff shock period are included (17,599 spells deleted). These two restrictions ensure the randomness in the occurrence of the events across job spells. The estimation sample consists of 201,932 workers observed over 242,975 job spells.[41]

Events, job separations, and sample characteristics

Events. Table 1.2 provides the proportion of spells that are affected by the incidence of a given event. The first column gives the fraction of those spells in the whole sample while subsequent columns report information for specific subgroups. Around 16 percent of the job spells in the estimation sample are affected by the layoff shock, and roughly 43 percent of the job spells cross the reform moment. After the policy change, about 8 percent of the job spells experience the layoff shock.

Table 1.2: Fraction of spells affected by the layoff shock and/or the reform

	Full sample	Female	Age>30	College	Services	High-Unemp.
Layoff shock	0.165	0.150	0.176	0.187	0.149	0.154
Reform	0.433	0.440	0.451	0.491	0.433	0.455
if layoff shock	0.078	0.074	0.085	0.099	0.076	0.076

Notes: Layoff shock applies to workers still employed 12 months before the start of the firm event year. Reform classifies spells that are affected by the reform (February, 2012), i.e. they have at least one month under the new policy regime. High-Unemp identifies workers who start a job in a province with the unemployment rate above the median of the national unemployment rate in the quarter of hiring.

[41]Only 10 percent start more than one job spell during the hiring period considered.

Job separations. Table 1.3 presents the share of spells finished due to a quit (worker voluntary separation) as well as those right-censored. Column 1 looks at the overall sample and successive columns focus on selected subgroups. Job spells are followed up to job termination or the end of the observation period (December 2017). The spell ends if the employment relationship is dissolved, in which case the realization of the duration variable of interest is observed. The job separation might be either employer- or employee-initiated. The main outcome variable for the analysis are workers' voluntary separations (quits), which is the reason of termination for around 33 percent of the spells in the sample. The duration is right-censored due to the end of the observation window for around 20 percent of the spells.[42]

Table 1.3: Fraction of spells finished and right-censored

	All workers	Female	Age>30	College	Services	High-Unemp.
Quit	0.328	0.311	0.277	0.391	0.350	0.272
Right-censored	0.197	0.198	0.205	0.235	0.188	0.204

Notes: Right-censored stands for spells that do not finsih within the observation period. High-Unemp identifies workers who start a job in a province with the unemployment rate above the median of the national unemployment rate in the quarter of hiring.

Job quit duration. Table 1.4 presents the mean and some selected quantiles of relevant job quit durations. There is significant variation in the realized duration at which workers enter the layoff shock window, with an average of almost 27 months. There is also variation in the moment at which workers experience the policy change, with an average of 34 months after job start. The difference between the incidence of these two events also exhibits some variation, with an average distance of roughly 13 months between the realization of both events. The ample variation observed indicates that the assumption of randomness in the moment of treatment assignment is satisfied in the data. This variation allows to isolate the effect of the event of interest from duration dependence. Notice also that spells for which the incidence of a given event

[42]Note that the relatively low number of right-censored observations is driven by the fact that most job spells affected by the mass-layoff or plant closing events are observed to end within the observation period.

is observed are consistently longer than spells not affected, as it takes time before the event is realized.

Table 1.4: Distribution of job quit duration (in months)

	Mean	Q10	Q25	Q50	Q75	Q90
S if layoff shock	27.2	10	13	20	35	56
R if reform	33.6	11	16	28	49	66
$S - R$	13.3	1	1	15	27	38
Y	28.7	9	13	20	35	62
No layoff shock						
Y if no reform	19.7	8	11	16	25	36
Y if reform	60.4	22	36	57	81	103
$Y - R$	28.1	4	10	25	44	59
Layoff shock						
Y if no reform	30.6	15	20	28	38	50
Y if reform	65.2	29	42	62	85	106
$Y - R$	26.1	4	10	23	40	54

Notes: Layoff shock applies to workers still employed 12 months before the start of the firm event year. The reform moment is February 2012. S denotes the observed duration up to the moment the layoff shock time window starts. R refers to the realized duration up to the reform. $S - R$ describes the difference between the realization of the layoff shock and the reform. Y stands for the observed job quit duration. $Y - R$ measures the time in employment after the policy change.

Characteristics. Table 1.5 presents summary statistics of observed characteristics for the whole sample and for workers who separately experience a given event.[43] Characteristics are measured at job start. Around 47 of the workers are women. A large share of the sample is formed by Spanish workers (85 percent), and almost 20 percent of the workers hold a university degree. Workers are on average 31 years old at job entry. These workers were, on average, employed 56 percent of the time since they entered the labor market. Regarding job characteristics, around 78 percent start a full-time job and roughly 14 percent start their job in a high-skill occupation. Average entry-level real daily

[43]Table 1.C2, 1.C3, and 1.C4 provide further summary statistics for individuals affected differently by the events of interest.

wage is around 46 euros. Hiring establishments are mostly in the service sectors (75 percent), and 42 percent are located in the four biggest cities in Spain.[44] These firms have been on average for 8 years in business and have around 27 employees.

There are some interesting differences between workers who are affected by the layoff shock and those who are not. In particular, workers who experience the layoff shock are more likely to be male, older, and more educated. They also start jobs with higher skill requirements and, consequently, earn higher wages. Moreover, their employers are on average larger and typically found in the manufacturing sector. There is also some heterogeneity between workers who survive until the reform moment and those who exit their job earlier. Workers who stay in their job until the reform moment are on average more educated, they start their jobs in high-skill occupations with higher entry-level daily wages. In terms of their employers, they are more likely to be larger and more mature organizations from the service sector.

[44]Madrid, Barcelona, Sevilla, and Valencia represent the four metropolitan areas with more than 1 million inhabitants.

Table 1.5: Observed characteristics at spell start

	All	No layoff shock	Layoff shock	No reform	Reform
Worker-level variables					
Female	0.471	0.479	0.424	0.465	0.475
Age	30.85	30.79	31.53	30.85	31.14
Spanish	0.846	0.842	0.875	0.807	0.900
College	0.197	0.194	0.231	0.179	0.235
Employment history	0.561	0.558	0.619	0.543	0.619
Non-employment	0.561	0.574	0.453	0.584	0.506
Job-level variables					
Full-time job	0.784	0.770	0.855	0.778	0.793
High-skill	0.141	0.134	0.177	0.124	0.163
Real daily wage	45.92	44.71	52.06	43.35	49.30
Employer-level variables					
Services	0.749	0.766	0.653	0.744	0.764
Biggest 4 cities	0.423	0.421	0.471	0.443	0.414
Plant Age	8.26	8.17	9.65	7.80	9.69
Size	27.48	25.12	68.13	28.21	42.02
No. spells	242,975	202,829	40,146	137,881	105,094
No. workers	201,932	172,185	38,795	119,168	105,094
No. plants	158,253	140,583	25,135	103,891	70,727

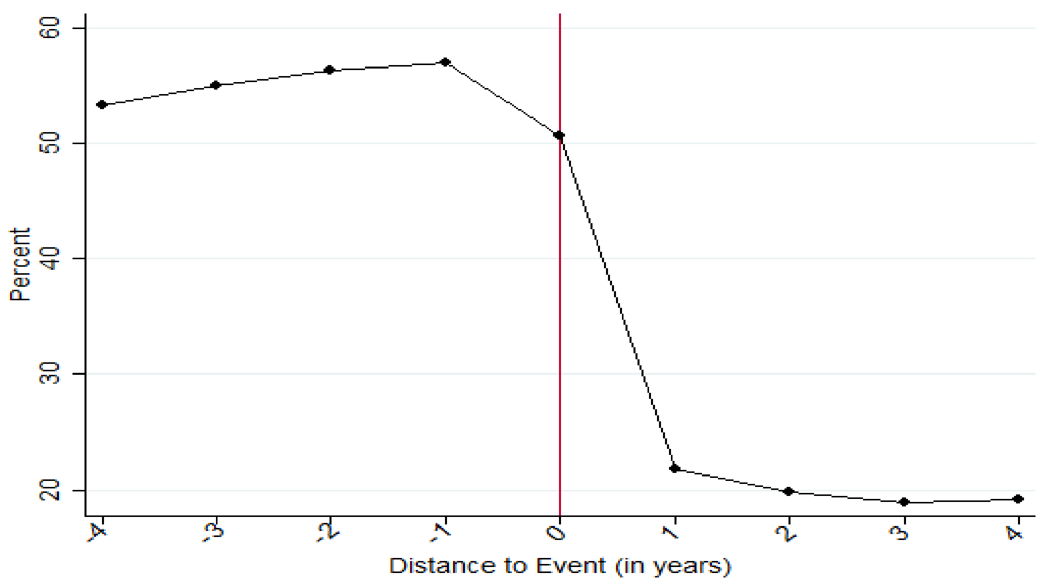
Notes: Worker characteristics summarized across worker observations. Job characteristics are averaged across job spells. Employer characteristics computed at the plant-level. Layoff shock applies to workers still employed 12 months before the firm was last observed in operation. Employment history refers to the share of time that a worker was employed since labor market entry. Non-employment identifies workers coming from non-employment at job start. Wages are deflated using the 2017 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Evolution plant-size around event moment. Figure 1.1 shows the evolution of plant-size 5 years around the event start moment.[45] The figure reveals two interesting facts. Firstly, plant-size continuously grows up to one year prior the start of the event, i.e. when the large employment contraction occurs. Secondly, the year before the start of the event, plant-size begins to shrink and after the event firms keep, on average, downsizing up to around the third year after the event. This evidence suggests that the 12 months before the start of the event year is an sufficiently long time span to be capturing potential

[45]Figure 1.C3 and 1.C4 show the result separately for mass-layoff and closing plants respectively.

information flows about the current situation of the firm and the possibility that redundancies at the plant will be needed.

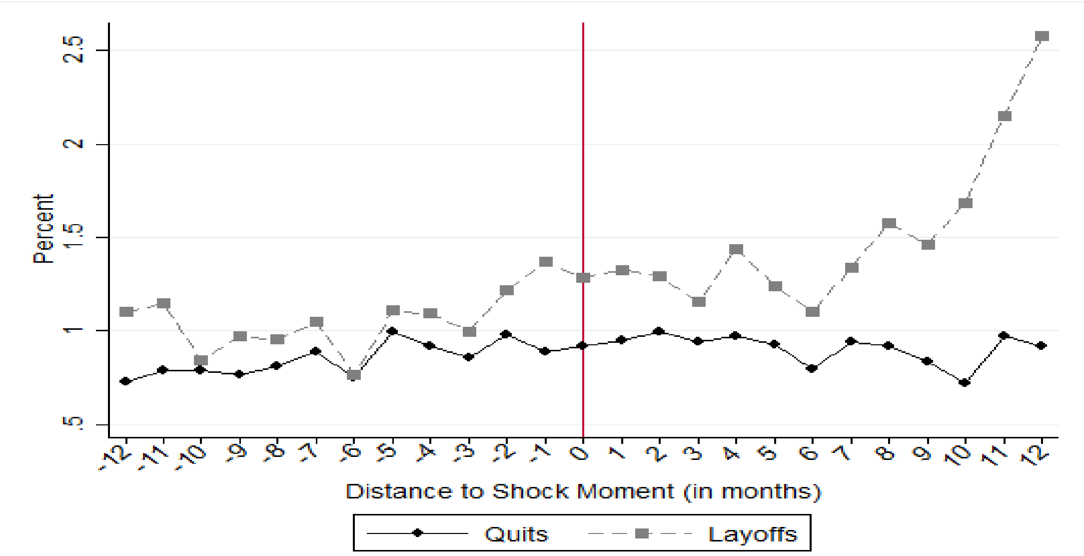
Figure 1.1: Evolution of plant-size around event moment



Notes: The figure depicts the evolution of plant size around the start of the event year. The vertical line refers to start of the event year, i.e. the year when the large employment contraction occurs.

Separation flows around layoff shock month. Figure 1.2 displays job quit and layoff flows around the layoff shock moment. The flows are computed as the numbers of quits or layoffs observed at each distance (monthly frequency) to the layoff shock moment divided by the total number of workers observed at that distance. The figure reveals that before the layoff shock month considered, separation flows seem to have a stable evolution. After the information shock moment, layoffs start to rapidly increase as the start of the firm event year approaches. In turn, quit flows seem to have a much more stable behavior. In sum, the evolution of quits and layoffs seem to vary smoothly around the layoff shock month considered. This is suggestive evidence of no anticipation effects before the benchmark month assumed.

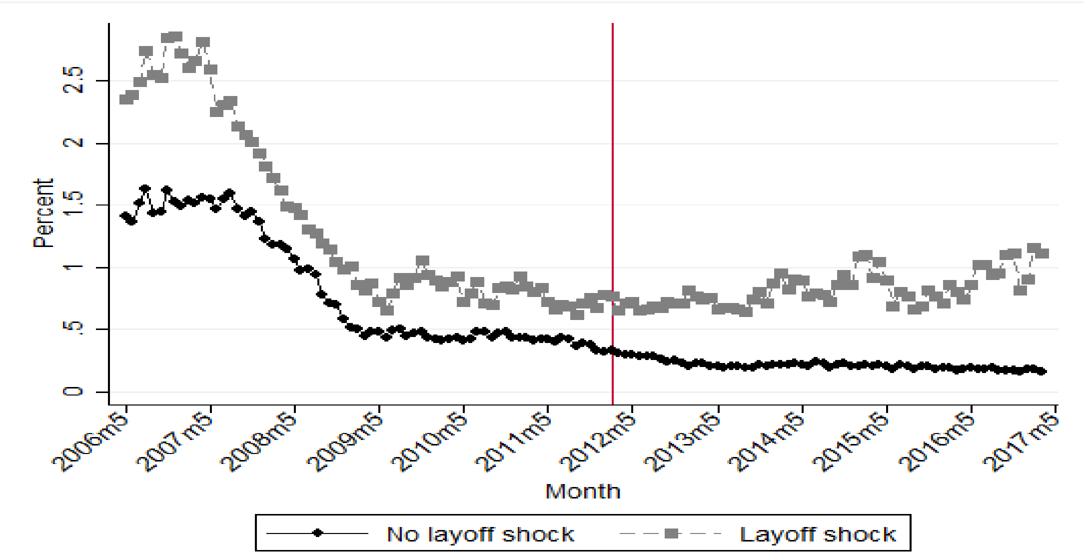
Figure 1.2: Separation flows around layoff shock month



Notes: The figure displays the percentage of workers who separate either voluntarily (quits) or involuntarily (layoffs) from their employer by the distance to the layoff shock moment considered. The vertical line refers to the layoff shock month assumed, i.e. 12 months before the year when the large employment contraction occurs.

Monthly quit flows. Figure 1.3 shows job quit flows at a monthly frequency for no layoff shock and layoff shock groups. The monthly quit flow is the number of quits observed each month divided by the total number of workers observed that month. The time series shows that quits decreased for both groups of workers as aggregate conditions started to worsen after 2008, though the share of quits is larger for workers affected by the layoff shock. The figure suggests that there is no anticipation of the reform moment by any of the two groups of workers. Additionally, the evolution of quits seems to follow the same path for both groups before the occurrence of the policy change (red vertical line), suggesting the absence of pre-treatment effects. Around the reform date, the paths seem to diverge. However, these rates do not account for any of the observed composition difference between workers in the two groups, nor do they account for the length of job spells or the right-censoring due to the end of the observation window. A crucial part of the identification strategy is to separate the reform effect from the effect of job duration, as well as to account for the fact that some jobs are not observed to end within the set of spells not affected by the layoff shock. This latter point is key in the current framework, as the likelihood of quitting decreases with time on the job and I am only considering job starts up to the reform date. This implies that monthly quit flows decrease in a mechanical way as the end of the observation period approaches, especially in the absence of the employer closure. The duration model allows to account for this issue, as it accommodates right-censored spells in a natural way.

Figure 1.3: Monthly job quit flows



Notes: Percentage of workers who voluntarily leave their employer each month over the total number of workers observed in each group. Monthly flows are smoothed using a centered moving average filter with a four months window. The vertical line identifies the reform moment (February 2012). Layoff shock applies to workers still employed 12 months before the start of the firm event year.

1.5 Estimation of a discrete time duration model

To estimate the model discussed in Section 1.3, the spell data is expanded so that the spell length of each individual determines a vector of binary responses. Specifically, I estimate a reduced-form discrete time duration model, where the individual quit hazard rate is specified to take the complementary log-log link form (Jenkins, 2005).[46] Recall from Table 1.4 that job spells not affected by any of the events are systematically shorter. To balance observed differences in the length of job spells and to guarantee there are observations at each realized duration in any of the four potential states defined by the occurrence of the events, job spells are artificially censored at 72 months. This implies that only quits occurring up to the 72th month contribute to identify the effect of interest.

Hazard rate. The individual job quit hazard rate is given by[47]

$$h(t|s,r,X,V) = 1 - exp(-exp(\tau R + \gamma S + \delta R \times S + \lambda(t) + X(t)\beta + V))$$

R is an indicator variable that takes value one after the policy change. S identifies workers who are affected by the layoff shock. $R \times S$ then stands for workers affected by the layoff shock and the policy change. δ measures the change in the mobility response of workers to the layoff shock due to the reduction in severance pay induced by the reform relative to the shift for those workers who are not affected by the layoff shock. Hence, δ refers to the average treatment effect on the treated.

To separate the effect of these events from confounding factors, I include a large set of explanatory variables that may have an impact on workers' mobility decisions. $\lambda(t)$ represents the baseline hazard, specified to be piece-wise constant with cut-off points selected to match the Q_T-quantiles of the observed

[46]The model fits the discrete time analogue to the continuous time proportional hazards model. Given that the data was grouped at monthly intervals from daily frequency durations, the choice of this model is the most appropriate compared to, for instance, a logit specification that should be used when the time is intrinsically discrete (Prentice and Gloeckler, 1978; Kalbfleisch and Prentice, 2011). Heckman and Navarro (2007) provide details on the identification of dynamic treatment effects in a discrete time setting.

[47]When estimating the job quit hazard rate, I treat layoffs and other types of separation as censored. This modeling assumption implies that competing risks are independent conditional on observed and unobserved factors, i.e. all relevant mobility decisions variables are accounted for in the model. In Table 1.B1 in Appendix 1.B I estimate a competing risk model using as alternative exits from employment quits and layoffs and correlated the unobserved component of each alternative. In Table 1.B1 in Appendix 1.B, I estimate a competing risk model using quits and layoffs as competing events of employment outflows and correlate the unobserved component of each alternative.

job quit duration. I set it to $Q_T = 10$, which imposes 9 parameters to estimate.[48] The baseline hazard thus accounts for the fact that job exit probabilities change over time spent employed. In other words, it captures the (negative) duration dependence pattern of job quit duration.[49] Notice that individuals are affected differently by the layoff shock and, hence, they exit at different moments affecting the composition of the workforce at each elapsed duration within the layoff shock period. To account for these dynamics, the baseline hazard is allowed to vary with time after the occurrence of the layoff shock.

X represents a vector of observed characteristics. The model accounts for both past and current characteristics.[50] Worker characteristics are gender, age, a dummy for college graduates, the share of time employed since labor market entry, and the immediately prior employment state (3 categories: non-employment, temporary job, permanent job) to account for the labor market path that led individuals to the current job. Regarding current job variables, I consider categorical variables for full-time job and high-skill occupations, and (log) real daily wages. To control for heterogeneity in the plants where workers start their jobs, the model includes sector of activity (10 categories), employer size and age. The estimation also includes the quarterly provincial unemployment rate and a nation-wide economic activity index at a monthly frequency to control for aggregate and provincial demand side effects. I also introduce year of hiring fixed effects (7) to account for aggregate conditions at job start, which have been pointed out as key determinants of future development of the labor relationship (Schmieder and von Wachter, 2010).

Observed characteristics are allowed to be time-varying whenever possible, as they provide a more robust source of model identification (Gaure et al., 2007). Lagged time-varying characteristics act as exclusion restrictions in the

[48]Note that with a sufficiently large number of time intervals any duration dependence pattern can be approximated closely. I re-estimate the model increasing the number of cut-off points for the baseline hazard with no significant change in the results.

[49]Figure 1.C1 depicts the empirical job quit hazard rate.

[50]Given that the dataset provides a natural starting point of each individual labor market history allows to introduce past labor market outcomes as explanatory variables in the model.

sense that past values of these covariates have an impact on the current transition probabilities only through the selection process (Brinch, 2007). Intuitively, workers with the same observed characteristics in period t but different values of past time-varying covariates should only have a different transition probability if the composition with respect to unobserved heterogeneity is unequal.[51]

V stands for the unobserved heterogeneity term. Accounting for unobserved determinants of the job quit rate is important as subjects with relatively high hazard rates due to unobserved factors leave the state of interest faster, so the sample of survivors is selected (Lancaster, 1990). Differences between groups of individuals at different times then reflect behavioral differences as well as selection effects. Recall that in the current setting workers affected by a given event stay, on average, longer than those not affected. Thus, not being able to control for unobserved factors correlated with the hazard of quitting can impact the identification strategy as workers who experience the event might not be a random sample. Unobserved determinants are then specified as random effects, which are assumed to follow a Gamma distribution (van den Berg, 2001; Abbring and van den Berg, 2007). Notice that the proportional hazard assumption implies that unobserved heterogeneity acts multiplicatively in the hazard rate and is assumed to be independent of X. However, with time-varying covariates, this assumption is not crucial for separating structural duration dependence and unobserved heterogeneity (Brinch, 2007).

[51] Note that, in the current setting, treatment assignment is dynamic and, hence, individuals who experience the occurrence of a given event belong to the non-treated group up to the incidence of the event. Thus, the inclusion of time-varying covariates allows to mitigate the impact of heterogeneity in mobility patterns that may lead some workers to be more likely to be affected by a particular event.

1.6 The impact of severance pay on voluntary mobility

1.6.1 Quit decisions

Table 1.6 presents the benchmark estimation results. The main parameter of interest (δ) measures the effect of the reduction in severance pay generosity induced by the 2012 Spanish labor market reform on the job quit hazard rate. Columns 1 to 5 control for a different set of covariates, independently. Column 6 presents the benchmark specification including all the covariates simultaneously.

The results point to a positive effect of the reduction in severance pay on the quit hazard rate. Specifically, the full model in Column 6 indicates that the cut in severance pay generosity shifted by roughly 13 percent the quit hazard rate for workers who face the layoff shock relative to workers not affected by the shock.[52] Column 2 shows that there is no significant different in the parameter estimated compared to Column 1. This suggests that the unobserved heterogeneity term seems to capture most of the worker-level heterogeneity that could bias the identified treatment effect. Columns 3 and 4 in turn indicate that both job and employer characteristics play a prominent role explaining differences in the behavioral response to the policy change between workers who face and those who do not face the layoff shock. Accounting for aggregate conditions is also important, as suggested by Column 5. Notice also that when including controls for aggregate conditions the reform effect becomes positive, which highlights the role of aggregate conditions affecting job quit rates.

The findings indicate that a reduction in mobility costs induced by the severance pay reform made workers more likely to voluntarily leave (quit) their

[52]The percentage effect is obtained by $100 \times (exp(\beta) - 1)$, where β is the parameter of interest.

employer. The results are in line with theoretical predictions pointing to a negative effect of severance pay generosity on voluntary mobility, due to the increase in the opportunity cost of job quit (Burdett, 1978; Mitchell, 1982). The results are also in line with previous empirical work finding a negative link between severance pay and voluntary mobility. Yet, the effect found is significantly smaller than in Kettemann, Kramarz and Zweimüller (2017). This could be expected, as Kettemann et al. (2017) analyzed the substitution of the severance pay system in Austria by an occupational pension scheme. In other words, they look at the abolition of severance pay, whereas this work investigates a milder reduction in severance pay entitlements and, hence, the workers' mobility response is expected to be lower.

Table 1.6: Benchmark specification

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect (δ)	0.2013***	0.2085***	0.1831***	0.1639***	0.1744***	**0.1236*** **
	(0.0337)	(0.0338)	(0.0337)	(0.0337)	(0.0343)	**(0.0343)**
Lay-off shock	0.3643***	0.3717***	0.4116***	0.3762***	0.3362***	0.3880***
	(0.0204)	(0.0204)	(0.0205)	(0.0205)	(0.0202)	(0.0202)
Post-reform	-0.3327***	-0.2858***	-0.3702***	-0.3297***	0.0717***	0.0688***
	(0.0143)	(0.0144)	(0.0143)	(0.0144)	(0.0198)	(0.0198)
Observations	8,642,598	8,642,598	8,642,598	8,642,598	8,642,598	8,642,598
No. spells	242,975	242,975	242,975	242,975	242,975	242,975
No. workers	201,932	201,932	201,932	201,932	201,932	201,932
Baseline hazard	Yes	Yes	Yes	Yes	Yes	Yes
Worker characteristics	No	Yes	No	No	No	Yes
Job characteristics	No	No	Yes	No	No	Yes
Employer characteristics	No	No	No	Yes	No	Yes
Macro effects	No	No	No	No	Yes	Yes
Unobs. heterogeneity	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Baseline hazard is a piece-wise constant function with 10 step points to match the deciles of the distribution of job quit duration. Worker characteristics include gender, age, a dummy for college graduates, share of time employed since labor market entry, and the immediately prior employment state (2 dummies). Job characteristics are indicators for full-time and high-skill occupations, and (log) real daily wages. Employer variables contain plant size and age, categorical variables for sector of activity (11), and a dummy variable for plants located in large metropolitan areas. Macro effects include dummy variables for year of job start (7), quarterly provincial unemployment rate, and the monthly national activity index. Unobserved heterogeneity is introduced as a random effect assuming a gamma frailty. LR test in Specification (6) of Gamma var. = 0 (92.02). Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

The impact of observed characteristics on the quit hazard rate conforms to existing literature on worker mobility (see Table 1.C6). Quit rates decrease with age, consistent with Burdett (1978) model. In line with human capital theory, highly educated workers who have more general human capital exhibit higher quit hazard rates. Female workers are less likely to voluntarily leave their employer, which can be related to the difficulty of women to get a permanent job in Spain (Guner et al., 2014). Concerning previous labor market history, individuals with more stable labor market careers exhibit a lower likelihood of voluntarily leaving their employer. There are no significant differences in job quit hazard rates between workers whose previous job was under a temporary contract those whose previous employment state was non-employment. In turn, workers whose previous employment state was a job under a permanent contract are less likely to quit. Furthermore, individuals coming from another permanent job are less likely to quit than previously non-employed individuals. Regarding job characteristics, job quit probabilities are lower for workers with full-time jobs or in a high-skill occupation. Moreover, quit rates decline with wage-level as better wage job offers are less likely (Burdett, 1978). Regarding employer characteristics, the results show that quit rates decrease both with employer size and age (Burdett and Mortensen, 1998). Voluntary mobility is negatively related to the unemployment rate, as outside options for employed workers are worse when the labor market is slack (Davis et al., 2012).

1.6.2 Assumptions under scrutiny

In this section, I test the assumptions made to identify the effect of the 2012 Spanish labor market reform on workers' quit decisions. First, I study the plausibility of the assumption about the exogeneity of the employer event. Second, I check the sensitivity of the results to the moment when the layoff shock is realized. Third, I perform a series of placebo tests that "anticipate" the actual reform date to check for the existence of pre-treatment effects.

Exogeneity of employer event. A potential threat to identification of the treatment effect concerns worker selection into employers that are bound to fail or experience a large employment contraction. To analyze this issue, I use matching as a selection mechanism. More precisely, for each job started in a mass-layoff or closing plant, I look for exact matches in terms of the following characteristics: hired in the same quarter, same gender, college degree, same sector (11 categories), and same location (50 provinces). If there are multiple matched subjects, I take the one with the nearest propensity score based on age, employment history, and previous employment state. This criterion allows to find a valid pair for 94 percent (Sample1). Additionally, I extend the selection criterion to include the quantiles of the plant-size distribution (Sample2) as an additional variable for exact matching. This norm generates a valid control for 81 percent of the subjects. Alternatively, I use the quantiles of the entry-level daily wage distribution to select controls (Sample3). This criterion yields a valid control for 82 percent of the workers starting a job in a firm which experiences a large employment contraction within a year. Finally, Sample4 includes in the initial criterion the quantiles of both size and daily wage distributions as variables to perform exact matching.[53] Table 1.7 compares the point estimates of the treatment effect between the benchmark sample and the alternative samples. The results suggest that worker selection does not compromise the identification strategy.

[53] Table 1.C5 shows observed characteristics for the full and matched samples.

Table 1.7: Exogeneity of employer closure

	Benchmark	Sample1	Sample2	Sample3	Sample4
Treatment effect (δ)	**0.1236*** **	0.1786***	0.1591***	0.1593***	0.1116**
	(0.0343)	(0.0403)	(0.0434)	(0.0428)	(0.0485)
Observations	8,642,598	4,521,647	4,108,197	3,970,129	3,188,717
No. spells	242,975	132,964	115,088	116,886	89,334
No. workers	201,932	105,206	88,407	93,207	69,771

Notes: Sample1 select controls for workers starting a job in plants that eventually close down based on exact matches in terms of the following characteristics: hired in the same quarter, same gender, college degree, same sector (11), and same province (50). If there are multiple matched controls, the one with the closest propensity score based on age, employment history, and previous employment state is chosen. Sample2 adds the quantiles of the plant size distribution at job start as an additional characteristic for the exact matching algorithm. Sample3 adds to the initial selection criterion the quantiles of the daily wage distribution at job start as an additional characteristic for the exact matching algorithm. Sample4 adds the quantiles of both the size and daily wage distribution to the initial selection criterion. All specifications use the same set of controls as in Table 1.6 Column 6. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Layoff shock realization moment. My identification strategy assumes that the arrival of information regarding the upcoming employer event (layoff shock) is realized 12 months before the year when the large employment drop occurs. Despite the timing being consistent with previous literature on workers' expectations about future job loss (Hendren, 2017), 12 months is still an arbitrary number. Table 1.8 presents the results of the benchmark specification for alternative time lengths to define the moment when the layoff information shock is realized.[54] The results indicated that the identified treatment effect varies smoothly around the 12 months considered in the benchmark specification. However, the results also highlight that the effect increases, the shorter is the time window considered for the arrival of the information shock. This suggests that a narrower period before the event year (6 months) might be overestimating the effect, as it is potentially capturing the realization of some

[54]Note that for wider time windows some job spells are excluded to guarantee the restriction on workers qualifying for severance pay when the layoff shock period is assumed to start. The results remain unchanged if all job spells included in the benchmark sample are included when using longer time horizons to define the arrival of the layoff shock.

decisions made by workers earlier in time. Similarly, the effect decreases the wider is the time window (18 months) assumed, in line with the idea that this longer period might be capturing workers who are more alike to individuals in non-event employers. In other words, the further away workers are from the future employer event the more similar is their mobility behavior relative to workers in stable employers. Overall, the findings are indicative of non-anticipation effects of the closure moment before the 12 months considered in the benchmark specification.

Table 1.8: Layoff shock arrival month

τ = event month							
	$\tau-6$	$\tau-9$	$\tau-11$	$\tau-12$	$\tau-13$	$\tau-15$	$\tau-18$
Treatment effect (δ)	0.1781***	0.1355***	0.1232***	**0.1236*** **	0.1197***	0.1143***	0.0972***
	(0.0379)	(0.0362)	(0.0350)	**(0.0343)**	(0.0342)	(0.0349)	(0.0360)
Observations	8,642,598	8,642,598	8,642,598	8,642,598	8,624,162	8,569,413	8,488,649
No. spells	242,975	242,975	242,975	242,975	242,931	240,456	236,030
No. workers	201,932	201,932	201,932	201,932	201,910	200,439	197,853

Notes: All specifications use the same set of controls as in Table 1.6 Column 6. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Placebo reforms. The identification strategy relies on the fact that in the absence of the reform the differences in the job quit hazard rate between workers who are and are not affected by the layoff shock would have remained constant. Figure 1.3 already suggested no differential paths of the quit rate prior to the actual reform moment between the two group of workers considered. In order to further examine the existence of pre-treatment effects, I repeat the same experiment as for the benchmark model, but using hypothetical reform dates and considering that the end of the observation period is the true reform moment (February 2012). Table 1.9 shows the results for different placebo reform dates. The point estimate for the effect of interest during the pre-reform period is at most 47 percent of the actual reform effect, and is never significantly different from zero. This is direct evidence of no pre-treatment effects.

Table 1.9: Placebo reforms

	Feb2012	Feb2010	Nov2009	Aug2009	May2009	Feb2009
Treatment effect (δ)	**0.1236*** **	0.0587	0.0425	0.0524	-0.0486	0.0199
	(0.0343)	(0.0389)	(0.0397)	(0.0398)	(0.0410)	(0.0406)
Observations	8,642,598	5,447,236	5,219,075	4,920,202	4,512,655	4,304,278
No. spells	242,975	186,742	179,987	173,045	165,597	157,321
No. workers	201,932	164,425	159,601	154,652	149,055	142,789

Notes: All specifications use the same set of controls as in Table 1.6 Column 6. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

1.7 Conclusion

This chapter studies the behavioral response of workers to severance pay generosity. To identify the effect of interest, I exploit the 2012 Spanish labor market reform, which reduced severance pay entitlements for permanent contracts, creating exogenous variation in severance pay generosity across job spells. I combine this reform with the exposure of some workers to a layoff information shock in a dynamic framework to isolate the reform effect from confounding factors that influence workers' voluntary mobility decisions.

The analysis shows that the impact of firing costs raised by employment protection legislation extends beyond firms' hiring and firing decisions. In particular, it shows that employment protection in the form a lump-sum transfer from the firm to the worker upon dismissal, distort workers' mobility decisions, as it increases the opportunity cost of job quit. My results point that a reduction in mobility costs induced by the reduction in severance pay made workers who may expect to be displaced in the near future more likely to voluntarily leave their employer. In fact, the job quit hazard rate for workers exposed to this layoff shock relative to workers not affected by such shock increased by 13 percent after the policy change.

The current findings have implications for labor market policy. First of all, they highlight that policies targeting firms behavior may have also an impact on workers' behavior, which should be taken into account for the design of labor market policies that alter the economic incentives embedded in the employment relationship of firms and workers. Secondly, the behavioral response of workers shows that varying severance pay generosity may affect the extent of layoffs and, consequently, the pool of workers who qualify for unemployment benefits. My results therefore highlight the relevance of analyzing together these two interlinked labor market institutions: employment protection and unemployment insurance.

Appendix 1.A Variables definition

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

Education. Retrieved from the Spanish Residents registry up to 2009, and from 2009 thereafter the Ministry of Education directly reports individuals' educational attainment to the National Statistical Office and this information is used to update the corresponding records in the Residence registry. Therefore, the educational attainment is imputed backwards whenever it is possible, i.e. when a worker is observed in the MCVL post-2009. In the imputation, I assigned 25 years as the minimum age to recover values related to university education.[55]

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

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

Labor market entry. The MCVL retrieves all relationships with the Social Security system since the date of the first job spell, or 1967 for earlier entrants. Therefore, the date of the first observed job spells is used as a proxy for labor market entry.

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

Potential labor market experience. Computed at the moment of job start as the difference between the date of the first spell observed in the MCVL and the date of the current spell.

Actual labor market experience. Computed relying on all the spells available for each worker in the MCVL. For each of the spells, I sum the number of days worked. At the moment of job start, actual labor market experience is the cumulative sum of years worked since the first wage-employment spell was observed.

Labor market history. Defined using the two measures of labor market experience defined above. Specifically, it is computed as the ratio between actual and potential labor market experience, which measures the share of time that an individual was employed since she entered the labor market for the first time up to a new job start.

Previous employment state. Defined using the difference between the starting date of the current job and the ending date of the previous job. Previous employment status refers to non-employment state if the difference between the two dates is over two weeks. If two weeks or less, I distinguish between two previous employment states: temporary and permanent contract.

Contract type. The MCVL contains a long list of contracts (+100 types) that are summarized in two broad categories, according to its permanent or temporary nature. Permanent contracts include regular permanent contracts (*contrato indefinido fijo*). Temporary contracts include specific project or service contracts (*temporal por obra o servicio*), temporary increase in workload (*eventual de produccion*), and substitution contracts (*interinidad o relevo*). Seasonal permanent contracts (*indefinido fijo-discontinuo*) are also included within the temporary contract category due to its intermittent nature.

Reason of termination. Reported by the employer to the Social Security administration (*causa de baja en afiliacion*). This variable is relevant to determine entitlements to severance pay and unemployment benefits. I create three major categories based on the following codes: code 51 refers to quits or voluntary separations, 54, 69, 77, 91, 92, 93 and 94 to layoffs or involuntary separations; and the remaining are assigned to other reasons for termination.

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

Full-time job. Hours worked by an individual are available as the percentage of time of a full-time job in the current employer. A job is defined as full-time if this variable has value 100, and as part-time otherwise.

Daily wages. Refers to the Social Security contribution base. It captures gross monthly labor earnings plus one-twelfth of year bonuses.[56] Earnings are bottom and top-coded. The minimum and maximum caps vary by Social Security regime and contribution group, and they are adjusted each year

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

according to the evolution of the minimum wage and inflation rate. Earnings are deflated using monthly CPI at the national level. Daily wages are computed dividing real monthly earnings by the number of days worked in a given month.

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

Sector of activity. Main sector of activity at a three-digit level (*actividad economica de la cuenta de cotizacion, CNAE*). Due to a change in the classification in 2009, the MCVL contains CNAE93 and CNAE09 for all plants observed in business from 2009 onwards, but only CNAE93 for those that stop their activity earlier on. I use the CNAE09 classification when available, and CNAE93 otherwise relying on the correspondence table provided by the Spanish National Statistical Office.[57] Then, I aggregate the three-digit industry information into 14 categories: agriculture and extraction (CNAE09 codes 1 to 99); manufacturing and utilities (100 to 399); construction (411 to 439); wholesale and retail trade (451 to 479); transportation and storage (491 to 532); accommodation and food services (551 to 563); information and communication technologies (581 to 639); financial, insurance and real estate activities (641 to 683); professional, scientific and technical activities (691 to 750); administrative, support and other services (771 to 829 and 950 to 970); education, health and social work (851 to 889); entertainment (900 to 949); public sector/social security (841 to 843) and international organizations (990).

[57]http://www.ine.es/daco/daco42/clasificaciones/rev.1/cnae2009_cnae93rev1.pdf

Plant size. Corresponds to the number of active employees in the contribution account at the data extraction moment. In case of inactive plants, this variable takes value zero.

Plant creation date. Date when the first employee was registered in the contribution account. I use this date as a proxy for the plant creation date to compute the age of the plant.

Plant location. The municipality in which the establishment conducts its activity if above 40,000 inhabitants, or the province for smaller municipalities (*domicilio de actividad de la cuenta de cotizacion*). Based on that, I create a dummy variable (biggest 4 cities) identifying Madrid, Barcelona, Sevilla, and Valencia that are the metropolitan areas with over 1 million inhabitants.

Unemployment rate. Refers to the provincial quarterly unemployment rate retrieved from the National Statistical Office. This variable can be downloaded from `http://ine.es/jaxiT3/Tabla.htm?t=3996&L=0`

Activity index. Measured using the FEDEA Index that summarizes the evolution of economic activity in Spain using information available from many different sources (GDP, industrial production, indices of economic sentiment, etc.). For a more detailed description of the index, see `http://www.fedea.net/indice/`

Appendix 1.B Sensitivity analysis

Table 1.B1: Hazard rate specification

	Single risk		Competing risks	
	Benchmark sample	Exclude other seps.	Benchmark sample	Exclude other seps.
Treatment effect (δ)	**0.1236*** **	0.1397***	0.1275***	0.1445***
	(0.0343)	(0.0343)	(0.0346)	(0.0345)
Observations	8,642,598	8,445,118	8,642,598	8,445,118
No. spells	242,975	238,018	242,975	238,018
No. workers	201,932	198,462	201,932	198,462

Notes: Single risk refers to the benchmark specification of the job quit hazard rate. Competing risks specification considers quits and layoff as alternative exits from employment and includes correlated unobserved frailties between the alternatives. Single risk and competing risks specifications based on complementary log-log link for the hazard rate. Exclude other seps. columns consider only quits and layoffs as job separations. All specifications use the same set of controls as in Table 1.6 Column 6. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 1.B2: Employer events

	Benchmark	No plant closings	No mass-layoffs	Mass-layoff 30%	Mass-layoff 50%
Treatment effect (δ)	**0.1236*** **	0.1093**	0.2115***	0.1760***	0.1973***
	(0.0343)	(0.0492)	(0.0459)	(0.0362)	(0.0399)
Observations	8,642,598	7,682,027	7,381,002	8,953,926	9,084,854
No. spells	242,975	215,489	199,501	251,059	257,008
No. workers	201,932	182,542	172,083	207,590	211,315

Notes: No plant closings removes establishments that go out of business from the analysis. No mass-layoffs excludes from the analysis establishments that experience a mass-layoff. Mass-layoff 30% and 50% columns modify the mass-layoff definition in the benchmark specification to select plants whose employment contracts by more than 30 or 50 percent within a year, respectively. All specifications use the same set of controls as in Table 1.6 Column 6. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 1.B3: Further sensitivity tests

	Benchmark	No Ref2010	First spell	Single plant	Corporations
Treatment effect (δ)	**0.1236*** **	0.1505***	0.1005**	0.1226***	0.1498***
	(0.0343)	(0.0446)	(0.0406)	(0.0457)	(0.0355)
Observations	8,642,598	7,362,773	7,220,368	5,253,560	7,728,756
No. spells	242,975	210,216	201,932	159,482	210,859
No. workers	201,932	180,449	201,932	137,912	178,092

Notes: No Ref2010 specification excludes from the analysis job spells created after the LM reform in June 2010. First spell model only considers the first job spell observed for each worker between January 2005 and July 2011. Single plant sample exclude multi-establishment firms. Corporations specification excludes from the analysis sole proprietor employers. All specifications use the same set of controls as in Table 1.6 Column 6. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Appendix 1.C Supplementary tables and figures

Table 1.C1: Summary statistics, analysis sample constraints

	Born +1950	Full info	Priv. sector	Gral. Regime	Standard jobs	Start +Jan05	Active plants	Sev. Pay	Final sample
Outcome variables[a]									
Completed	0.953	0.941	0.943	0.938	0.935	0.911	0.907	0.655	0.803
Quit	0.170	0.176	0.187	0.202	0.205	0.175	0.175	0.349	0.328
Layoff	0.802	0.818	0.808	0.792	0.788	0.820	0.819	0.626	0.647
Other reasons	0.029	0.006	0.005	0.006	0.006	0.005	0.0054	0.025	0.025
Duration (months)	13.80	13.80	13.47	14.89	15.31	10.81	11.52	40.89	51.87
Censored spells	75.61	69.32	64.81	66.72	67.02	41.22	43.22	58.33	114.1
Completed spells	10.72	10.34	10.37	11.45	11.74	7.820	8.251	31.72	36.64
Policy variables[a]									
After reform	0.290	0.346	0.346	0.327	0.332	0.508	0.514	0.655	0.433
Layoff shock	0.057	0.071	0.073	0.073	0.075	0.107	0.118	0.159	0.165
Job characteristics[a]									
Permanent contract	0.167	0.181	0.190	0.218	0.228	0.203	0.209	1	1
Full-time job	0.778	0.732	0.728	0.687	0.694	0.659	0.660	0.746	0.784
High-skill	0.093	0.076	0.060	0.060	0.056	0.058	0.060	0.150	0.141
Real daily wage	32.60	31.92	31.03	32.80	32.95	32.95	33.32	45.37	45.92
Worker characteristics[b]									
Female	0.467	0.469	0.464	0.465	0.461	0.467	0.466	0.465	0.471
Age	23.69	27.20	27.22	27.16	27.50	31.94	31.96	31.42	30.85
Spanish	0.816	0.844	0.839	0.845	0.844	0.818	0.821	0.845	0.846
College	0.145	0.153	0.144	0.149	0.148	0.141	0.143	0.213	0.197
Employer characteristics[c]									
Services	0.597	0.620	0.614	0.685	0.677	0.698	0.710	0.768	0.749
Biggest 4 cities	0.326	0.321	0.323	0.349	0.356	0.353	0.359	0.423	0.423
Plant Age	3.70	4.29	4.12	4.30	4.47	6.02	7.06	8.06	8.254
Size	7.73	8.34	7.42	8.02	8.26	10.11	12.50	21.92	27.44
No. spells	11,144,351	7,465,393	6,829,791	5,875,781	5,212,098	3,264,553	2,946,396	444,784	242,975
No. workers	1,124,248	992,250	949,949	914,939	883,971	714,052	692,203	311,582	201,932
No. plants	2,246,820	1,737,361	1,696,409	1,485,458	1,329,525	919,271	720,178	256,044	158,253

Notes: [a]Variables averaged over job spells at the moment of hiring. [b]Variables averaged across workers' first job spells observed. [c]Variables averaged across plant's first observation. After reform identifies job spells with at least one month observed after the reform date (February 2012). Layoff shock applies to workers still employed 12 months before the start of the firm event year. Real daily wages refer to the starting daily wage and are deflated using the 2017 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants. Final sample: job starters who already qualify for severance pay at the moment of occurrence of a given event.

Table 1.C2: Employer event: Observed characteristics for sub-groups

	Full sample		Exclude mass-layoff		Exclude closing	
	No event	Event	No close	Close	No mass-layoff	Mass-layoff
Worker-level variables						
Female	0.483	0.432	0.483	0.431	0.483	0.434
Age	30.82	31.17	30.82	31.29	30.82	31.17
Spanish	0.847	0.848	0.847	0.828	0.847	0.881
College	0.195	0.221	0.195	0.185	0.195	0.280
Employment history	0.564	0.593	0.564	0.592	0.564	0.607
Non-employment	0.576	0.487	0.576	0.510	0.576	0.442
Job-level variables						
Full-time job	0.765	0.832	0.765	0.818	0.765	0.847
High-skill	0.129	0.169	0.129	0.130	0.129	0.223
Real daily wage	44.29	49.87	44.29	46.70	44.29	54.39
Employer-level variables						
Services	0.770	0.681	0.770	0.695	0.770	0.658
Biggest 4 cities	0.411	0.470	0.411	0.463	0.411	0.484
Plant Age	8.07	9.22	8.07	8.046	8.07	11.94
Size	22.10	53.73	22.10	31.34	22.10	107.8
No. spells	172,015	70,960	172,015	39,115	172,015	27,486
No. workers	150,997	64,910	150,997	36,906	150,997	26,177
No. plants	121,585	39,151	121,585	26,798	121,585	10,945

Notes: All characteristics measured at the month of job start. Worker characteristics summarized across worker observations. Job characteristics are averaged across job spells. Employer characteristics computed at the plant-level. Layoff shock applies to workers still employed 12 months before the start of the firm event year. Non-employment identifies workers coming from non-employment at job start. Wages are deflated using the 2017 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Table 1.C3: Layoff shock: Observed characteristics for sub-groups

	Full sample		Closing plants		Mass-layoff plants	
	No shock	Shock	No shock	Shock	No shock	Shock
Worker-level variables						
Female	0.479	0.424	0.434	0.423	0.443	0.421
Age	30.79	31.53	30.87	31.66	30.86	31.40
Spanish	0.842	0.875	0.795	0.858	0.850	0.896
College	0.194	0.231	0.186	0.194	0.273	0.277
Employment history	0.558	0.619	0.572	0.616	0.585	0.625
Non-employment	0.574	0.453	0.539	0.471	0.471	0.419
Job-level variables						
Full-time job	0.770	0.855	0.794	0.846	0.818	0.868
High-skill	0.134	0.177	0.126	0.140	0.210	0.219
Real daily wage	44.71	52.06	44.64	49.03	50.95	55.62
Employer-level variables						
Services	0.766	0.653	0.742	0.657	0.730	0.638
Biggest 4 cities	0.421	0.471	0.483	0.463	0.537	0.493
Plant Age	8.17	9.65	8.23	8.34	11.45	12.13
Size	25.14	68.16	32.13	41.28	114.20	119.30
No. spells	202,829	40,146	19,954	23,520	12,616	19,229
No. workers	172,185	38,795	18,979	22,975	12,071	18,907
No. plants	140,583	25,135	14,890	16,848	6,537	9,943

Notes: All characteristics measured at the month of job start. Worker characteristics summarized across worker observations. Job characteristics are averaged across job spells. Employer characteristics computed at the plant-level. Layoff shock applies to workers still employed 12 months before the start of the firm event year. Non-employment identifies workers coming from non-employment at job start. Wages are deflated using the 2017 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Table 1.C4: Reform event: Observed characteristics for sub-groups

	Full sample		No shock		Shock	
	No reform	Reform	No reform	Reform	No reform	Reform
Worker-level variables						
Female	0.465	0.475	0.475	0.483	0.407	0.442
Age	30.85	31.14	30.78	31.02	31.50	31.68
Spanish	0.807	0.900	0.800	0.899	0.851	0.904
College	0.179	0.235	0.177	0.229	0.206	0.263
Employment history	0.543	0.619	0.536	0.614	0.607	0.640
Non-employment	0.584	0.506	0.601	0.523	0.461	0.429
Job-level variables						
Full-time job	0.778	0.793	0.761	0.783	0.872	0.837
High-skill	0.124	0.163	0.117	0.156	0.162	0.193
Real daily wage	43.35	49.30	42.07	48.29	50.42	53.90
Employer-level variables						
Services	0.744	0.764	0.765	0.774	0.616	0.708
Biggest 4 cities	0.443	0.414	0.441	0.407	0.481	0.474
Plant Age	7.80	9.69	7.63	9.82	9.51	10.45
Size	28.21	41.88	24.81	40.99	72.02	84.64
No. spells	137,881	105,094	1167,25	86,104	21,156	18,990
No. workers	119,168	105,094	101,649	86,104	20,799	18,990
No. plants	103,891	70,727	92,043	60,884	15,001	11,712

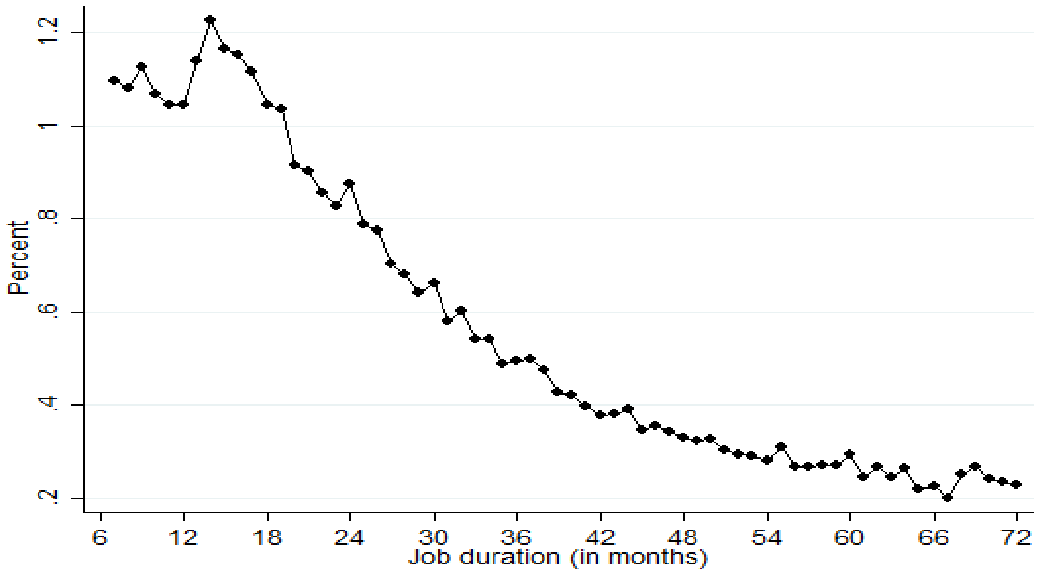
Notes: All characteristics measured at the month of job start. Worker characteristics summarized across worker observations. Job characteristics are averaged across job spells. Employer characteristics computed at the plant-level. Layoff shock applies to workers still employed 12 months before the start of the firm event year. Non-employment identifies workers coming from non-employment at job start. Wages are deflated using the 2017 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Table 1.C5: Observed characteristics for benchmark and matched samples

	Benchmark		Sample1		Sample2		Sample3		Sample4	
	No event	Event	No event	Event	No event	Event	No event	Event	No event	Event
Worker-level variables										
Female	0.483	0.432	0.446	0.429	0.442	0.423	0.440	0.423	0.437	0.415
Age	30.82	31.17	30.79	31.16	30.74	31.11	30.78	31.07	30.65	31.07
Spanish	0.847	0.848	0.840	0.844	0.847	0.839	0.836	0.838	0.845	0.834
College	0.195	0.221	0.198	0.201	0.191	0.191	0.187	0.190	0.189	0.194
Employment history	0.564	0.593	0.584	0.595	0.593	0.599	0.587	0.597	0.598	0.603
Non-employment	0.576	0.487	0.520	0.486	0.516	0.483	0.526	0.488	0.513	0.479
Job-level variables										
Full-time job	0.765	0.832	0.806	0.832	0.827	0.838	0.823	0.833	0.836	0.843
High-skill	0.129	0.169	0.150	0.163	0.161	0.162	0.161	0.162	0.178	0.175
Real daily wage	44.29	49.87	47.18	49.71	49.68	50.19	49.55	49.64	51.37	51.21
Employer-level variables										
Services	0.770	0.681	0.726	0.676	0.718	0.673	0.725	0.673	0.714	0.672
Biggest 4 cities	0.411	0.470	0.506	0.495	0.544	0.549	0.546	0.538	0.609	0.624
Plant Age	8.06	9.22	8.46	9.29	9.80	9.43	8.66	9.43	10.07	9.79
Size	22.04	53.84	39.22	55.43	58.77	58.91	42.23	58.24	66.70	65.60
No. spells	172,015	70,960	66,482	66,482	57,544	57,544	58,443	58,443	44,667	44,667
No. workers	150,997	64,910	44,195	61,011	35,389	53,018	39,387	53,820	28,351	41,420
No. plants	121,585	39,151	38,184	36,992	27,312	32,214	33,617	33,112	21,568	25,345

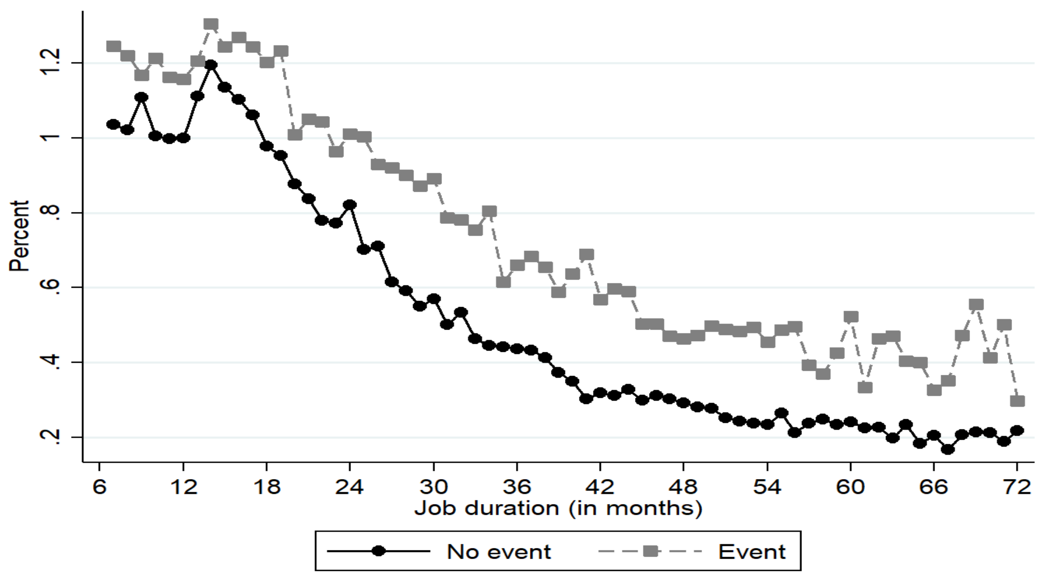
Notes: Sample1 select controls for workers starting a job in plants that eventually close down based on exact matches in terms of the following characteristics: hired in the same quarter, same gender, college degree, same sector (11), and same province (50). If there are multiple matched controls, the one with the closest propensity score based on age, employment history, and previous employment sate. is chosen. Sample2 adds the deciles of plant size at job start as an additional characteristics for the exact matching algorithm. Sample3 uses the same criterion as Sample1 but considering only workers who experience the layoff shock. Layoff shock applies to workers still employed 12 months before the start of the firm event year. All characteristics measured at the month of job start. Worker characteristics summarized across worker observations. Job characteristics are averaged across job spells. Employer characteristics computed at the plant-level. Non-employment identifies workers coming from non-employment at job start. Wages are deflated using the 2017 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Figure 1.C1: Empirical quit hazard rate



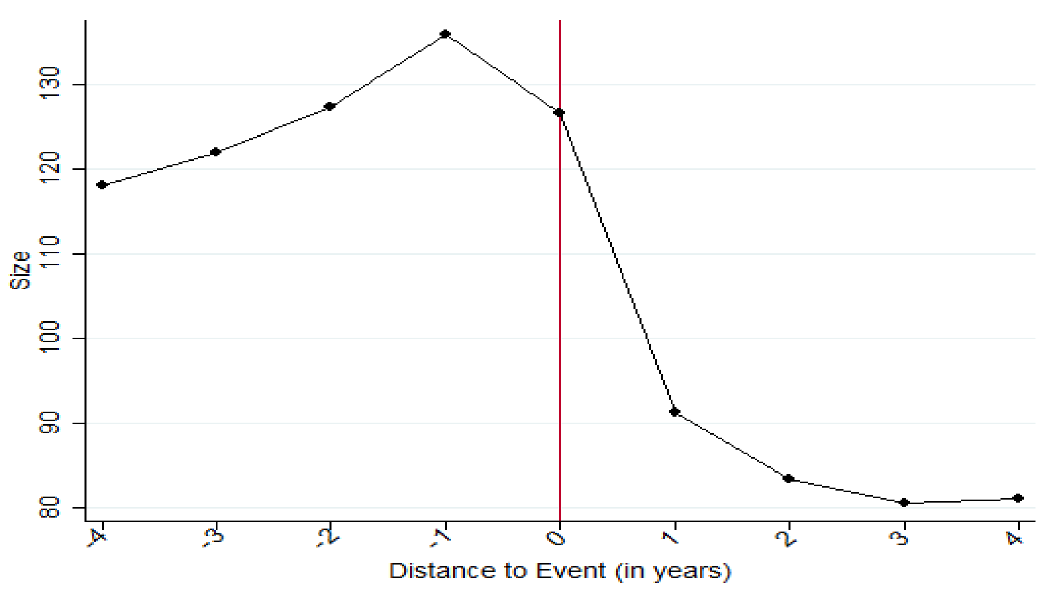
Notes: The figure depicts the job quit hazard rate, which exhibits a negative duration dependence pattern, i.e. the quit rate decreases with time employed. The empirical job quit hazard rate represents the share of workers quitting at a given realized duration over the total number of workers still employed at that exact job duration.

Figure 1.C2: Empirical quit hazard rate by employer event



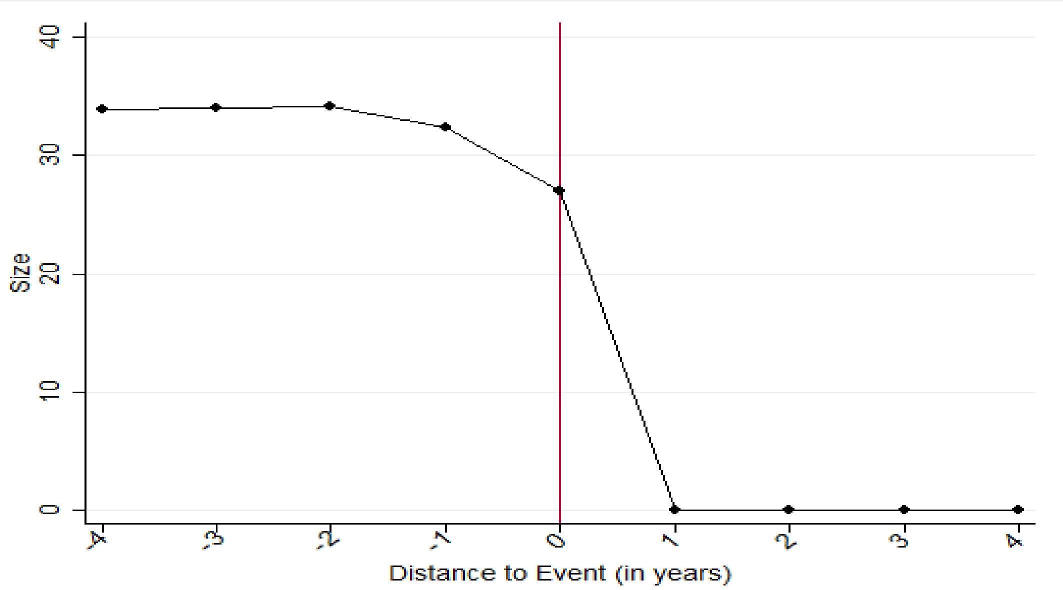
Notes: The figure depicts the job quit hazard rate for both workers in *stable* firms (no event) and workers in firms which experience the large employment contraction (event). The empirical job quit hazard rate is computed as the share of workers quitting at a given realized duration over the total number of workers still employed at that exact job duration in each group.

Figure 1.C3: Evolution of plant-size around event moment (mass-layoffs plants)



Notes: The figure depicts the evolution of plant size around the start of the event year (vertical line) for mass-layoff plants.

Figure 1.C4: Evolution of plant-size around event moment (closing plants)



Notes: The figure depicts the evolution of plant size around the start of the event year (vertical line) for closing plants.

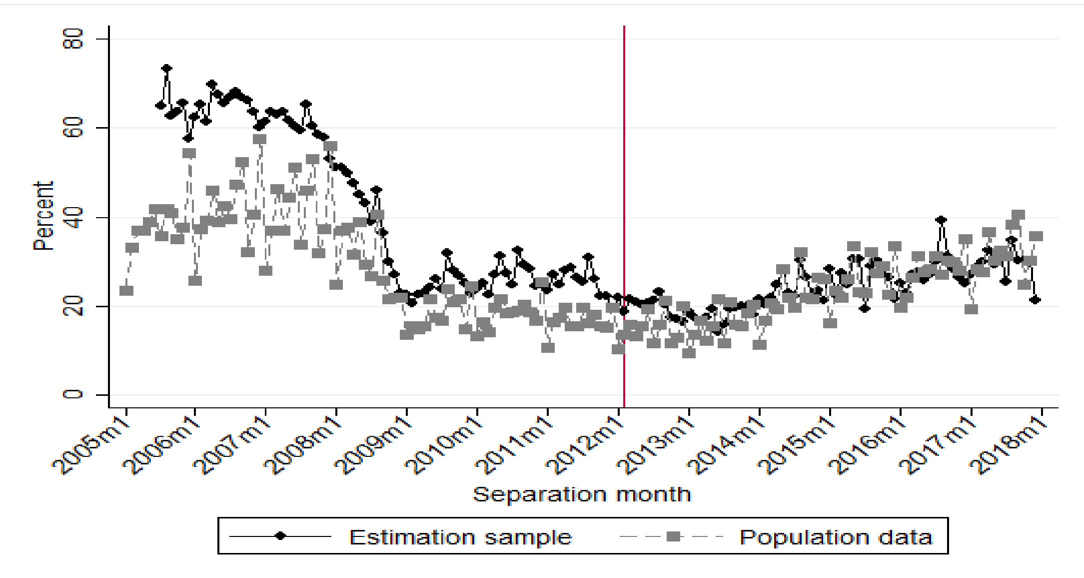
Table 1.C6: Benchmark specification

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect (δ)	0.2013***	0.2085***	0.1831***	0.1639***	0.1744***	**0.1236*** **
	(0.0337)	(0.0338)	(0.0337)	(0.0337)	(0.0343)	**(0.0343)**
Lay-off shock	0.3643***	0.3717***	0.4116***	0.3762***	0.3362***	0.3880***
	(0.0204)	(0.0204)	(0.0205)	(0.0205)	(0.0202)	(0.0202)
Post-reform	-0.3327***	-0.2858***	-0.3702***	-0.3297***	0.0717***	0.0688***
	(0.0143)	(0.0144)	(0.0143)	(0.0144)	(0.0198)	(0.0198)
Female		-0.1037***				-0.2809***
		(0.0083)				(0.0089)
Spanish		-0.7370***				-0.6037***
		(0.0099)				(0.0103)
College		0.1788***				0.2462***
		(0.0099)				(0.0115)
Age		-0.0211***				-0.0172***
		(0.0006)				(0.0006)
Emp. history		-0.3442***				-0.1267***
		(0.0142)				(0.0144)
Prev. temporary contract		-0.0269**				0.0003
		(0.0111)				(0.0112)
Prev. permanent contract		-0.1691***				-0.1025***
		(0.0121)				(0.0124)
High-skill			0.3464***			0.2843***
			(0.0115)			(0.0134)
Full-time job			-0.0774***			-0.1637***
			(0.0117)			(0.0122)
(log) Real daily wage			-0.5101***			-0.4365***
			(0.0073)			(0.0082)
Size/100				-0.0175***		-0.0135***
				(0.0009)		(0.0008)
Plant Age				-0.0067***		-0.0060***
				(0.0004)		(0.0004)
Unemp. rate					-0.0423***	-0.0502***
					(0.0010)	(0.0010)
FEDEA Index					0.2192***	0.2074***
					(0.0085)	(0.0086)
Constant	-4.5036***	-3.0313***	-2.6328***	-4.9229***	-3.9008***	-1.3526***
	(0.0113)	(0.0239)	(0.0241)	(0.0177)	(0.0154)	(0.0380)
Observations	8,642,598	8,642,598	8,642,598	8,642,598	8,642,598	8,642,598
No. spells	242,975	242,975	242,975	242,975	242,975	242,975
No. workers	201,932	201,932	201,932	201,932	201,932	201,932
Baseline hazard	Yes	Yes	Yes	Yes	Yes	Yes
Worker characteristics	No	Yes	No	No	No	Yes
Job characteristics	No	No	Yes	No	No	Yes
Employer characteristics	No	No	No	Yes	No	Yes
Macro effects	No	No	No	No	Yes	Yes
Unobs. heterogeneity	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Baseline hazard is a piece-wise constant function with 10 step points to match the deciles of the distribution of job quit duration. Employer characteristics refer to categorical variables for sector of activity (11). Macro effects include dummy variables for year of job start (7). Unobserved heterogeneity is introduced as a random effect assuming a gamma frailty. LR test in Specification (6) of Gamma var. = 0 (92.02). Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

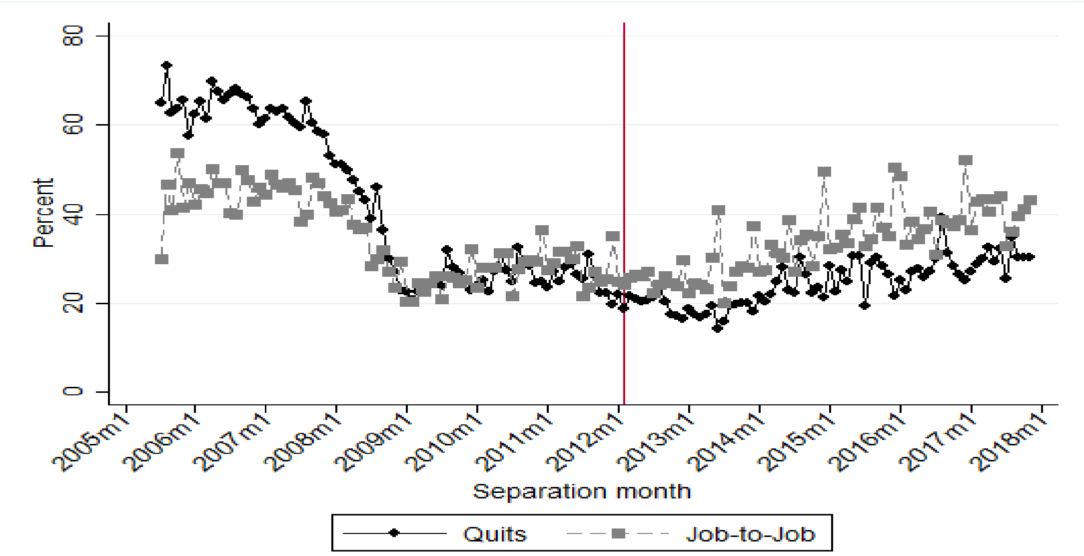
Appendix 1.D Further evidence on LM conditions in Spain

Figure 1.D1: Quits over total separations



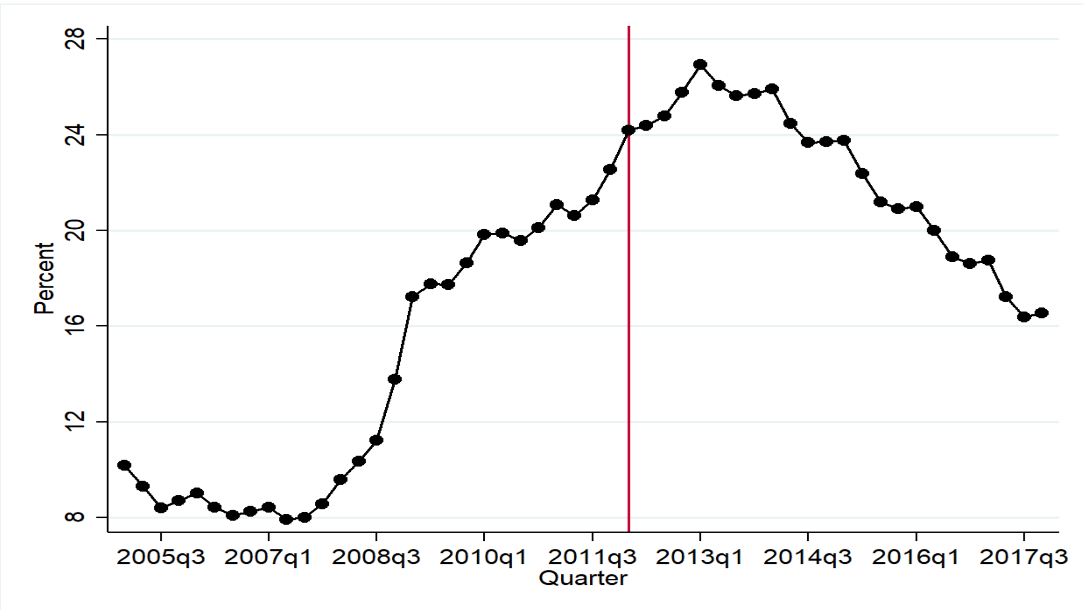
Notes: The figure shows the share of quits over total separations by month of spell end for permanent contracts in the General Regime of the Social Security. Estimation sample refers to the MCVL data after applying the constraints described in Section 3.4. Population data comes from the Spanish Ministry of Employment and Social Security. Population data also includes finished contracts that started before January 2005.

Figure 1.D2: Quits versus job-to-job transitions



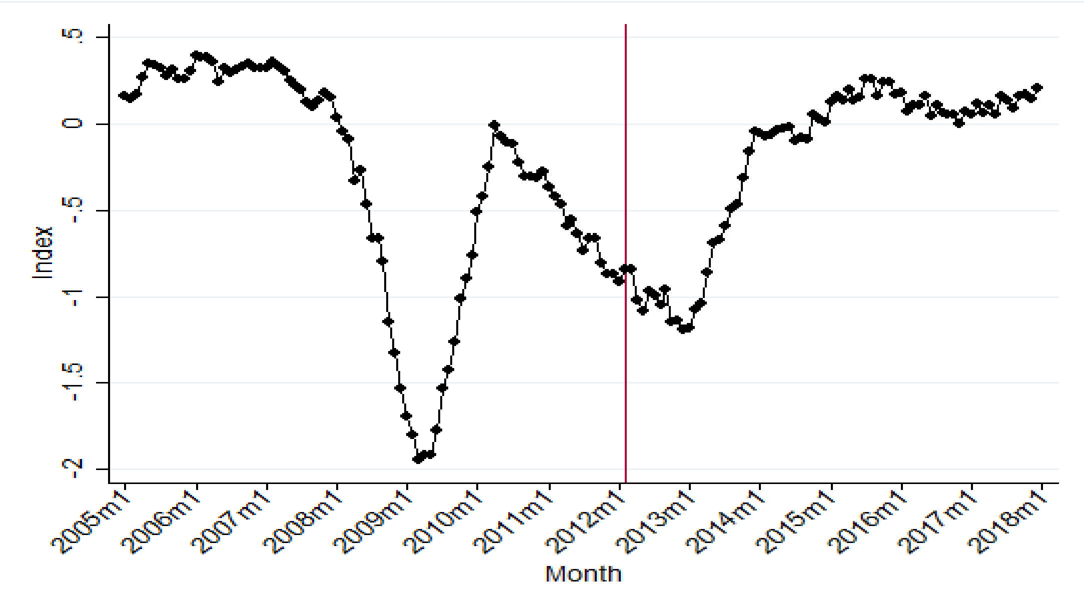
Notes: The figure shows the share of quits and job-to-job transitions over total separations by month of spell end for permanent contracts in the estimation sample. Job-to-job transitions are defined as workers switching employers with at most 15 days in between the end of the previous job and the start of the new job.

Figure 1.D3: National unemployment rate, 2005-2017



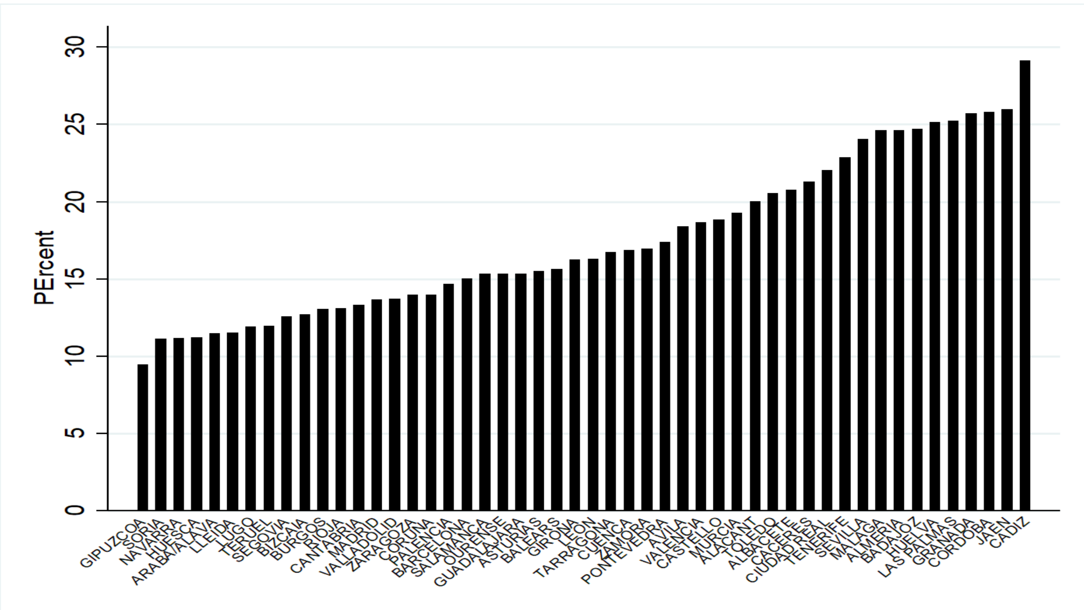
Source: *Instituto Nacional de Estadistica*.

Figure 1.D4: FEDEA index of economic activity, 2005-2017



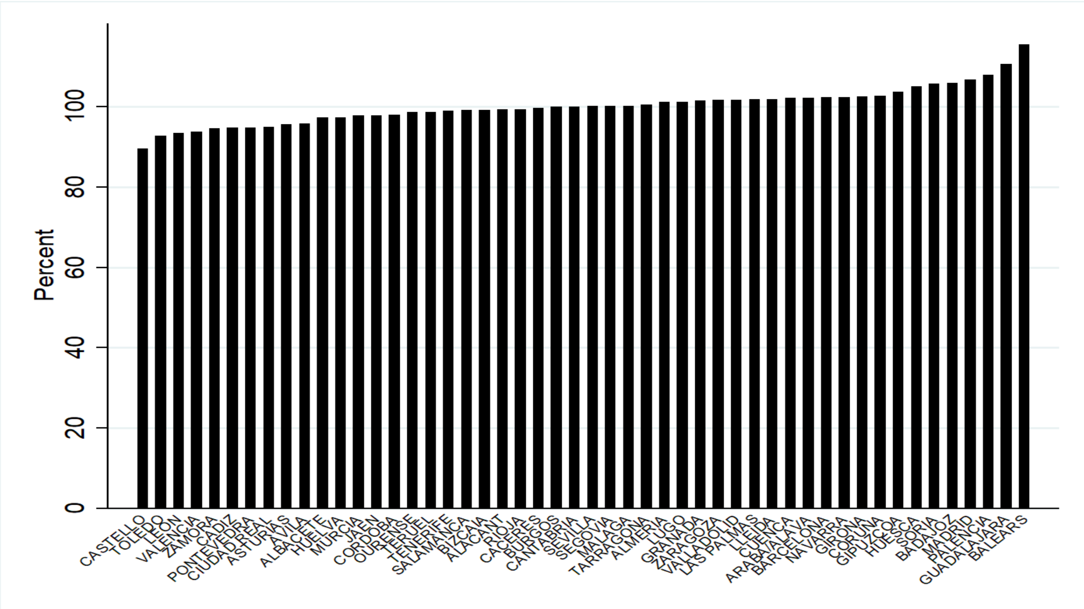
Source: *FEDEA.*

Figure 1.D5: Average unemployment rate by province, 2005-2017



Source: *Instituto Nacional de Estadistica.*

Figure 1.D6: Employment by province, ratio 2017/2005



Source: *Ministario de Empleo, Migraciones y Seguridad Social*.

Chapter 2

Coworker Networks and the Labor Market Outcomes of Displaced Workers: Evidence from Portugal

with Marta Silva

2.1 Introduction

A rich and growing literature has been highlighting the importance of social contacts in the labor market, helping both job seekers to find job opportunities and employers to fill vacancies. In particular, cross-country evidence indicates that between 30 to 50 percent of all job matches are typically obtained through personal connections rather than through formal search methods (Topa, 2011).

The main explanation for this copious use of social contacts is related to the potential to act as an information transmission mechanism for both agents in the labor market. On the one hand, personal connections might ease the job search process as a result of the exchange of information about job opportunities among social contacts (Topa, 2001; Calvo-Armengol and Jackson, 2004, 2007). On the other hand, employers might rely on social contacts as a recruitment strategy in which such contacts act as referrals and provide information about the unobserved quality of potential employees (Montgomery, 1991; Galenianos, 2013; Dustmann et al., 2016; Glitz and Vejlin, 2019). These mechanisms suggest that social contacts tend to reduce informational asymmetries and improve match quality (Beaman, 2016).

In this chapter, we use Portuguese administrative data to investigate the role of social contacts on the labor market outcomes of displaced workers. We are interested in the impact of personal connections both on hiring probabilities of displaced workers as well as the re-employment outcomes of those who succeed finding a job. To perform this analysis, our empirical strategy relies on firm closures to identify workers who are exogenously displaced and, hence, forced to search for new jobs. We then define their social contacts as personal connections that arose from interactions at the workplace during the years before the displacement event (Cingano and Rosolia, 2012; Glitz, 2017; Eliason et al., 2019; Saygin et al., 2019; Glitz and Vejlin, 2019).

We proceed with our analysis in two steps. We start studying the impact of former workers on the hiring probabilities of displaced workers. We hinge on the coworkers network concept to identify firms that are connected to each closing firm through former coworkers of the displaced workers who are employed at the moment of firm closure, similarly to Kramarz and Skans (2014); Saygin et al. (2019); or Eliason et al. (2019). This empirical strategy allows us to shed light on the role of having a direct tie to a firm through a former coworker on the re-hiring probability of a displaced worker relative to a worker displaced from the same displacement event but who does not have such a link. After studying the role of former coworkers affecting hiring probabilities, we focus on successful displaced workers to analyze how having a connection in the hiring firm shapes their re-employment outcomes. In this second step, our goal is two fold. On the one hand, we are interested in understanding whether having a former coworker improves the re-employment perspectives of displaced workers. On the other hand, we seek to shed light on whether these former coworkers might be transmitting information about the quality of the potential new employee that otherwise would not be observable by the employer. To explore these issues, we compare entry-level wages and type of contract of connected and non-connected workers, as well as their employment outcomes three years after hiring in terms of wage growth, employment and job stability.

Our analysis yields the following results. Firstly, the hiring analysis indicates that former coworkers improve hiring probabilities of displaced workers. We find that displaced workers with a direct link to a connected firm are 2.4 times more likely to be hired by that firm relative to workers who were displaced from the same closing event but without that direct link to the connected firm. We find however some heterogeneity across demographic groups, being males, young, and low educated individuals those who benefit the most from having a connection in the firm. Secondly, our results indicate that former coworkers links play a prominent role improving hiring probabilities that involve either inter-industry or regional mobility, which suggests that former coworkers might be a source of information and ease job transitions that can be more difficult to take place in the absence of that information advantage. Thirdly, among displaced workers who succeed getting a new job, we find that connected workers earn higher starting wages and are more likely to start the new job under a permanent contract. Fourthly, we also find that, three years after re-employment, connected workers are more likely to remain employed in the same firm, but these differences are driven by the type of firms were connected workers typically start their new jobs. Fifthly, our results suggest that initial differences in wages seems to dissipate over time. Finally, conditional on starting a job under a temporary contract, we do not find differences in the conversion of these contracts into permanent contracts between connected and non-connected workers.

We aim to contribute to the empirical literature on the role of social contacts in the labor market in different dimensions. Firstly, most of the existing literature has studied the impact of social contacts in countries with flexible and dynamic labor markets. Portugal instead is characterized by low levels of worker mobility and high levels of long-term unemployment (Blanchard and Portugal, 2001; OECD, 2013a). Thus, our study adds to the existing literature by analyzing the relevance of social contacts for re-employment outcomes in a sluggish labor market.

Secondly, an important literature on the costs of job displacement has documented strong and persistent negative effects in terms of earnings and future employment stability (Jacobson et al., 1993; Stevens, 1997; Eliason and Storrie, 2006; Davis and von Wachter, 2011; Raposo et al., 2015; Lachowska et al., 2018). Our focus on displaced workers and their labor market outcomes then allows us to shed light on how former coworker networks can help these workers to recover from displacement, especially in a country where only one fourth of the workers find a job after a year (OECD, 2013a), and almost 40 percent of the wage losses associated with job displacement are accounted for by poorer worker-firm match quality upon re-employment (Raposo et al., 2015).

Finally, displaced workers face a high risk of re-entering employment under atypical forms of work, such as fixed-term or part-time contracts (OECD, 2013a). If the quality of a new worker-firm match is an experience good (Jovanovic, 1979), employers may use fixed-term contracts, which entail lower firing costs, to evaluate the quality of the match when hiring displaced workers. In this regard, employers could rely on former coworkers as referrals (Galenianos, 2013; Glitz and Vejlin, 2019), to reduce the initial uncertainty about the quality of the match, instead of using fixed-term contracts to screen workers. Thus, our analysis on the type of contract that connected workers got upon re-employment adds to the existing literature on whether former coworkers are a source of information about the quality of new potential employees to their employers and, hence, reduce initial uncertainty. In the same vein, our article contributes to the debate in the Portuguese economy on whether fixed-term contracts are used as screening devices (Varejao and Portugal, 2005; Portugal and Varejao, 2009) or just as buffer stocks allowing firms to adjust their employment level (Centeno and Novo, 2012).

The rest of the chapter is structured as follows. Section 2.2 describes the key institutional features of the Portuguese labor market. In Section 2.3, we provide information on our data and describe our estimation sample and key concepts. Section 2.4 is devoted to the hiring analysis, whereas Section 2.5 focuses on re-employment outcomes. Section 2.6 concludes.

2.2 The Portuguese labor market

The institutional setting in the Portuguese labor market is characterized by three key elements: collective wage agreements, stringent employment protection, and generous unemployment insurance.

Wages in Portugal are determined by mandatory minimum wages and sector-wide collective agreements. Collective agreements are conducted by trade unions and employers' federations and set minimum working conditions with respect to the base monthly wage for each category of workers, overtime pay, and the normal duration of work. A salient feature is the existence of extension mechanisms of the agreements to the whole sector of activity, which makes the agreements binding for all the employers regardless of the rate of worker's unionization.[1] Therefore, national legal minimum wages and pervasive wage floors fixed by collective bargaining translate into downward nominal wage rigidity (Carneiro et al., 2014; Guimaraes et al., 2017).

During the sample period, Portugal was the OECD country with the most stringent employment protection legislation for permanent contracts (OECD, 2004, 2013b).[2] Highly protected contracts coexist with temporary contracts that have lower employment protection. Fixed-term contracts represent approximately 20% of total employment in Portugal and since 2006 accounts for over 80% of all new hires (OECD, 2014).[3] Fixed-term contracts may be used

[1] Although the share of unionized workers is as low as 11%, sector-level collective agreements cover approximately 90% of the workers (Martins, 2015). The use of extension mechanisms was limited after 2011 and could only take place if the companies signing the agreement represented over than 50% of the employment level of the sector of activity.

[2] In 2011, the Portuguese government passed a law that reduced severance payments for permanent contracts from 30 days of base wages and seniority payment to a common level of 20 days. After October 2013, the severance payment was reduced to 18 days of base wages and seniority payment for the first three years of tenure and 12 days afterwards.

[3] Other types of contract, such as temporary agency workers, represent less than 5% of the total number of employees.

to fulfill temporary tasks or needs, to launch a new activity, or to hire long-term unemployment workers or people seeking for the first job. The maximum legal duration of these type of contracts is typically equal to three years.[4] Workers on fixed-term contracts are entitled to severance payments upon termination of the term, but they cannot appeal to courts in case of dismissal, implying much lower procedural costs than open-ended contracts.[5]

Portugal is also one of the European countries with the most generous unemployment insurance systems (Venn, 2012). The eligibility criteria depends on the number of days worked during the past 24 months.[6] The average replacement rate is 65% of the average monthly gross pay earned in the 12 months before the last employed month. The duration depends both on the age of the worker and the length of the contributory period. The generosity of the unemployment insurance system is typically blamed for the low unemployment outflows observed (Addison and Portugal, 2008). Additionally, the generosity of unemployment insurance along with the stringency of the employment protection is associated with the low level of worker flows given job flows and the unemployment rate, and also with the high-levels of long-term unemployment (Blanchard and Portugal, 2001; OECD, 2014).

[4]Between 2004 and 2009, the maximum duration was six years. In 2012 it was increased to four and a half years, and it was increased again in 2013 to five and a half years.

[5]Workers on fixed-term contracts are entitled to receive severance payments since 1989, when it corresponded to 2 days of base wage for each complete month of tenure. In 2001, it increased to 3 days of base wages and in 2003, it decreased to 2 days of base wage and seniority payment per month worked for contracts that lasted longer than six months. After a major labor market reform which came into force in 2011, fixed-term contracts celebrated after November 2011 were entitled to a severance payment equal to 20 days of base wage and seniority payment. After September 2013, the severance payment is equal to 18 days of base wage and seniority payment in the first three years of tenure and 12 days thereafter.

[6]The eligibility criteria changed several times. Before 2003, the contribution period was 540 days over the previous 24 months. Between 2003-2006, this conditions was 270 days during the previous 12 months. From 2006 up to 2012, the eligibility was based on 450 days over the previous 24 months, and from 2012 was lowered to 360 days during the same 24 months.

2.3 Data, estimation sample, and descriptive statistics

Our main data source is *Quadros de Pessoal* (QP), a Portuguese linked employer-employee database, for the period 1986 to 2013. QP is an administrative database collected on an annual basis by the Ministry of Labor and Social Solidarity.[7] This dataset covers all firms with at least one paid employee, which are required to report detailed information on the firm and each one of its employees and establishments. QP contains information on the main economic activity, founding date, location and shareholder equity of the firm. For each worker, the firm reports gender, age, education, occupation, type of contract, hours worked and wages earned.[8] Each unit (firm, worker, establishment) is assigned a unique identifier, which allows us to follow them over time. Therefore, this dataset is suitable to conduct the network analysis since it allows us to identify the set of coworkers that shared the same establishment as the displaced worker at some point in time. Given that the data reported by the firm is publicly displayed at the firm and each worker is entitled to consult it, the measurement error in the information reported is minimized.

2.3.1 Estimation sample and definitions

Our initial sample consists of 55,776,315 observations corresponding to 7,404,544 workers observed over 841,364 firms.[9] From this sample, we observe 628,234 workers more than once in a year, corresponding to 2,586,569 worker-year observations. In such cases, we select a unique worker-year observation according to the following criteria. Firstly, we discard 23,975 exact

[7]The data reports to the the month of March until 1993 and October afterwards.

[8]The type of contract is only available from 2002 onwards and, for this reason, we will focus on the period 2002-2013 in our analysis.

[9]From the original dataset we have excluded firms in the agriculture, household activities, public firms, and international organizations. Similarly, we have left out from the initial sample workers without valid identifiers.

duplicated worker-year observations. Secondly, we look at personal traits and discard repeated observations with inconsistencies with respect to gender or year of birth (453,918 worker-year observations dropped).[10] Thirdly, we select the main job according to the longer hours worked or, in case of a tie, the job paying higher wages (779,319 worker-year observations excluded). If there are still more than one worker-year observation, we opt to keep the observation for which the firm identifier is the same as in the previous year to keep the consistency of the worker's employment history and properly identify displaced and co-displaced workers (135,658 worker-year observations dropped). Finally, if after these criteria, we still observe workers with more than one job in the year, we decide to exclude the worker from the sample (302,684 worker-year observations).[11] Finally, we focus on employees aged 20 to 55 who worked at least one hour in the reference month and had complete information. This latter restriction also implies that we restrict the analysis to the period 2002-2013, as the type of contract is only available after 2002 and it is a key variable (30,349,326 worker-year observations discarded). These constraints yield a final sample of 23,755,410 observations corresponding to 4,178,774 workers observed over 538,019 firms. From this sample, the main agents in our analysis are defined as follows.

Closing firms. We define closing firms according to the last year when we observe the employer identifier in the data. We refer to this last year as the closing year. To mitigate the inclusion of firms involved in corporate actions such as merges and acquisitions as closing firms, we analyze worker flows between firms. Specifically, we re-code as non-closing those firms with at least 5 employees for which we observe more than 50% of their workforce moving to the same firm in the year following the closure event. Finally, we consider only firm's closings that involved at least the displacement of two workers

[10] We remove observations such that gender and year of birth are different from the information reported in more than 50% of the observations of a given worker. This selection criterion led us to exclude 6,281 unique workers.

[11] This observations correspond to 23,758 unique worker identifiers.

due to the empirical design explained below. This definition yields 19,071 firms that went out of business between 2002 and 2009.

Displaced workers. From the closing firms defined above, displaced workers are those individuals who are employed in the last year in which we observe the firm in operation and have at least one year of tenure. Moreover, in order to exclude spurious behavior of workers involved in several firm closure events, we only look at workers who experience a single displacement event. We end up with 166,796 workers who were displaced between 2002-2009.

Former coworkers. We consider as former coworkers individuals with whom the displaced workers shared the same establishment in at least one of the five years before displacement year. Workers who are displaced by the same closure event are excluded from the set of former coworkers, as these individuals belong to the control group in our identification strategy to study hiring probabilities. Additionally, for a former coworker to be a suitable connection in the sense that she can provide information either to the displaced worker about job opportunities or refer that worker to her current employer, we require that the former coworker is already employed by another firm at the time of displacement and is still employed by that firm in the following year. We obtain 132,287 former coworkers satisfying the previous criteria.

Connected firms. We refer as connected firms to those employers that are linked to at least one of the closing firms through former coworker networks. In other words, these are firms where at least one of the former coworkers of one, or some, of the displaced workers is employed at the displacement moment and the year after. Thus, all workers displaced from the same closure event are connected (directly or indirectly) to the same set of firms, which can potentially hire them. This strategy produces a set of connected firms conformed by 58,717 employers.

2.3.2 Descriptive statistics

In this subsection, we summarize the characteristics of all the labor market agents involved in the analysis.[12] Namely, we present descriptive statistics for displaced workers and their former coworkers; and closing, hiring and connected firms included in our estimation sample.

Displaced workers. Panel A of Table 2.1 reports summary statistics of the 166,796 displaced workers in our estimation sample. Around 46 percent of the displaced workers are female and, on average, they are 37 years old and have low levels of education (over 70% of workers have at most 9 years of education). More than half of the workers who lost their job due to firm closure had a blue-collar occupation and almost 80% were on open-ended contracts in the closing firm. Importantly, only 21% were able to find a new job within one year.[13] In Panel B of Table 2.1, we present the characteristics of these displaced workers who succeed finding a job at most one year after displacement. These workers are less likely to be female and they are on average 2 years younger. With respect to the job held in the closing firm, at the displacement year, they had less tenure, earned lower wages, and were slightly less likely to be under a temporary contract.

In terms of the coworker network, each displaced worker has, on average, around 60 former coworkers, but only 13.7% out of those are employed at the displacement moment (and the year after). Moreover, each worker is connected to approximately 44 firms, but they have at least one connection in only 4 out of those firms. These figures are not very different if we only consider workers who were able to find a job one year after the displacement event. The most remarkable differences are in the set of employed former cowork-

[12] Appendix 2.A provides a detailed description of the variables.

[13] Accdoring to OECD (2014), the share of displaced workers who find a new job within one year in Portugal decreased from approximately 35% between 2000 and 2008 to 25% in the awake of the most recent economic and financial crisis. These figures are much lower than those for the US, Finland and Sweden where more than 70% of displaced workers get re-employed one year after.

ers. The number of firms to which they are directly connected is also slightly larger. Among these successful displaced workers, only 10 percent of them had a former coworker already employed in the new firm.

We also report key descriptive statistics of the new jobs found by displaced workers at most one year after displacement. On average, they receive a marginally higher real hourly wages in the new firm compared to wages the closing firm (0.9% higher), and the growth rate of real hourly wages of those who remain employed three years after is 5.6%. In addition, roughly 50% of these workers remain employed after three years, but only 37% are still with the same employer. Finally, we observe that 40% of these individuals started the new job under an open-ended contract, and slightly more than 50% of them are offered an open-ended contract three years after being hired.

Table 2.1: Summary statistics: Displaced workers

	Mean	Sd
Panel A: All		
Female	0.46	
Age	37.5	9.25
Elementary education	0.73	
High-School	0.18	
University	0.09	
Blue-collar occ.	0.56	
Tenure at closing firm	7.65	7.99
Real hourly wage (euros) at closing firm	5.43	4.92
Open-ended contract	0.76	
Hired in t+1	0.21	
Network		
Coworkers per displaced worker	59.7	80.0
Employed coworkers per displaced worker	8.20	18.1
All connected firms per displaced worker	44.4	87.4
Direct connected firms per displaced worker	4.22	7.11
No. workers		166,796
Panel B: Hired in t+1		
Female	0.39	
Age	35.2	8.67
Elementary education	0.72	
High-school	0.19	
University	0.09	
Blue-collar occ. at closing firm	0.58	
Tenure at closing firm	5.43	5.90
Real hourly wage (euros) at closing firm	5.07	4.48
Open-ended contract at closing firm	0.70	
Network		
Coworker present in new firm	0.10	
Coworkers per displaced worker	59.7	83.4
Employed coworkers per worker	10.5	21.8
All connected firms per worker	50.3	100.1
Direct connected firms per worker	4.87	8.10
Reemployment Outcomes		
Blue-collar occ. in new firm	0.57	
Real hourly wage (euros) in new firm	5.12	4.36
Open-ended contract (OEC) in new firm	0.43	
Stayer-three years after hiring	0.37	
Employed-three years after hiring	0.55	
Wage growth-three years after hiring	0.056	0.27
OEC-three years after hiring	0.51	
No. workers		34,484

Former coworkers. In Table 2.2, we switch the focus to the former coworkers of the displaced workers. Panel A of Table 2.2 reveals that former coworkers are mainly male, slightly older, and better educated than displaced workers. Among all former coworkers, 74% are employed both at the displacement moment and the following year in the same firm. These employed coworkers (described in Panel B) receive higher hourly wages than displaced workers (almost 20% higher) and are mainly blue-collar workers on open-ended contracts.

With respect the network characteristics, former coworkers are linked to 4.39 displaced workers (Table 2.2, Panel A). Namely, a former coworker could potentially share information about job opportunities or refer to her current employer around 4 displaced workers. Panel B of Table 2.2 shows that this figure is slightly lower for employed former coworkers, meaning they would need to share information or refer less former colleagues. Moreover, employed coworkers are linked to less than 2 closing firms. This suggests that most of their former colleagues are displaced from the same closing event.

Table 2.2: Summary statistics: Former coworkers

	Mean	Sd
Panel A: All		
Female	0.38	
Age	37.9	8.31
Elementary education	0.70	
High-School	0.19	
University	0.11	
Employed in t+1	0.74	
Network		
Displaced workers per coworker	4.39	10.7
No. workers		132,287
Panel B: Employed in t+1		
Female	0.38	
Age	38.3	8.27
Elementary education	0.70	
Highschool	0.19	
University	0.12	
Tenure	7.82	7.35
Real hourly wage (euros)	6.46	5.74
Blue-collar occ.	0.54	
Open-ended contract	0.80	
Network		
Displaced workers per coworker	3.59	9.14
Closing firms per coworker	1.73	1.33
No. workers		97,542

Closing firms. Panel A of Table 2.3 summarizes the characteristics of the 19,071 closing firms in our estimation sample. These firms have been, on average, for 12 years in business and, at the closing moment, that had 13 employees. More than half of these firms operate in services sectors, but have a larger representation in construction than the connected of hiring firms (Panel B and C of Table 2.3, respectively). Closing firms are mainly located in the North and Lisbon and both connected and hiring firms have a similar distribution over the Portuguese territory. On average, a closing firm is linked to 78 former coworkers through their displaced workers and to 9.78 firms through coworkers of displaced workers.

Connected firms. Panel B of Table 2.3 looks at the characteristics of the 58,717 connected firms in our final sample. These employers have been more time in business compared to closing firms and have around 29 employees. The network information indicates that almost 7 employees of the average connected firm might serve as referrals or inform displaced colleagues about job opportunities at their firm. Furthermore, on average, there are 126 displaced workers and 3.18 closing firms per connected firm. This suggests that each displaced worker would (potentially) compete with other 125 displaced workers to get a position in the same connected firm.

Hiring firms. Panel C of Table 2.3 contains characteristics of the firms that hired at least one displaced worker during the sample period. Roughly 51% of these firms operate in the service sector, 27% in manufacturing and 22% in construction. These firms are larger than the average connected or closing firm, employing on average 48 workers by the time of the displacement event. Importantly, there are around 11 former coworkers present in these firms, which is almost the double than in the average connected firm. On average, these firms are also connected to more displaced workers and closing firms than connected firms.

Table 2.3: Summary statistics: Firms

	Mean	Sd
Panel A: Closing Firms		
Age	12.2	12.8
Size (closing moment)	13.1	37.9
Manufacturing	0.26	
Construction	0.24	
Services	0.51	
Region		
North	0.39	
Algarve	0.05	
Centre	0.20	
Lisbon	0.31	
Alentejo	0.06	
Network		
Coworkers per firm	78.4	202.0
Connected firms per firm	9.78	22.0
No. firms		19,071
Panel B: Connected Firms		
Age	14.3	19.3
Size	29.1	164.9
Manufacturing	0.25	
Construction	0.18	
Services	0.57	
Region		
North	0.39	
Algarve	0.05	
Centre	0.21	
Lisbon	0.30	
Alentejo	0.05	
Network		
Displaced workers per firm	126.2	411.3
Coworkers per firm	6.73	21.0
Closing firms per firm	3.18	8.95
No. firms		58,717
Panel C: Hiring Firms		
Age	12.2	17.9
Size (closing moment)	48.0	262.9
Size (hiring moment)	55.3	272.6
Manufacturing	0.27	
Construction	0.22	
Services	0.51	
Region		
North	0.40	
Algarve	0.05	
Centre	0.23	
Lisbon	0.28	
Alentejo	0.05	
Network		
Displaced workers per firm	189.5	687.1
Coworkers per firm	11.3	33.2
Closing firms per firm	4.80	16.2
No. firms		16,511

2.4 Hiring probabilities of displaced workers

In this section, we present our analysis of hiring probabilities of connected and non-connected workers. We first outline our identification strategy to study to what extent social contacts in the form of past coworkers affect hiring probabilities of displaced workers, and then discuss our main findings.

2.4.1 Empirical strategy

We are interested in checking whether having at least a former coworker in a given firm impacts the probability that a worker will be hired by that firm, compared to a similar worker who does not have such a link to the same firm. Thus, our set-up needs to account for the counterfactual probability that a displaced worker would have been hired by a firm where her former coworker is, even if the former coworker was not employed there. We then rely on firm deaths to identify (plausible) exogenously displaced workers to investigate their re-hiring probabilities and exploit *co-displaced workers* as conterfactuals, in line with Saygin et al. (2019) and Eliason et al. (2019).[14]

We specify the following regression model for the probability that (connected) firm h hires worker i who was displaced due to the closure of firm k

$$Y_{i,k(i),h} = \gamma_{k(i),h} + \beta C_{i,k(i),h} + X_i\Omega + \epsilon_{i,k(i),h}$$

where $Y_{i,k(i),h} = 1$ if worker i displaced from firm k is hired by firm h. $\gamma_{k(i),h}$ are closing-hiring firm fixed effects to account for potential unobserved factors that may lead workers from closing firm k to be more likely to move to firm h for reasons other than the presence of a former coworker. $C_{i,k(i),h}$ is an indicator capturing whether at least one former coworker of displaced worker i is employed in firm h at both displacement moment and hiring moment.

[14]Kramarz and Skans (2014) use a similar approach to analyze school-to-work transitions of young workers in Sweden. Their analysis focuses on whether having a parent in a given firm increases the probability for a young worker to get her first job in that firm, using former classmates as counterfactuals for the lack of ties in the hiring firm.

Our main parameter of interest is β, which measures how much more (or less) likely is a firm h to hire a displaced worker who has a direct connection in the firm through a former coworker than someone else from the same closing firm who lacks a direct tie to firm h. X_i represent observed worker characteristics. In our main specification, we do not include observed characteristics of displaced workers to avoid capturing part of the effect of the actual connection. In other words, we do not account for characteristics that may not be easily observed by new employers. The information problem faced by firms when searching for new employees is one of the main theoretical arguments stressed in the literature on why employers prefer to hire workers with a connection in the firm, as they reduce informational asymmetries (Montgomery, 1991; Galenianos, 2013; Dustmann et al., 2016; Glitz and Vejlin, 2019). However, we show that including workers' personal traits does not affect our results.

To implement our empirical strategy, we use the linked employer–employee dataset and we organize observations in the form of pairs of displaced workers i from closing firm k and connected hiring firm h. As explained before, connected hiring employers are those firms where a displaced worker could potentially find a job either because she has a former coworker employed there or because a displaced worker from the same closing event has a former coworker employed there.[15] The inclusion of closing-hiring firm fixed effects implies that the main parameter of interest β is identified by comparing two workers displaced from the same closing firm k where one of them has a connection through a former coworker employed in firm h whereas the other does not. Then, only variation in direct connections ($C_{i,k(i),h}$) to firm h among individuals displaced from the same closing firm k contribute to identify the effect of interest. Consequently, observations such that all displaced workers from firm k are directly connected to firm h through former coworkers do not contribute to identify the effect of interest. Then, given our research

[15]Notice that this definition of potential hiring firms excludes employers with no connection to closing firm k. However, given our identification strategy, these firms do not contribute to identify the role of former coworkers on the differential hiring probability between workers with and without a link to the hiring firm.

design and to keep the data manageable, we restrict the analysis to pairs of closing firm k and hiring firm h such that one, or several, but not all of the displaced workers i from closing firm k are connected to firm h through a former coworker.[16]

2.4.2 Are connected workers more likely to be hired?

Table 2.4 reports the results of the hiring analysis with the coefficients and standard errors multiplied by 100, so that they can be interpreted as percentages. We present the results for different specifications based on the inclusion of alternative fixed effects. Our benchmark specification is reported in Column 4, where we control for closing-connected firm fixed effects. Alternatively, we present results for the specifications without fixed effects (Column 1), connected firm fixed effects (Column 2) or connected firm-year fixed effects (Column 3), as well as extending our benchmark specification including worker characteristics that can be easily observed by the potential employer.

The results in Table 2.4 Column 4 show that the baseline probability of a displaced worker being hired by a connected firm within a year after displacement is equal to 0.06. In the case of connected workers, this baseline probability increases by 0.15 percentage points. This implies that displaced workers are 2.4 times more likely to be hired by an employer where a former coworker is employed relative to other displaced workers from the same firm closure event but who do not have such a tie to the firm.[17]

The comparison across specifications without and with different sets of fixed effects suggests that there are some unobserved factors, such as firm-specific skills, that may lead workers from a given closing event to be more likely to move to the same firm regardless of the presence of a former coworker. Interestingly, when extending our benchmark specification to include work-

[16] Including closing-hiring firm pairs with no variation in $C_{i,k(i),h}$ affects marginally the baseline hiring probability, but the conclusions remain intact.

[17] Our point estimate is smaller than the effect found by Eliason et al. (2019) in Sweden, but larger than that documented by Saygin et al. (2019) in Austria, even though the relative importance is similar.

ers' observed characteristics, our coefficient of interest barely changes. Thus, this evidence supports the decision of relying of closing-hiring firm pairs fixed effects to capture all factors that can affect worker mobility patterns beyond the personal connection and, hence, could bias our estimates.

Table 2.4: Probability of being hired by a connected firm

	No FE	Hiring Firm FE	Hiring Firm-Year FE	Hiring-Closing Firm FE	Worker Controls
Former Coworker Link	0.3419***	0.3171***	0.2353***	**0.1551*** **	0.1560***
	(0.0217)	(0.0216)	(0.0160)	**(0.0126)**	(0.0126)
High School Education					-0.0087***
					(0.0032)
University Education					-0.0060
					(0.0038)
Age 20-35					0.0195***
					(0.0029)
Female					-0.0106***
					(0.0034)
Blue Collar Occ.					0.0098***
					(0.0036)
Constant	0.0463***	0.0487***	0.0564***	**0.0641*** **	0.0578***
	(0.0055)	(0.0038)	(0.0029)	**(0.0012)**	(0.0028)
Adjusted R^2	0.001	0.067	0.142	0.217	0.217
No. fixed effects	-	58,717	102,376	186,569	186,569
No. observations	7,408,537	7,408,537	7,408,537	7,408,537	7,408,537

Notes: All coefficients and standard errors are multiplied by 100. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We now study the heterogeneity of our results with respect to worker and firm characteristics. We begin by looking at the heterogeneity with respect to observed characteristics of the displaced workers (Table 2.5). The first four columns display the results from different regressions where we include one worker characteristic and its interaction with the coworker link separately, whereas the last column introduces all the covariates and their interactions at the same time. Our findings highlight that females are less likely to be hired by a connected firm relative to males, which could be driven by differences in their network characteristics (Lindenlaub and Prummer, 2018). In terms of education level, we find that low educated individuals benefit more from having a connection in the hiring firm compared to individuals with high-school education, while the effect is not significantly different between low and university educated workers. We also find differences across age groups, namely younger individuals (aged 20-35) have higher returns to direct links to connected firms, which can be rationalized by the fact that in our setting older workers are less likely to have employer former coworkers, despite having a larger network. Finally, blue-collar workers seem to benefit more from having a direct link to the connected firm than white-collar workers. Overall our results are in line with previous findings for the Portuguese economy on the use of informal search methods across demographic groups (Addison and Portugal, 2002) and, more generally, in the literature on the use of social contacts in the labor market indicating that job-search methods are more intensively used by individuals with low socioeconomic status (Topa, 2011).[18]

[18]Our results from the heterogeneity analysis are also in line with the findings of Eliason et al. (2019) for Sweden with respect to the hiring probability of low educated workers and gender. However, our results are at odds with Saygin et al. (2019) who find no gender differences, and that more-highly qualified and older workers benefit the most from work-related networks in the Austrian economy.

Table 2.5: Heterogeneous effects of the probability of being hired by a connected firm

	Female	Education	Age	Occupation	All
Former Coworker Link	0.1905***	0.1707***	0.1165***	0.1347***	0.1535***
	(0.0163)	(0.0139)	(0.0141)	(0.0170)	(0.0253)
Female	-0.0038				-0.0032
	(0.0030)				(0.0030)
Link x Female	-0.0843***				-0.0819***
	(0.0174)				(0.0177)
High School Education		-0.0000			-0.0000
		(0.0000)			(0.0000)
Link x High School		-0.0669***			-0.0724***
		(0.0198)			(0.0220)
University Education		-0.0000			-0.0000
		(0.0000)			(0.0000)
Link x University		-0.0308			-0.0343
		(0.0272)			(0.0306)
Age 20-35			0.0104***		0.0107***
			(0.0021)		(0.0021)
Link x Age 20-35			0.0847***		0.0971***
			(0.0157)		(0.0160)
Blue Collar Occ.				0.0083***	0.0075**
				(0.0032)	(0.0032)
Link x Blue Collar				0.0385**	0.0162
				(0.0180)	(0.0212)
Constant	0.0661***	0.0647***	0.0597***	0.0597***	0.0581***
	(0.0018)	(0.0014)	(0.0014)	(0.0020)	(0.0027)
Adjusted R^2	0.217	0.217	0.217	0.217	0.217
No. fixed effects	186,569	186,569	186,569	186,569	186,569
No. observations	7,408,537	7,408,537	7,408,537	7,408,537	7,408,537

Notes: All coefficients and standard errors are multiplied by 100. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We next investigate two dimensions of the firm heterogeneity. Firstly, we study differences in hiring probabilities according to the size of the firm closure event. Table 2.6 displays the results based on the number of workers displaced in the closure event. Firm closures might have an impact on the local labor market and its magnitude is likely to be correlated with the size of the closure event. On the one hand, it can reduce competition on the product market and create growth opportunities for firms in the market (Cestone et al., 2018), which would affect the causal interpretation of our results. On the other hand, it could depress the local labor market (Gathmann et al., 2018), which would impact hiring probabilities through the competition for fewer jobs. Our findings indicate that the main effect of interest, as well as the hiring probability, becomes smaller with the size of the workforce displaced. The results suggests that our main findings are not driven by any of these channels in a critical way.

Table 2.6: Probability of being hired by a connected firm by the size of the closure event

	Size<10	Size [10-50]	Size>50
Former Coworker Link	0.2837***	0.1692***	0.1025***
	(0.0274)	(0.0190)	(0.0189)
Constant	0.2556***	0.1238***	0.0473***
	(0.0097)	(0.0042)	(0.0012)
Adjusted R^2	0.280	0.157	0.177
No. fixed effects	68,215	70,966	47,388
No. observations	239,165	1,049,499	6,119,873

Notes: All coefficients and standard errors are multiplied by 100. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, we look at the heterogeneity with respect to the characteristics of the connected firms in Table 2.7. We distinguish between the size, age, sector, and location of the connected firm in the year of the closure event. With respect to the size of the connected firm, we do not find relevant differences between size bins. Heterogeneity in the time that a firm has been in operation indicates that more mature firms tend to rely less on current employees when hiring new workers, which can be due to having more formal hiring practices typically in the form of a human resources department. We find interesting evidence depending on whether the closing and the connected firm operate in the same sector.[19] The results show that the effect of a direct link to the firm on the probability of being hired is larger for within industry movements. However, note that the baseline probability for within industry mobility is 10 times that of between industry mobility. Thus, the relative importance of a direct connection is larger for between industry movements, as directly connected workers are almost 5 times more likely to be hired by a firm in a different sector compared to workers without a direct link. This finding represents further evidence that reduced competition in the product market is not driving our results, otherwise we would find a nearly zero effect on the inter-industry mobility. Additionally, this higher (relative) importance of direct links for between industry movements suggests that former coworkers may reveal information to their employers about the quality of the potential new employees, easing job mobility that one might expect to be more difficult in the absence of this information. Regarding whether the closing and connected firms are located in the same region, the effect of a direct link is somewhat smaller, suggesting that former coworker ties may ease regional mobility by mitigating information asymmetries.

[19]We consider that closing and connected firms operate in the same sector if they share the same section of the Portuguese Classification of Economic Activities (revision 2.1). See Appendix 2.A for a detailed description of the sector of activity variable.

Table 2.7: Probability of being hired by heterogeneous connected firms

	Size<10	Size [10-50]	Size>50	Age<5	Age [5-10]	Age>10	Same Sector	Different Sector	Same Region	Different Region
Former Coworker Link	0.1783***	0.2041***	0.1934***	0.1879***	0.1667***	0.1396***	0.2492***	0.0808***	0.1669***	0.1253***
	(0.0191)	(0.0227)	(0.0156)	(0.0287)	(0.0260)	(0.0164)	(0.0250)	(0.0093)	(0.0152)	(0.0207)
Constant	0.0344***	0.0424***	0.0390***	0.0834***	0.0581***	0.0601***	0.1527***	0.0169***	0.0768***	0.0404***
	(0.0018)	(0.0022)	(0.0015)	(0.0028)	(0.0025)	(0.0015)	(0.0033)	(0.0007)	(0.0017)	(0.0014)
Adjusted R^2	0.396	0.249	0.304	0.295	0.230	0.176	0.217	0.201	0.225	0.183
No. fixed effects	45,397	60,882	106,279	36,465	43,234	106,870	75,604	110,965	133,033	53,536
No. observations	1,687,986	2,296,310	3,984,296	1,409,388	1,687,228	4,311,921	2,539,803	4,868,734	4,779,071	2,629,466

Notes: All coefficients and standard errors are multiplied by 100. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.5 Re-employment outcomes of displaced workers

So far, our findings suggest that employed former coworkers provide an advantage to connected displaced workers in terms of higher hiring probabilities. We now look at re-employment outcomes of workers who found a job in a firm where a former coworker is, relative to hired non-connected workers. Our goal in this section is twofold. Firstly, we are interested in studying whether connected workers are better off in terms of earnings and employment perspectives than hired non-connected displaced workers. Secondly, we seek to shed light on the possibility that employers rely on their current employees when hiring in order to reduce information asymmetries with respect to (unobserved) quality of the match, by exploiting the use of temporary vs permanent contracts when hiring.

2.5.1 Econometric model

To explore these issues, we look at entry-level wages and the starting type of contract, as well as the following labor market outcomes after three years: in terms of wage growth, employment perspectives, job stability, and conversion of fixed-term contracts. Given our interest in comparing these outcomes both between and within hiring firms, we estimate two complementary regression models similar to Kramarz and Skans (2014):

$$Y_{i,k(i),h} = \beta C_{i,k(i),h} + X_{i,k(i),h}\Omega + Z_h\Gamma + \alpha_{k(i)} + \epsilon_{i,k(i),h} \tag{2.1}$$

$$Y_{i,k(i),h} = \beta C_{i,k(i),h} + X_{i,k(i),h}\Omega + W_{k(i)}\Upsilon + \phi_h + \epsilon_{i,k(i),h} \tag{2.2}$$

$Y_{i,k(i),h}$ stands for our outcome variables for worker i displaced from firm k and hired by firm h on year after displacement. The main variable of interest is $C_{i,k(i),h}$, which indicates whether a displaced worker i displaced from closing firm k is hired by a firm h where a former coworker was already present. Model 2.1 compares labor market outcomes of workers displaced by the same closing event with and without a direct link through a former coworker to the new employer. For this end, we include closing plant fixed effects $\alpha_{k(i)}$ and control for worker characteristics as well as the new occupation of worker i and the observed characteristics of the hiring firm h (size, age, sector of activity, and location). In Model 2.2, we instead include hiring plant fixed effects ϕ_h and we control for worker characteristics and the occupation in the new firm h as well as the characteristics of the closing firm k (size, age, sector of activity, and location). In this specification, we compare outcomes of workers with and without connections who were hired by the same firm h.

2.5.2 The value of a former coworker in the new firm

Table 2.8 presents the results of our analysis of re-employment outcomes of displaced workers who found a job within a year after displacement. We look at both the moment of hiring and three years after re-employment to shed light on the short- and medium term outcomes.

Findings in the first column of Table 2.8 indicate that workers with a connection have higher entry-level wages compared to non-connected workers. The starting wage premium becomes slightly larger when including hiring firm fixed effects instead of closing firm fixed effects (3.8 vs 2.9 percent, respectively). Thus, within firms, displaced workers with a former coworker present in the firm tend to earn higher wages relative to displaced workers who had no such connection. Interestingly, our results highlight that connected workers are also more likely to start their job under a permanent contract. Again, the overall effect is modest (4 percentage points) and it increases to 4.8 percentage points when accounting for hiring firm fixed effects. The comparison between specifications using either closing or hiring firm fixed effects thus suggests that displaced workers who presumably found their job through a former coworker tend to be hired by plants that in general pay lower wages and are more likely to offer a temporary contract to displaced workers. Thus, conditional on the idea that employers rely on temporary contracts as a screening device (Varejao and Portugal, 2005; Portugal and Varejao, 2009; Faccini, 2014), our results would imply that the adverse selection problem could be less of a concern when hiring connected workers.

Turning to medium-term outcomes and comparing workers displaced by the same closing event, we find that connected workers are more likely to remain employed (3 percentage points), and this effect is particularly strong for the probability of remaining in the same firm (6.4 percentage points). Interestingly, within firms, we find no differences between connected and non-connected workers on the probability of remaining employed after three years. This latter finding could be related to the fact that when comparing workers within the same firm, differences in worker turnover between connected and non-connected workers die out over time as learning occurs (Galenianos, 2013; Glitz and Vejlin, 2019).

Now we compare wage growth and the probability of getting a temporary contract in the sample of displaced workers who stay in the hiring firm after three years, to shed light on the role of former coworkers as a source of information for their employers.[20] With respect to the estimates of the wage growth regressions we find a negative, although non-significant, effect be-

[20]In Table 2.B4 in Appendix 2.B we present the results of wage growth and the probability of having a permanent contract after three years for all displaced workers who remain employed regardless of the firm.

tween connected and non-connected individuals suggesting that initial differences might dissipate over time as learning occurs. In terms of the probability of conversion of a temporary into a more stable employment contract after three years in the firm, we also find no significant differences between connected and non-connected individuals. This latter result is consistent with the idea that, if former coworkers act as referrals, they should help to mitigate initial uncertainty about match quality. Then, conditional on entering on a temporary contract, we should not expect relevant differences between connected and non-connected displaced workers after the match is formed, as both parties have the opportunity to learn about the quality of the match.

Overall, our estimates indicate that displaced workers benefit from having a connection in the hiring firm, as they have higher initial earnings and enjoy greater job security both by being more likely to receive a permanent contract at the hiring moment and being matched with firms that offer greater employment stability in the medium-term. This suggests that former coworkers may help to alleviate the costs of job displacement that arise from multiple job losses after re-employment (Stevens, 1997; Eliason and Storrie, 2006). Moreover, our findings provide support to the relevance of the referral channel. Namely, employers seem to rely on personal connections as a screening mechanism (referrals) to reduce uncertainty about match quality and initial differences die out over time as learning occurs (Simon and Warner, 1992; Galenianos, 2013; Dustmann et al., 2016; Saygin et al., 2019; Glitz and Vejlin, 2019).

Table 2.8: Short- and medium-term outcomes for displaced workers

	Entry		Three years after			
	ln(Hourly wage)	Perm. contract	Employed	Same firm	Δln(Hourly wage)	Contract conversion
Closing firm fixed effects	0.0275**	0.0401***	0.0301***	0.0619***	-0.0150	0.0111
	(0.0101)	(0.0140)	(0.0124)	(0.0138)	(0.0131)	(0.0382)
Hiring firm fixed effects	0.0371**	0.0475***	0.0115	0.0264	-0.0161	-0.0356
	(0.0151)	(0.0163)	(0.0180)	(0.0186)	(0.0203)	(0.0392)
No. workers	34,484	34,484	34,484	34,484	10,805	6,716

Notes: ln(Hourly wage) is a linear regression specification for entry level hourly wages. Perm. contract stands for a linear probability model for the likelihood of starting the job under a permanent contract compared to a temporary contract. Employed and Same firm specifications are linear probability models for the likelihood a worker is still employed or employed in the same, respectively, three years after being hired. Δln(Hourly wage) estimates the three year change in hourly wages for workers staying in the same firm. Contract conv. is a linear probability model for the likelihood workers who were under a temporary contract and have a permanent contract three years after being hired in the same firm. Column 1 and 3 to 5 include as controls a quadratic polynomial in worker age, indicators for education level (high-school and university, omitted category: elementary education.), females, permanent contracts, and blue-collar occupation. Column 2 and 6 includes as controls a quadratic polynomial in worker age, indicators for education level, females, and blue collar occupation. Closing firm fixed effects specifications include as additional controls the following characteristics of the hiring firm. Hiring firm fixed effects model include as controls the characteristics of the closing firm. Firm characteristics include a quadratic polynomial of degree two in (log) size, firms' age, and indicators for broad industry (construction and services; omitted category manufacturing) and location of the firm (4 regions; omitted category northern region). Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.6 Conclusion

This chapter investigates the impact of personal connections on the labor market outcomes of displaced workers. We implement our analysis on the Portuguese economy, a two-tier labor market characterized by low worker mobility and high long-term unemployment. We show that social contacts make displaced workers more likely to be hired within a year after displacement and that they benefit from having a personal connection in the hiring firm as they find better paid and more stable jobs.

The relevance of our results extends beyond the quantification of the role of social contacts in the labor market on increasing hiring probabilities or improving the labor market outcomes of displaced workers. We uncover a new channel through which personal connections help displaced workers to improve their re-employment perspectives after displacement. More precisely, we show that connected workers are more likely to receive a permanent contract upon re-employment. From a worker perspective, our findings suggest that personal connections may help displaced workers to be better matched with their new employer and, hence, mitigate an important source of earnings losses after displacement in Portugal, i.e. poorer new matches (Raposo et al., 2015). From an employer perspective, our results provide support to the relevance of the referral channel through which former coworkers help to alleviate information asymmetries in the labor market and ease the learning process about match quality (Galenianos, 2013; Glitz and Vejlin, 2019), restraining employers from using alternative screening devices like fixed-term contracts. Therefore, our analysis also adds to the debate on whether fixed-term contracts are mainly used as screening devices (Portugal and Varejao, 2009) or as an employment buffer (Centeno and Novo, 2012). Specifically, our results suggest that employers may be using temporary contracts as screening devices to hire new workers, when they lack alternative mechanisms to reduce the initial uncertainty about match quality.

Altogether our findings indicate that social contacts play a decisive role in the labor market by reducing information asymmetries and helping displaced workers to find new jobs. Yet, if former coworkers are a source of information about the quality of new employees, we should expect larger and more significant effects for stronger personal connections. Therefore, further research will be oriented to investigate how the current results are shaped by the strength of the relationship between displaced workers and their former coworkers (and their employers).

Appendix 2.A Variables definition

Worker age and gender. Obtained from the workers' files. In case a time inconsistency on the workers' year of birth or gender is found, we replace them by the value reported for more than 50% of the observations for that worker, similarly to Cardoso (2006).

Education. Corresponds to the highest level of education completed by the worker. We aggregate this variable into three levels: i) elementary education (less than 12 years of education completed); ii) high school education and iii) university education (including polytechnic degrees (*Bacharelatos*) and bachelor's, master's and PhD degrees). If there is any inconsistency, i.e., the educational attainment in the current year is lower than that in the previous year, we replace it by one of the contiguous observation when possible or by the mode of the education level for that worker.

Occupation. We rely on the Portuguese Classification of Occupations to create occupation categories. Blue-collar occupations include the following 1-digit codes: 6-Farmers and skilled agricultural, fishery and forestry workers; 7-Craftsman; 8-Plant and machine operators; and 9-Unskilled workers. White-collar occupations include: 1-Directors and executive managers; 2-Intellectual and scientific activities specialists; 3-Technicians and associate professionals; 4-Clerical support workers; 5-Salespersons.

Hourly wages. Wage is computed as the sum of base wages, regular and overtime payments divided by normal and overtime hours worked. Wages are deflated using the Consumer Price Index (base 2012).

Tenure. We calculate tenure using the reported date of admission of the worker (month-year pair). If the date of admission is missing in a given year, either we replace it by the previous (or following) admission date reported, provided that it is prior to the reference moment of the data, or if no information is provided we rely on the year we first observe the worker in the firm.

Open-ended contract. Indicator variable that identifies employment contracts with no predetermined duration. The indicator takes value zero for workers on fixed-term contracts, temporary agency workers or in case the contract type is not applicable or ignored.

Sector of activity. Main sector of activity according to the Portuguese classification of economic activities (revision 2.1). We further aggregate this classification into three levels: i) Manufacturing (extractive industries, manufacturing and electricity production and distribution and water supply), ii) Construction, and iii) Services (wholesale and retail, lodging and restaurants, transport, financial activities, property, public administration, education, health and social work and collective, social and personal services).

Firm age. We use the founding year to compute firm's age. When the founding year is inconsistent over time, we replace it by the mode. If the mode is inconsistent with the first year in which the firm reports information, we replace it by the year of admission of the most tenured worker at the firm. If this information is not consistent or is not available, we use the first year in which we observe the firm in the data.

Firm size. Total number of employees working at the firm during the reference period.

Location. We divide the location of the firm into five categories —North, Algarve, Centre, Lisbon and Alentejo— according to the second level of the Nomenclature of Territorial Units for Statistics (version 2013).

Appendix 2.B Supplementary tables and figures

Table 2.B1: Probability of being hired by a connected firm (1993-2010)

	No FE	Hiring Firm FE	Hiring Firm-Year FE	Hiring-Closing Firm FE	Worker Controls
Former Coworker Link	0.3235***	0.3081***	0.2222***	0.1534***	0.1547***
	(0.0165)	(0.0166)	(0.0125)	(0.0101)	(0.0100)
High School Education					-0.0098***
					(0.0030)
University Education					-0.0054
					(0.0037)
Age 20-35					0.0227***
					(0.0027)
Female					-0.0126***
					(0.0031)
Blue Collar Occ.					0.0124***
					(0.0032)
Constant	0.0500***	0.0517***	0.0610***	0.0685***	0.0596***
	(0.0048)	(0.0034)	(0.0025)	(0.0011)	(0.0026)
Adjusted R^2	0.001	0.053	0.131	0.200	0.200
No. fixed effects		79297	156527	268332	268332
No. observations	9792557	9792557	9792557	9792557	9792557

Notes: All coefficients and standard errors are multiplied by 100. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.B2: Short- and medium-term outcomes: Full specification with closing firm FE

	Entry		Three years after			
	ln(Hourly wage)	Perm. contract	Employed	Same firm	Δln(Hourly wage)	Contract conv.
Former Coworker Link	0.0290***	0.0401***	0.0275**	0.0619***	-0.0150	0.0110
	(0.0101)	(0.0140)	(0.0124)	(0.0138)	(0.0131)	(0.0382)
Worker's age	0.0244***	0.0061**	0.0598***	0.0411***	-0.0056*	0.0044
	(0.0021)	(0.0027)	(0.0035)	(0.0033)	(0.0032)	(0.0086)
Worker's age^2	-0.0003***	-0.0000	-0.0009***	-0.0006***	0.0001	-0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Female	-0.1963***	-0.0094	-0.0419***	-0.0241***	0.0004	0.0113
	(0.0082)	(0.0094)	(0.0094)	(0.0089)	(0.0078)	(0.0252)
Education (omitted: elementary)						
High School Education	0.0903***	0.0111	-0.0068	0.0025	0.0201**	-0.0147
	(0.0080)	(0.0097)	(0.0096)	(0.0096)	(0.0096)	(0.0227)
University Education	0.4555***	0.0982***	-0.0077	-0.0092	0.0327**	0.0768**
	(0.0183)	(0.0145)	(0.0140)	(0.0140)	(0.0132)	(0.0362)
Open-ended contract	0.0613***		0.0563***	0.0550***	-0.0042	
	(0.0080)		(0.0103)	(0.0098)	(0.0087)	
Blue-Collar Occ.	-0.1828***	-0.0451***	-0.0091	-0.0331***	-0.0022	-0.0439
	(0.0130)	(0.0111)	(0.0099)	(0.0120)	(0.0096)	(0.0267)
(log) Firm's Size	0.0734***	-0.1162***	0.0190**	0.0227**	-0.0054	0.0257
	(0.0059)	(0.0090)	(0.0086)	(0.0089)	(0.0105)	(0.0348)
(log) Firm's Size^2	-0.0071***	0.0092***	-0.0009	-0.0027***	0.0006	-0.0012
	(0.0007)	(0.0010)	(0.0009)	(0.0010)	(0.0012)	(0.0045)
Firm's Age	0.0003	-0.0006*	0.0012***	0.0021***	-0.0001	0.0015***
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0005)
Sector (omitted: manufacturing)						
Construction	-0.0102	-0.0354*	-0.0891***	-0.1094***	-0.0093	-0.0811
	(0.0146)	(0.0215)	(0.0167)	(0.0171)	(0.0189)	(0.0501)
Services	-0.0490***	-0.0617***	-0.0249*	-0.0805***	0.0007	-0.0289
	(0.0118)	(0.0178)	(0.0134)	(0.0146)	(0.0127)	(0.0367)
Region (omitted: northern region)						
Algarve	0.0063	-0.1915***	-0.0204	-0.0817**	-0.0180	-0.1993
	(0.0281)	(0.0372)	(0.0402)	(0.0411)	(0.0420)	(0.1267)
Centre	0.0178	-0.0335	-0.0073	0.0342	0.0097	0.0202
	(0.0155)	(0.0272)	(0.0227)	(0.0265)	(0.0202)	(0.0618)
Lisbon	0.0334***	-0.0535**	-0.0380**	-0.0397**	-0.0043	-0.0581
	(0.0127)	(0.0212)	(0.0157)	(0.0175)	(0.0191)	(0.0592)
Alentejo	-0.0114	0.0099	-0.0128	-0.0354	0.0712**	0.0110
	(0.0236)	(0.0351)	(0.0291)	(0.0314)	(0.0312)	(0.1249)
Constant	0.9236***	0.6276***	-0.4398***	-0.3143***	0.1733***	0.4809***
	(0.0412)	(0.0552)	(0.0673)	(0.0646)	(0.0629)	(0.1674)
Adjusted R^2	0.591	0.413	0.183	0.212	0.286	0.361
No. workers	34,484	34,484	34,484	34,484	10,805	6,716

Notes: ln(Hourly wage) is a linear regression specification for entry level hourly wages. Perm. contract stands for a linear probability model for the likelihood of starting the job under a permanent contract compared to a temporary contract. Employed and Same firm specifications are linear probability models for the likelihood a worker is still employed or employed in the same, respectively, three years after being hired. Δln(Hourly wage) estimates the three year change in hourly wages for workers staying in the same firm. Contract conv. is a linear probability model for the likelihood workers who were under a temporary contract and have a permanent contract three years after being hired in the same firm. Firm characteristics correspond to the hiring firm. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.B3: Short- and medium-term outcomes: Full specification with hiring firm FE

	Entry		Three years after			
	ln(Hourly wage)	Perm. contract	Employed	Same firm	Δln(Hourly wage)	Contract conv.
Former Coworker Link	0.0371**	0.0475***	0.0116	0.0264	-0.0161	-0.0356
	(0.0151)	(0.0164)	(0.0180)	(0.0186)	(0.0203)	(0.0392)
Worker's age	0.0233***	0.0044	0.0570***	0.0368***	-0.0030	0.0093
	(0.0027)	(0.0031)	(0.0053)	(0.0046)	(0.0044)	(0.0092)
Worker's age^2	-0.0002***	-0.0000	-0.0008***	-0.0005***	0.0000	-0.0001
	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Female	-0.1654***	0.0083	-0.0264*	-0.0016	0.0068	0.0071
	(0.0105)	(0.0116)	(0.0141)	(0.0123)	(0.0120)	(0.0260)
Education (omitted: elementary)						
High School Education	0.0817***	0.0051	-0.0167	-0.0048	0.0041	0.0097
	(0.0105)	(0.0111)	(0.0155)	(0.0131)	(0.0153)	(0.0266)
University Education	0.4371***	0.0885***	-0.0367*	-0.0425**	0.0119	0.0576
	(0.0255)	(0.0188)	(0.0209)	(0.0194)	(0.0199)	(0.0513)
Open-ended contract	0.0978***		0.0703***	0.0753***	-0.0032	
	(0.0177)		(0.0212)	(0.0196)	(0.0156)	
Blue-Collar Occ.	-0.1985***	-0.0578***	-0.0319**	-0.0270*	-0.0091	-0.0153
	(0.0217)	(0.0183)	(0.0158)	(0.0153)	(0.0162)	(0.0326)
(log) Firm's Size	0.0082	-0.0445**	-0.0333	-0.0460**	-0.0031	-0.0372
	(0.0136)	(0.0223)	(0.0205)	(0.0183)	(0.0269)	(0.0498)
(log) Firm's Size^2	-0.0010	0.0072**	0.0052*	0.0063**	0.0006	0.0052
	(0.0019)	(0.0034)	(0.0030)	(0.0025)	(0.0040)	(0.0068)
Firm's Age	0.0006	-0.0003	-0.0002	-0.0001	-0.0006	-0.0003
	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0011)
Sector (omitted: manufacturing)						
Construction	0.0110	-0.0056	-0.0187	0.0021	-0.0160	-0.0021
	(0.0174)	(0.0179)	(0.0272)	(0.0222)	(0.0284)	(0.0655)
Services	0.0117	0.0337*	-0.0199	0.0036	-0.0111	0.0202
	(0.0170)	(0.0176)	(0.0220)	(0.0199)	(0.0229)	(0.0495)
Region (omitted: northern region)						
Algarve	-0.0025	0.0428	0.0117	0.0342	-0.0183	0.0526
	(0.0335)	(0.0477)	(0.0558)	(0.0443)	(0.0608)	(0.1409)
Centre	-0.0230	0.0067	0.0137	-0.0086	0.0201	-0.0241
	(0.0176)	(0.0233)	(0.0285)	(0.0249)	(0.0264)	(0.0677)
Lisbon	0.0176	0.0160	-0.0095	-0.0490**	0.0038	-0.0055
	(0.0180)	(0.0238)	(0.0240)	(0.0244)	(0.0254)	(0.0492)
Alentejo	0.0412	-0.0041	-0.0176	-0.0354	-0.0234	0.0229
	(0.0261)	(0.0303)	(0.0386)	(0.0360)	(0.0359)	(0.0989)
Constant	1.0271***	0.3647***	-0.3003***	-0.1851**	0.1459*	0.4519**
	(0.0581)	(0.0642)	(0.1006)	(0.0925)	(0.0824)	(0.1860)
Adjusted R^2	0.684	0.707	0.257	0.372	0.419	0.683
No. workers	34,484	34,484	34,484	34,484	10,805	6,716

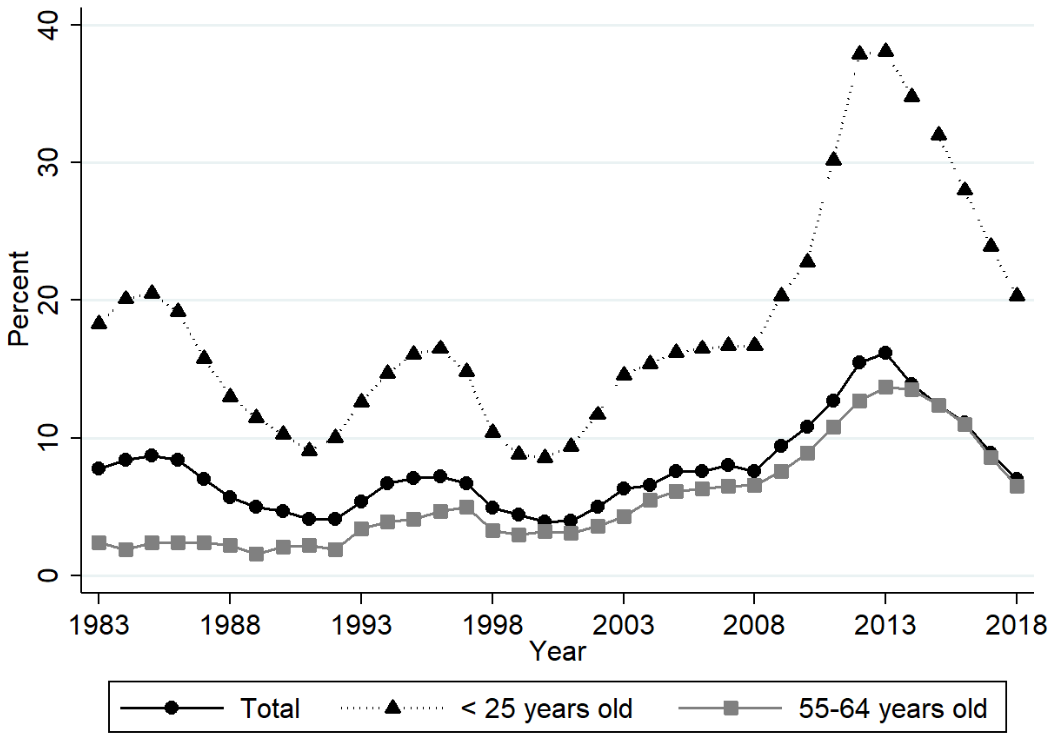
Notes: ln(Hourly wage) is a linear regression specification for entry level hourly wages. Perm. contract stands for a linear probability model for the likelihood of starting the job under a permanent contract compared to a temporary contract. Employed and Same firm specifications are linear probability models for the likelihood a worker is still employed or employed in the same, respectively, three years after being hired. Δln(Hourly wage) estimates the three year change in hourly wages for workers staying in the same firm. Contract conv. is a linear probability model for the likelihood workers who were under a temporary contract and have a permanent contract three years after being hired in the same firm. Firm characteristics correspond to the closing firm. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.B4: Wage growth and permanent contract after three years for all workers

	Closing firm FE		Hiring firm FE	
	Δln(Hourly wage)	Perm. contract	Δln(Hourly wage)	Perm. contract
Former Coworker Link	-0.0160	0.0145	-0.0207	-0.0525
	(0.0114)	(0.0284)	(0.0157)	(0.0407)
Worker's age	-0.0021	0.0113*	-0.0056	0.0130
	(0.0033)	(0.0068)	(0.0046)	(0.0094)
Worker's age^2	0.0000	-0.0002*	0.0001	-0.0002
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Female	-0.0093	-0.0084	-0.0105	0.0030
	(0.0076)	(0.0217)	(0.0115)	(0.0281)
Education (omitted: elementary)				
High School Education	0.0260***	-0.0101	0.0157	-0.0089
	(0.0093)	(0.0181)	(0.0138)	(0.0259)
University Education	0.0552***	0.0733**	0.0373*	0.0783*
	(0.0131)	(0.0297)	(0.0213)	(0.0443)
Open-ended contract	-0.0060		-0.0008	
	(0.0083)		(0.0143)	
Blue-Collar Occ.	0.0138	-0.0366*	0.0034	0.0074
	(0.0096)	(0.0191)	(0.0156)	(0.0285)
(log) Firm's Size	-0.0218	-0.0219	-0.0051	0.0254
	(0.0202)	(0.0452)	(0.0082)	(0.0250)
(log) Firm's Size^2	0.0034	0.0017	0.0005	-0.0025
	(0.0028)	(0.0064)	(0.0009)	(0.0031)
Firm's Age	0.0001	0.0003	-0.0001	0.0018***
	(0.0005)	(0.0011)	(0.0002)	(0.0004)
Sector (omitted: manufacturing)				
Construction	-0.0045	-0.0243	-0.0172	-0.1009***
	(0.0270)	(0.0481)	(0.0190)	(0.0322)
Services	-0.0246	0.0118	0.0189	-0.0291
	(0.0192)	(0.0398)	(0.0121)	(0.0264)
Region (omitted: northern region)				
Algarve	0.0044	-0.0601	0.0064	-0.1846**
	(0.0507)	(0.1058)	(0.0547)	(0.0832)
Centre	0.0533**	-0.0232	-0.0316	-0.0090
	(0.0239)	(0.0526)	(0.0206)	(0.0434)
Lisbon	0.0358	-0.0587	-0.0130	-0.0489
	(0.0223)	(0.0443)	(0.0147)	(0.0394)
Alentejo	-0.0057	-0.0645	-0.0037	0.0304
	(0.0356)	(0.0871)	(0.0291)	(0.0786)
Constant	0.1336**	0.3239**	0.1969**	0.3530*
	(0.0623)	(0.1319)	(0.0842)	(0.1862)
Adjusted R^2	0.154	0.260	0.298	0.443
No. workers	16,367	10,466	16,367	10,466

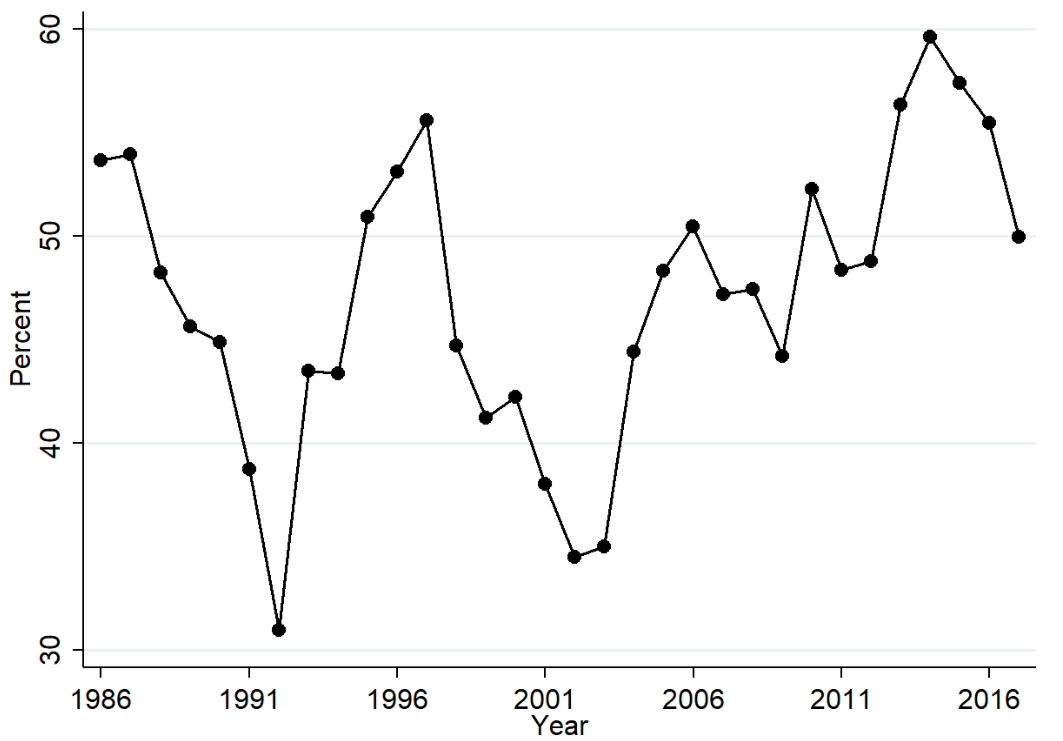
Notes: The table presents the medium term outcomes in terms of wage growth and the probability of having a permanent contract for all workers, regardless the establishment where they are employed after three years. ln(Hourly wage) is a linear regression specification for entry level hourly wages. Perm. contract stands for a linear probability model for the likelihood that worker re-employed under a temporary contract hold a permanent contract three years after. Closing firm fixed effects models include characteristics of the hiring firm, whereas hiring firm fixed effect regressions control for the traits of the closing firm. Standard errors (in parentheses) are clustered at the closing firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.B1: Unemployment rate, 1983-2018



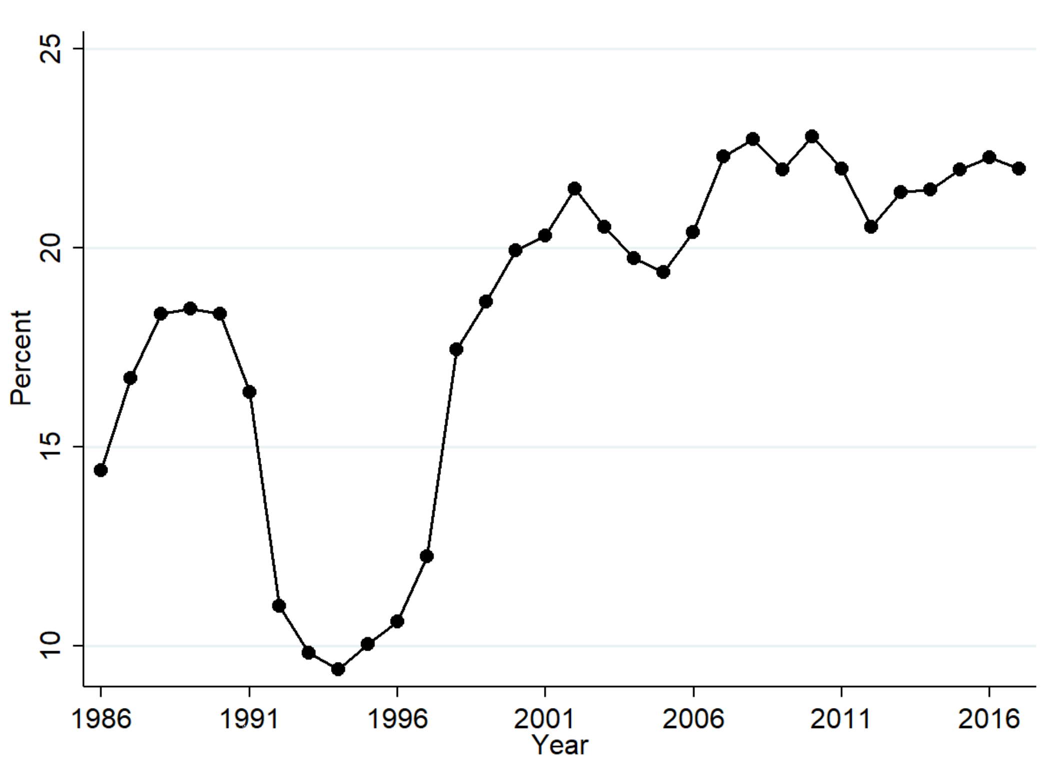
Note: Unemployed people out of active population.
Source: *Pordata*.

Figure 2.B2: Long-term unemployment rate, 1986-2016



Notes: Long-term unemployment refers to people who have been unemployed for 12 months or more.
Source: *OECD*.

Figure 2.B3: Temporary employment rate, 1983-2018



Notes: Temporary employment includes wage and salary workers whose job has a pre-determined termination date.
Source: *OECD*.

Chapter 3

Employment Resilience in Worker-Owned Firms: Evidence from Spain

3.1 Introduction

Unlike capital-owned firms, where the rights to choose the leadership of the organization and to participate in its profits are based on capital contributions, in worker-owned enterprises working partners own and control the business. Different arrangements in terms of ownership and control lead to differences in economic behavior. Namely, worker-owned organizations seek to fulfill their social goal and meet the needs of their members, not just to make profits for shareholders (Perotin, 2013). In worker-owned firms, partners are then willing to make their income and work effort (e.g. hours worked) more flexible in order to protect their jobs, i.e. their income source, which translates into greater employment stability compared to workers in mainstream business (Pencavel, 2013).

Worker-owned firms are also allowed to hire external labor under the same conditions as capitalist firms do. In this environment, there is no a clear outcome regarding whether worker-owned firms will extend employment guarantees to hired-workers. On the one hand, daily workplace interactions might enhance reciprocity and internal solidarity between partners and employees, extending employment security to conventional employees within worker-owned firms (Bowles and Gintis, 2002). The social nature of worker-owned firms may increase workers' engagement and cooperation with management (owners) that can enhance productivity and lower the need to lay-off workers. Similarly, it may reduce bargaining costs within the organization and boost workers' willingness to adjust labor conditions in exchange of lower risk of job loss. On the other hand, allowing worker-owned firms to employ non-partner workers may simply lead worker-owned firms to act like capitalist enterprises (Ben-Ner, 1984). That is, the employment of hired-workers in worker-owned firms will respond as in conventional capitalist enterprises.

In this chapter, I investigate whether employment guarantees of partners of worker-owned firms extend to conventional employees and, if so, whether employment stability differences between organizations can be related to a more intense adjustment in earnings and/or working-time over the business cycle. In particular, I analyze cyclicality differences in terms of employment, labor income, and working-time between similar workers employed either by capital- or worker-owned firms (WFs and CFs, hereafter) in the Spanish economy between 2005 and 2016. To perform my analysis, I rely on rich administrative data that allows me to unambiguously identify both capital- and worker-owned firms along with their (hired) workers.

The particular focus on comparing conventional employees in CFs and WFs is motivated by the attention that social economy businesses have been subject to by policymakers in the European Union as a way of achieving the goals of the Europe 2020 Strategy, which aims to "create the conditions for a more competitive, sustainable and inclusive economy with more and better conditions of employment" (European Commission, 2010, 2011). This possibility has been also suggested by international organizations that point to the ability of worker-owned firms to provide good quality and stable jobs (Levin, 2002; Roelants et al., 2012; Teasdale and Buckingham, 2013). Therefore, comparing similar employees in both type of organizations turns out to be necessary to understand to what extent social economy businesses can be viewed as a source of more stable employment.

The analysis leads to the following insights. Firstly, with respect to the employment guarantees of worker-owned firms, my results indicate that conventional employees of worker-owned firms enjoy greater employment stability over the business cycle compared to similar workers hired by conventional enterprises. Secondly, the parallel analysis of labor income and working-time of workers employed in WFs relative to employees of CFs indicates that the volatility of these margins is similar between both type of organizations. The findings can be rationalized by the presence of labor regulations and differences in the objectives of both types of organizations. While both firms are constrained by the same regulations on the adjustments that can impose on wages and working hours, the social nature of worker-owned firms mitigates employment volatility in these organizations

Most of the existing literature has focused on the comparative analysis of wage-earners in CFs and working-partners in WFs in terms of employment and wages (Craig and Pencavel, 1993, 1992; Pencavel and Craig, 1994; Pencavel et al., 2006; Burdin and Dean, 2009, 2012), I focus on conventional workers, i.e. wage-earners, employed either by CFs and WFs. Closely related to my empirical analysis are Burdin and Dean (2009), Alves, Burdin and Dean (2016), and Kurtulus and Kruse (2018), who study employment adjustment differences between CFs and WFs by looking at comparable workers in those organizations. Burdin and Dean (2009) investigate differences in employment and wage responses between CFs and WFs in Uruguay between 1996 and 2006. They compare both working-partners and hired workers in WFs with employees in CFs. The comparison between conventional employees in both type of organizations point to no significant differences in terms of wage adjustment to changes in the environment, but greater employment stability. Alves et al. (2016) study job flows differences between CFs and WFs in Uruguay for the period 1996-2009. Their findings point to lower employment growth in WFs than in CFs. In addition, they show that for a given amount of net employment growth WFs create and destroy a significantly lower number of jobs than CFs, but this is driven by partners. This latter finding indicates that job flows for wage-earners in WFs are even higher than those observed for their twins working in CFs. This result implies that WFs might achieve low employment volatility for partners at the expense of hired workers. Kurtulus and Kruse (2018) analyze employment volatility in a different institutional framework. Specifically, they examine the relationship between employee ownership programs and employment stability in the US between 1999 and 2011. Their results show that firms that introduced employee ownership programs exhibit greater employment stability in the face of economy-wide and firm-specific shocks.

This study complements existing literature in several dimensions. Firstly, I perform the analysis in Spain, one of the countries with the largest share of cooperative employment to total population in the developed world, which has shown a high commitment to the promotion of social economy organizations through the Ley 5/2011 for the Social Economy. In addition, I analyze a full economic cycle of the Spanish economy 2005-2016, making the country an appealing case to test cyclical differences between CFs and WFs. Secondly, the dataset used in the analysis provides detailed information on each job a worker held with either CFs or WFs along with associated earnings, days and hours worked. Thus, I am able to compare not only employment and wage cyclicality differences between CFs and WFs, but also to perform a detailed analysis on working-time cyclicality. Thirdly, most of the existing literature relies on firm-level data to study employment and wage cyclicality differences, with little (or no) information on workers. A large literature on wage cyclicality highlights that workforce composition differences over the business cycle can induce a substantial counter-cyclical bias (Solon et al., 1994; Carneiro et al., 2012; De La Roca, 2014). Therefore, changes in the composition of the workforce should be taken into account. In this regard, the dataset at hand allows me to control both for worker and job observed characteristics, but also for worker-level unobserved heterogeneity.

The remainder of the chapter is organized as follows: Section 3.2 briefly overviews the theoretical work on the comparison between CFs and WFs. Section 3.3 explains the institutional setting for worker-owned firms in Spain. Section 3.4 describes the dataset and the estimation sample. Section 3.5 presents aggregate evidence on employment, earnings, and working-time in CFs and WFs, whereas Section 3.6 outlines the empirical strategy to compare the responses of employment, earnings, and working-time over the business cycle. The main findings are discussed in Section 3.7 and Section 3.8 concludes.

3.2 Theoretical background

The theoretical literature analyzing differences between CFs and WFs in terms of employment, wages, and short-run responses to changes in the economic environment dates back several decades (see the overviews by Bonin et al., 1993; Pencavel, 2013). The theoretical discussion finds its origins in the seminal work by Ward (1958). His model considers that WFs maximize revenue per worker rather than total profits as their capitalist twins, and labor (working partners) is the sole input. In this context, the model points that WFs exhibit a perverse short-run behavior. In particular, it predicts that in the face of an increase in the output price, WFs reduce employment and output in order to increase revenue per worker. That is, unlike a standard profit maximizing firm, WFs would exhibit a short-run supply curve with a negative slope.

Scholars have showed that Ward's predictions are not robust to mild theoretical variations such as incorporating other inputs in the production function (Bonin and Fyjkuda, 1986; Pencavel et al., 2006), allowing workers to adjust hours worked (Ireland and Law, 1981), using employment maximization as an alternative to income maximization (Kahana and Nitzan, 1989), or a more realistic treatment of membership that does not miss the collective nature of the firm. In contrast to Ward's model, these richer frameworks predict that WFs do not alter employment levels to the same extent as CFs in the face of a shock. This is because in worker-owned firms, shocks will have a first-order effect on each partners' income that will temper employment responses.[1]

Some theoretical models have also allowed for the possibility that WFs can hire and fire workers at market wages as traditional capitalist firms. Ben-Ner (1984) shows that successful WFs where partners' income is higher than market wages would progressively degenerate into CFs. That is, WFs would replace retired partners with conventional employees in order to increase partners' remuneration. Ward's model can be extended to incorporate hired employees into WFs (Pencavel, 2013). In this framework when hired employees perform tasks that are similar to those performed by partners, an improvement in the WFs product market will lead to an increase in the employment of hired employees and a reduction in the number of partners. Alternatively, when the tasks performed by hired workers and partners are complements the degeneration may not take place. Then, the whether (and how) WFs are able to extend their employment guarantees to hired workers would depend on the specific institutional setting.

3.3 Worker-owned firms in Spain

A worker-owned firm, or more specifically a cooperative, is defined as "an autonomous association of persons united voluntarily to meet their common economic, social, and cultural needs and aspirations through a jointly-owned and democratically-controlled enterprise" according to the International Labor Organization (Levin, 2002). The key elements differentiating worker-owned and mainstream capital firms are control and ownership. In conventional corporations, the rights to choose the leadership of the organi-

[1]Empirical research has tested the behavioral response in terms of employment and income of working-partners in WFs versus workers in CFs with respect to changes in the environment either in a neoclassical framekwork (Craig and Pencavel, 1993; Burdin and Dean, 2012) or taking as given the Ward's income maximization hypothesis (Craig and Pencavel, 1992; Pencavel and Craig, 1994; Pencavel et al., 2006; Burdin and Dean, 2009), Altogether, existing evidence suggests that employment tend to be less volatile in WFs compared to CFs, at the expense of income.

zation and to participate on its profits are based on capital contributions. In a worker-owned enterprise, working partners both own and control the business and each partner has one vote in the election of the board of directors and shares in the profits on the basis of an equitable formula settled in the company bylaws.

Worker-owned firms belong to what is known as the social economy that embeds all enterprises that aim at making profits for people other than shareholders. The Spanish social economy is currently ruled by the Ley 5/2011 on social economy. The Law defines social economy as the set of business and economic activities performed by institutions in the private sector, that seek general economic or social interest, or both, and in accordance with the following principles:

- Priority of the people and the social objective over capital. This is established by means of an autonomous, transparent, democratic and participatory management that prioritizes decision-making based on the people and their contribution to the work and services carried out for the institution or its social objective over their contribution to share capital.

- Turnover obtained from economic activity is mainly applied according to the work contributed and the service or activity carried out by the institution's partners or members and to the institution's end social objective.

- Encouraging internal solidarity and social solidarity that favors a commitment to local development, equal opportunities for men and women, social cohesion, the integration of persons at the risk of social exclusion, generating stable and quality employment, conciliation of personal and professional life and sustainability.

- Independence from the public authorities.

The Law considers the following institutions as a part of the social economy: cooperatives, labor societies, mutual societies, foundations and associations engaged in an economic activity, special employment centers, fishermen's associations, agricultural processing companies and special organizations created by specific rules that are governed by the principles above mentioned.

Two major types of worker-owned firms exist in Spain: cooperatives (*sociedades cooperativas*) and labor societies (*sociedades laborales*). These two type of firms share the common feature of being mainly owned by their working partners, that is, they control more than 51 percent of capital. However, there are key elements that differentiate them:

i. Restrictions to hire non-partner workers. In cooperatives, wage-earners cannot work more than 30 percent of the total hours worked by cooperative members during a year, while for labor societies this limit is set at 49 percent.[2]

ii. Minimum capital requirement. Labor societies are regulated by the same legislation as conventional corporations. The legislation stipulates that firms must deposit a minimum level of capital to set up the organization of 3,000 euros in case of limited liability labor societies, and 60,000 euros for limited liability companies that are publicly traded. In the case of cooperative businesses, there is no legal capital minimum requirement, but partners must stipulate the amount of the initial capital in the company bylaws.

iii. Capital ownership. In cooperatives, the owners of the firm are the working partners but the firm bylaws may establish the possibility of accepting collaborative partners whose total contributions cannot exceed 45 percent of the social capital. In the case of labor societies, there are two types of partners: working partners who own at least 51 percent of the capital, and capitalist partners who can have a share in the firm that cannot exceed 49

[2]In 2015, the Spanish government passed a new law for labor societies that, among other things, it increased the maximum limit of working time for wage-earners. Before the reform, the limit was 15 percent.

percent of the capital.

iv. Voting power of partners. The voting power in labor societies depends positively on the amount of capital provided by each member, while in cooperatives each member has the same voting power. If there exist collaborative partners in the cooperative, they cannot represent more than 30 percent of the votes in the company governing bodies.

v. Corporate taxation: Labor societies are taxed as any other type of for-profit business in Spain (25 percent of the profits). In the case of cooperatives, the corporate tax rate is lower regarding cooperative profits (20 percent).[3]

In summary, while cooperatives satisfy the definition used in the literature to analyze worker-owned firms, labor societies are a halfway between capitalist- and worker-owned firm. Therefore, this study solely considers cooperatives as worker-owned firms.[4] Furthermore, in 2017 the Spanish Government launched the Strategy for the Social Economy 2017-2020. This strategy established a wide set of incentives for creating new organizations as well as hiring and retaining workers in existing worker-owned firms. Thus, to avoid the impact of this law on the analysis of cyclical differences between worker- and capital-owned firms, the year 2017 is not considered.[5]

[3]Before 2016, the corporate tax rate for conventional companies was 30 percent. In the case of cooperatives, the tax rate was not changed but there was a distinction between the tax rate for cooperative and non-cooperative profits. Cooperative profits were taxed at 20 percent, while non-cooperative profits —those obtained from business activities that do not correspond to the purpose of the organization— were taxed at the general rate (30 percent).

[4]Throughout the analysis worker-owned firms and cooperatives will be used indistinctly.

[5]The impact of this new institutional setting will be worth investigating in future work, a few years after its implementation.

3.4 Data, employer categories, outcome variables, and estimation sample

The main data source is the Spanish Continuous Sample of Employment Histories (*Muestra Continua de Vidas Laborales* or MCVL), an administrative dataset collected annually by the Spanish Social Security administration since 2005 up to 2017.[6] The MCVL is a representative 4 percent random sample of individuals who had any relationship with the Social Security system at any time in the reference year.[7] Each MCVL wave is typically extracted in March/April of the year following the reference period.

The MCVL has a longitudinal design, since an individual present in a year who subsequently remains registered with the Social Security administration stays as a sample member. Additionally, since the dataset is refreshed each year, it remains representative of both the stock and flows of individuals in the Social Security system.[8]

For each sample member, the MCVL retrieves all relationships with the Social Security system since the date of the first job spell, or 1967 for earlier entrants.[9] All job spells are observed along with exact starting and ending dates, labor market status and, if employed, job and employer characteristics along with labor income. Individual traits are also such as gender, date and location of birth, nationality, and level of education are also observed.[10]

[6] The first version of the MCVL corresponds to 2004. This wave is discarded as most of the information clearly differs from that available for subsequent years. In addition, to avoid the impact of the Strategy for the Social Economy 2017-2020 on the analysis of cyclical differences between worker- and capital-owned firms, the year 2017 is not considered.

[7] This includes employed and self-employed workers, recipients of unemployment benefits and pension earners, but excludes individuals registered only as medical care recipients, or those with a different social assistance system (civil servants, such as the armed forces or the judicial power).

[8] Individuals who stop working remain in the sample while they receive unemployment benefits or other welfare benefits (e.g. retirement pension). Then, individuals leave the sample when they pass away or leave the country permanently. Likewise, each wave adds individuals who enter the labor market for the first time.

[9] Since 1980 including information on earnings.

[10] Appendix 3.A provides a detailed description of the main variables.

3.4.1 Employer categories

Two levels of employer identifiers are observed: plant and firm. The plant identifier refers to the Social Security contribution account.[11] The second identifier is based on tax IDs and is common to all plants within a firm. A crucial feature of the MCVL for the purpose of this analysis is that it also provides information on the legal setting of the firm, which is determined by firm-specific tax IDs. The first element of these IDs stipulates the legal status of the firm in terms of corporate taxation. The database also contains information on the specific type of the plant that identify certain type of employers that exhibit a particular relationship with the Social Security administration regarding contribution payments as well as information on the type of labor relationship between worker and employer.[12] I use these sources of information on firm ownership to create the two mutually exclusive categories, as follows.[13]

Capital-owned firms (CFs). Identified according to the following criteria. Firstly, I rely on information on the legal structure coming from tax IDs to select limited liability, joint-stock companies, and foreign entities (first digit of the tax ID: A, B, N and W). Secondly, capitalist and labor societies are under the same regulation for corporate taxation, implying the tax IDs do not allow to distinguish between them. Then, I use the type of the plant to separate capitalist firms (code 9999) from labor societies (5180).

[11] According to the Social Security administration, around 85 percent of the firms are single unit organizations, i.e. there have just one contribution account per firm. Each firm has one account for each treble province-Social Security regime-type of employment relation. Thus, the Social Security Administration identifies within a province different groups of employees of a given firm. By restricting the sample to standard labor relationships (e.g. traditional wage-employment workers) and the General Regime of the Social Security (e.g. no peculiarities in welfare entitlements) as will be described in Section 3.4.3, contribution accounts can be thought of as establishments.

[12] The type of plant is not reported in 2005 and, hence, it is recovered using the longitudinal design.

[13] Organizations that exhibit any inconsistency between the variables used to define the employer categories are removed.

Worker-owned firms (WFs). Coded in a similar way. I use tax IDs to select cooperatives (first digit F), which are the type of organization fulfilling all the international standards to qualify under the worker-owned firms enterprises. Additionally, I hinge on the type of labor relationship between worker and employer to isolate partners (code 930) from wage-earners employees in worker-owned firms.[14]

3.4.2 Outcome variables

Employment. The MCVL includes longitudinal records of the plants where the randomly selected workers have ever been employed. This study considers as the unit of analysis the plant since employer information (creation date, plant-size, sector of activity, and location) is observed at that level of disaggregation.[15] For each of these plants, plant-level employment stock is observed annually at the data extraction moment.[16]

Working-time. In the analysis of working-time differences, I look both at the extensive margin, i.e. days worked, and the intensive margin, i.e. hours worked. Number of days worked is computed exploiting the difference between ending and starting dates of each job spell held in a year and, then, aggregating over the year. Information on hours is provided in the MCVL as a continuous variable measuring the percentage of hours an employee works relative to a full-time worker in the same organization. Thus, I use this continuous variable as a proxy for the amount of hours worked. If, within a year, a

[14]In Spain, working partners of cooperatives are allowed to choose between self-employment contribution regime or the General Regime. According to national statistics, the ratio of cooperative self-employment to total cooperative employment is around 26 percent. Unfortunately, the dataset does not allow to link cooperative partners registered as self-employed to their specific establishment, implying that partners enrolled in the General regime are a selected sample and they will be excluded from the analysis.

[15]Throughout the analysis employer, firm and plant will be used indistinctly.

[16]If the plant is no longer in operation, the observed plant-size is zero. However, there are some cases in which plant size observations exhibit holes. Specifically, I observe the plant in $y-1$ and $y+1$, but not in y. In such cases, I recover plant size at y, by linearly interpolating employment stock between $y-1$ and $y+1$. In addition, I recover plant-size for 2005 from the 2004 file.

worker experienced a change in that variable within the job spell or held more than one job with the same employer, the percentage of hours worked is then calculated as the average of the percentage of hours worked weighted by the days worked under each regime in a given year.

Labor income. The MCVL reports labor income from two different sources: Social Security contribution bases and income tax records reported by employers.[17] Social Security contribution bases capture gross monthly labor earnings plus one-twelfth of year bonuses.[18] Earnings are bottom and top-coded. The minimum and maximum caps vary by Social Security regime and contribution group, and they are adjusted each year according to the evolution of the minimum wage and inflation rate.[19] Tax records come from the "Annual summary of retentions and payments for the personal income tax on earnings, economic activities, awards and income imputations" (model 190). This model is filled by employers and it includes all the amount of individual compensation paid to employees in a given year. This information is available for all employees, whether or not they were exempted from paying the labor income tax. For each worker-plant match, the labor income outcomes used in the analysis are total annual earnings and full-time equivalent daily wages in a given year.[20] Annual earnings are computed as the sum of all earnings received by an individual from a given employer in a year. Full-time equivalent daily wages are calculated as the ratio of total annual earnings over total days worked in a year times the share of hours worked. Both labor income measures are deflated using the Consumer Price Index and are expressed in Euros of 2016.

[17]Tax records are not available for the Basque Country and Navarra, as taxes are collected independently from the Spain's national government.

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

[19]Censored earnings are corrrected by fitting cell-by-cell Tobit models to log real daily wages. See Appendix 3.B for a detailed explanation on the imputation method.

[20]In the main analysis, I rely on censoring-corrected earnings coming from the Social Security contribution bases, but I present sensitivity tests excluding censored observations or either using censored earnings or tax records (Appendix 3.C Table 3.C10).

3.4.3 Estimation sample

Using the definitions for each employer category, I select CFs and WFs and apply the following constraints to create an annual panel of plants and workers for the period 2005-2016.[21] The initial sample consists of 761,143 plants satisfying the legal definition of CFs and WFs.

To ensure that I capture actual establishments and no special contribution accounts within a firm, I exclude contribution accounts of Special Regimes of the Social Security system that typically cover the primary sector and household activities, which have a distinctive labor regulation (44,873 plants dropped).[22] Similarly, I remove from the sample Social Security accounts that identify workers employed under non standard labor relationships (29,443).[23] From these plants, I keep only workers aged 20-60 who have at least one job spell with complete information on earnings and working-time between 2005 and 2016 (187,738 plants excluded).[24] Finally, I exclude years when individuals have worked less than a month or earned less than one-twelfth of the national minimum wage (33,476 plants excluded). These restrictions produce a final sample of 691,730 workers observed over 465,613 plants between 2005 and 2016.

The final sample contains 458,497 capital-owned plants observed on average for almost 7 years, and 7,116 worker-owned organizations present in the sample for almost 8 years.[25] Conventional enterprises are typically larger

[21] As discussed in Section 3.3, the year 2017 is excluded from the analysis to avoid the influence of the Strategy for the Social Economy 2017-2020.

[22] Remaining plants in agriculture, fishing, mining and other extraction industries as well as household activities are also removed.

[23] Note that this restriction also excludes Social Security accounts that identify the partners of cooperatives who decided to enroll in the General Regime of Social Security instead to register as self-employed. These exclusion involves around 10% of the Social Security accounts associated with a cooperative employer.

[24] The establishments removed correspond to employers that have positive plant-level employment at some point between 2005 and 2016, but there are no observed on-going job spells during the period. This is consequence of the sample design of the MCVL. Namely, these employers were picked by retrospective job spells that finished before 2005 and, hence, there are no individuals with on-going job spells in those plants. In Table 3.C5 I show that the results hold when considering all the plants.

[25] Appendix 3.C Table 3.C1 reports summary statistics of plants in each employer category.

than cooperatives, but cooperatives tend to be more mature organizations. In terms of sector of activity, cooperatives are more likely to perform manufacturing activities relative to capitalist employers, whereas the opposite is true for the construction sector. Worker-owned organizations are underrepresented in large metropolitan areas.[26]

In terms of workers, the final sample includes 684,027 workers in CFs and 20,793 individuals employed by WFs.[27] Workers in cooperatives are more likely to be female, have Spanish nationality, and hold a university degree. With respect to the labor relationship, employees of CFs earn, on average, higher annual earnings compared to hired workers of WFs. These differences are lower in terms of full-time equivalent daily wages, explained by fewer number of days and hours worked in a year by employees in WFs relative to their twins in CFs. Additionally, the share of workers under temporary contracts is higher in WFs, but duration of the employment relationship is slightly longer.

3.5 Overview of employment, earnings, and working-time in CFs and WFs

Figure 3.1 depicts the evolution of total employment in CFs and WFs. Total employment is computed by aggregating plant-level employment in each of the employer categories by year, which refers to the data extraction moment of the MCVL. The figure highlights the huge volatility of the Spanish labor market as consequence of its particular institutional setting characterized by: (i) downward wage rigidity that prevents wage adjustments, thereby shifting the burden of the labor market adjustment towards the employment margin (Diez-Catalan and Villanueva, 2015; Pajuste and Ruffo, 2019), and (ii) a labor

[26]Madrid, Barcelona, Sevilla, and Valencia, which are populated by over 1 million inhabitants, are considered as large metropolitan areas.

[27]Appendix 3.C Table 3.C2 presents descriptive statistics of workers in each employer category.

market where 30 percent of the workforce is under temporary contracts with low employment protection, and this dual nature of the labor market increases its volatility in terms of employment (Costain et al., 2010; Bentolila et al., 2012).

With respect to the years when the economy was growing, 2005-2008 and 2014-2016, Figure 3.1 suggests that the behavior of WFs and CFs was similar, with employment in both categories growing at similar rates during the expansionary phase of the cycle. The numbers indicate that the differences between these two type of organizations in terms of employment arose during the period of economic crisis, 2008-2013.[28]

Figure 3.1 shows that employment losses were significant and pervasive across employer categories, which highlights the severe effect that the economic slump had on the Spanish labor market with the unemployment rate increasing from 7.9 percent in 2007 to a striking 26.9 percent rate in 2013. The magnitude of employment losses yet differed between CFs and WFs. In the case of CFs, employment contracted by around 20 percent between 2008 and 2013. For WFs, the employment contraction was milder, with roughly a 16 percent reduction between 2008 and 2013. This evidence suggests that WFs were able to extend the employment guarantees of partners to conventional employees. However, the 2008-2013 recessionary period embedded two very different negative shocks, i.e. the bursting of a housing bubble (2008Q2-2009Q4) and the Eurozone (debt) crisis that led to a large-scale banking crisis (2010Q4-2013Q2). The figure reveals interesting differences on the impact of these shocks to CFs and WFs. Specifically, the burst of the housing bubble had a bigger impact on CFs, which lost 13 percent of their workforce between 2008 and 2010, compared to WFs that barely lost 6 percent. In the case of the second phase of the recession sparkled by the Eurozone debt crisis, employment losses were instead larger in WFs compared to CFs, 9 vs 6 percent drop between 2011-2013, respectively. This heterogeneous response to the two

[28]The Spanish Business Cycle Dating Committee dated the first recession from the second quarter of 2008 to the fourth quarter of 2009, and the second recession from the fourth quarter of 2010 to the second quarter of 2013.

shocks can be explained by differences in sectors where these two type of employers perform their activities.[29] Namely, whereas the first recession mostly hit the construction sector and related activities, where cooperatives are underrepresented; the Eurozone crisis affected all sectors and regions in a similar way. Thus, accounting for this heterogeneity on where WFs are located and in which kind of sectors operate relative to CFs is key to properly assess whether cooperatives are able to provide more stable employment for their (hired) workers compared to similar workers in mainstream enterprises.

Figure 3.1: Evolution of total employment, 2005-2016

Notes: Total employment is calculated as the sum of plant-level employment stock each year in capital- and worker-owned firms in the estimation sample. Employment stock refers to the data extraction moment of the MCVL.

Figure 3.2 provides evidence on the evolution of earnings in CFs and WFs. Figure 3.2a shows that annual earnings were significantly lower in WFs compared to CFs, and this gap shrinks when looking at full-time equivalent daily wages. With respect to the evolution over the period under analysis, the figure reveals that both measures were increasing up to around 2010. This finding highlights the delayed response of wages to economic conditions, amplify-

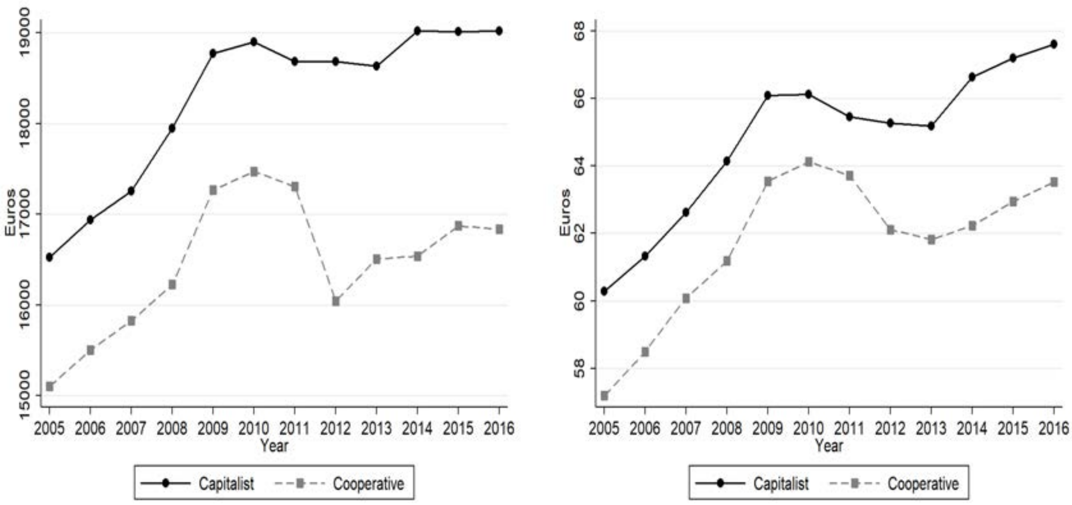
[29] Appendix Figures 3.C1 and 3.C2 depicts the distribution of cooperatives across provinces and sectors, respectively.

ing the effect of negative shocks in the labor market. Namely, the burden of the labor market adjustment in the Spanish economy is through quantities not prices, consequence of the downward wage rigidity (Diez-Catalan and Villanueva, 2015; Pajuste and Ruffo, 2019). After 2010, earnings and wages started to slowly adjust between 2010 and 2013. From 2014 onwards, and fueled by the economic recovery, the figure indicates that both measures start to increase. Interestingly, the evidence unveils a more intense adjustment in cooperatives between 2011 and 2012, the period when these enterprises experienced the larger employment losses. This suggests that part of the drop is explained by the destruction of an important share of the workforce. This is consistent with the evolution of wages, as the magnitude of the adjustment was milder.

Figure 3.2: Labor income in WFs and CFs, 2005-2016

(a) Annual earnings

(b) Full-time equivalent daily wages



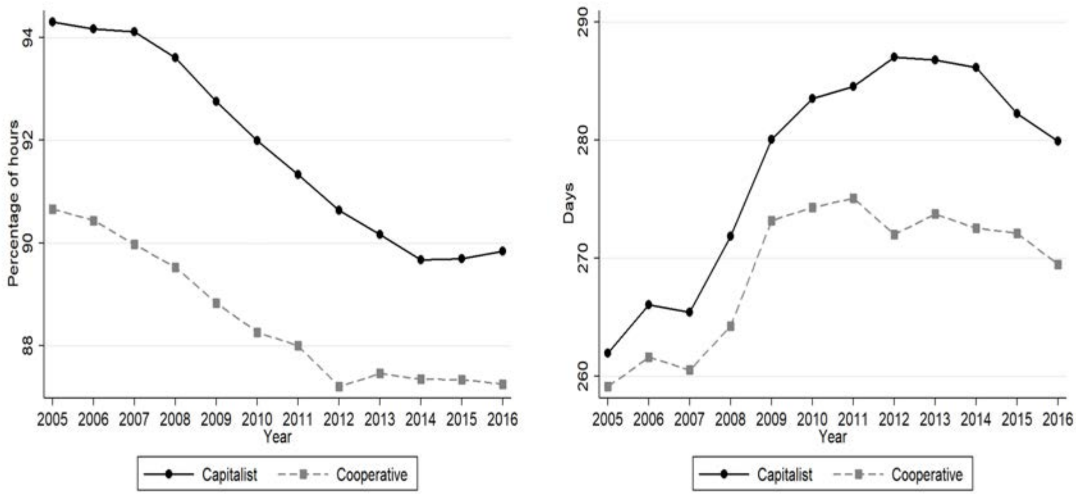
Notes: Annual earnings refer to the average annual labor income earned by employees in each of the employer categories. Full-time equivalent daily wages are calculated as the ratio of total annual earnings over total days worked in a year times the share of hours worked. Earnings and full-time equivalent daily wages are expressed in Euros of 2016.

Figure 3.3 presents evidence on working-time in CFs and WFs. Figure 3.3a shows that employees in WFs tend to work, on average, tend to work fewer hours during the year. The figure also reveals that the adjustment of working hours was more intense in CFs compared to WFs during the recessionary period. With respect to the number of days worked, Figure 3.3b shows that workers in WFs tend to work slightly fewer days, partially explained by a more widespread use of temporary contracts in these organizations. The number of days worked however increased during the recessionary period in both employer categories. This hike in working-days can be understood by the duality of the Spanish labor market where the burden of the employment adjustment tend to fall more heavily on workers under temporary contracts (Costain et al., 2010; Bentolila et al., 2012). Interestingly, the evidence unveils that working-days increased less in WFs. The evidence on working-time seems to indicate that the economic crisis disrupted to a lesser extent employment in WFs.

Altogether the descriptive evidence suggests that overall employment losses were lower in WFs compared to CFs. However, this employment resilience seems not to be associated with a more intense adjustment on the remuneration or the working-time of employees in WFs relative to workers of CFs. Nevertheless, the raw evidence should be taken with caution as workforce composition differences as well as the differential impact of the economic crisis might drive these results. Therefore, the next step is to properly account for the existent heterogeneity between CFs and WFs to evaluate their behavior over the business cycle.

Figure 3.3: Working-time in CFs and WFs, 2005-2016

(a) Working hours **(b)** Working days



Notes: Working hours represent the average percentage of time an employee worked in a year relative to a full-time worker in the same establishment. Working days are the total number of days an individual worked for the same employer during a year.

3.6 Econometric methodology

In this section, I outline the econometric model to analyze the elasticity of employment, labor income, and working-time with respect to aggregate economic conditions, and the differential evolution of these outcome variables for conventional enterprises and cooperatives. My results are based on regression models of the following form:

$$ln(y_{it}) = \beta Coop_{it} + \lambda_1 UR_{t-1} + \lambda_2 UR_{t-1} \times Coop_{it} + X_{it}\Omega + \alpha_1 t + \alpha_2 t^2 + \eta_i + \epsilon_{it}$$

where y_{it} is the outcome variable of interest, e.g. plant-size, worker's total annual earnings, full-time equivalent daily wages, the percentage of hours worked, or total days worked, of unit i (plant or worker) at year t. $Coop_{it}$ is a categorical variable identifying cooperatives, and UR_{t-1} stands for the proxy of aggregate economic conditions, i.e. the national unemployment rate. To limit endogeneity concerns related to the link between the dependent variable(s) and the contemporaneous aggregate conditions, I rely on the previous calendar year to measure the economic situation that triggers changes in the outcome variables. To study the comparative behavior of capitalist firms and cooperatives, I include an interaction term between the cooperative indicator and the proxy for aggregate conditions, $UR_{t-1} \times Coop_{it}$. The main coefficients of interest are thus λ_1 and λ_2. λ_1 is the semi-elasticity of y in conventional enterprises to changes in the unemployment rate, while λ_2 captures the differential response of y with respect to the unemployment rate between cooperatives and capitalist enterprises. X_{it} is a vector of observable characteristics of the unit under analysis (plants or workers) and will be discussed in the next section, and t is a (quadratic) time trend.

Previous findings in the wage cyclicality literature point to composition biases arising from different types of agents (firms or workers) present in distinct phases of the business cycle, which may lead to underestimate the true cyclicality (Solon et al., 1994; Carneiro et al., 2012; De La Roca, 2014).[30] To account for this composition bias, I include individual-level fixed effects (η_i) to capture time-invariant unobserved heterogeneity.

3.7 Cyclicality differences between CFs and WFs

This section is devoted to discuss the main findings on the differences between capital- and worker-owned firms in terms of employment, earnings, and working-time over the business cycle.

3.7.1 Employment

Table 3.1 reports the results of the regression to study plant-level employment cyclicality. The main coefficients of interest are those attached to the one year-lagged national unemployment rate and its interaction with the coopera-

[30]The decision of specifying the model in levels instead of first differences is also motivated by these findings. The goal is to include the entry of new agents, who by definition are not included in two consecutive years and, hence, would be disregarded when differentiating my outcome equation. The results are not affected by this decision.

tive indicator.[31] Controls include also a dummy variable for plants belonging to multi-establishment firms, categorical variables for plant age groups (3) in order to control for employment growth differences between young and mature firms (Haltiwanger et al., 2013), and a quadratic time-trend.[32] In specifications with no plant-fixed effects, I also control for workplace location (50 provinces) and sector of activity (11 sectors).

The results in Table 3.1 are discussed sequentially, including the variables of interest one at a time. Column 1 presents the regression of (log) plant-size on the cooperative indicator, controlling for plant observables. The finding shows that plant-size differences between CFs and WFs are not explained by composition differences. In Column 2, I introduce the proxy for aggregate conditions, which has a negative impact on plant-size pointing to the pro-cyclicality of employer size. Columns 3 and 4 report the results when I interact the unemployment rate with the cooperative dummy, with and without plant fixed-effects, respectively. Column 3 shows that plant-level employment cyclicality in WFs was lower. Column 4 reveals, that when including plant fixed-effects, differences in employment cyclicality between CFs and WFs remain, but plant-size is more pro-cyclical, as composition biases are accounted for. The estimates imply that one percentage point increase in the unemployment rate leads to a 1.41 percent decrease in plant-size of capital-owned establishments, whereas the semi-elasticity for worker-owned establishments is 0.43 percent.

[31] In Appendix Table 3.C4 Columns 2-4, I substitute the national unemployment rate by a nationwide index of economic activity, the province-level unemployment rate, or genuine year dummies and the main findings hold true.

[32] In Appendix 3.C, I re-estimate the model including either a sector-specific quadratic trend or sector-time fixed effects fixed effects (Table 3.C4 Columns 5 and 6, respectively). These specifications lead to somewhat larger employment cyclicality and to smaller differences between capital- and worker-owned firms, especially when using sector-time fixed effects.

Table 3.1: Plant-level employment

	(1)	(2)	(3)	(4)
Cooperative	-0.1086***	-0.1085***	-0.2218***	
	(0.0156)	(0.0156)	(0.0213)	
Unemp. rate		-0.0100***	-0.0102***	-0.0141***
		(0.0001)	(0.0001)	(0.0001)
Unemp. rate x Coop			0.0067***	0.0098***
			(0.0010)	(0.0007)
Plants	465,497	465,497	465,497	465,497
Plant-Year obs.	3,208,406	3,208,406	3,208,406	3,208,406
R-squared	0.1330	0.1335	0.1335	0.8704
Plant FE	No	No	No	Yes

Notes: Dependent variable is the (log) of plant-level employment. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include a quadratic time trend, a dummy for multi-establishment firms, and indicator variables for plant's age (3). Columns 1 to 3 also include indicators for sector of activity (11) and workplace location (50). Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

In Table 3.2, I repeat the analysis of plant-level employment cyclicality excluding some groups that could presumably drive observed differences between employer categories. First, I exclude from the estimation sample establishments that entry or exit the market during the observation period.[33] The results in Column 1 indicate that the startup of new plants or the death of old plants affect the cyclical behavior of employment, but they do not drive the differences between CFs and WFs.[34] Next, I remove plants located in the

[33]To define these dramatic events, I exploit information on the creation date of the plant to identify entries, whereas for exits I rely on the longitudinal dimension of the dataset and classify closing plants as those for which I observe plant-size to collapse to zero and there are no on-going job spells after the zero is observed.

[34]In Table 3.C5 in Appendix B, I also provide empirical evidence on the probability of observing a new plant or the likelihood a plant is last observed in business over the business cycle. In line with existing evidence (Perotin, 2006; Burdin, 2014), my findings highlight a higher entry rate of cooperatives as aggregate conditions worsen and a lower exit rate. Both results suggests that employment resilience of worker-owned firms might also arise from a differently behavior over the business cycle in terms of entry by creating new job opportunities through higher entry, or protecting existing ones by having a higher survival probability.

Basque Country or Navarra, as these regions represent the roots of the cooperative culture in Spain.[35] Column 2 highlights that the employment resilience of worker-owned firms is a nationwide phenomenon. Column 3 restricts the analysis to plants outside the construction sector, as WFs are barely represented in that sector compared to CFs and the aggregate evidence from Section 3.5 suggests that the key differences between WFs and CFs in terms of employment losses over the bussines cycle arose from the impact of the burst of the housing bubble on the economy. Interestingly, the results in Column 3 show that when excluding establishments in the construction sector the differences in employment cyclicality are even larger. In Columns 4 and 5, I analyze whether the disparities in employment cyclicality between WFs and CFs differ by the size or the age of the establishment. The results show that cyclicality differences are still present, but they become larger in establishments that have ever been larger than 25 between 2005 and 2016 and somewhat smaller in mature organizations, i.e. plants that have stayed for more than 5 years in business. Finally, I perform the analysis only on single unit firms, thereby excluding plants that belong to multi-establishment organizations. The results in Column 6 point that differences between CFs and WFs remain within the sample of single unit firms.

The findings indicate that employment of wage-earners in WFs is less volatile over the business cycle relative to CFs. Compared to previous literature on partners, employment is more volatile for wage-earners in WFs than for partners. In this regard, a match partner-firm is expected to be more stable than a match wage-earner-firm. Hence, in worker-owned firms "employment guarantees" for partners are expected to larger than for wage-earners. Yet, the results suggest that the employment resilience of partners of worker-owned firms are to some extent shared by all workforce in those organizations. In the next section, I investigate whether the employment resilience of wage-earners in WFs comes at the expenses of higher volatility of wages or working-time,

[35]The biggest cooperative in Spain, the Mondragón group, was created and perform their main activities in the Basque Country.

as it has been documented to be the margins of adjustment for partners.

Table 3.2: Plant-level employment: Sub-group analysis

	Excl. Entry & Exit	Excl. Navarra & Basque Country	Excl. Construction	Plants' Size >25	Plants' Age >5	Single Unit Firms
Unemp. rate	-0.0121***	-0.0143***	-0.0114***	-0.0157***	-0.0145***	-0.0146***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Unemp. rate x Coop	0.0076***	0.0099***	0.0077***	0.0128***	0.0098***	0.0107***
	(0.0007)	(0.0007)	(0.0007)	(0.0016)	(0.0007)	(0.0007)
Plants	257,739	438,601	368,327	117,793	340,888	400,843
Plant-Year obs.	2,104,731	3,003,973	2,631,548	982,760	2,902,664	2,731,420
R-squared	0.8986	0.8680	0.8905	0.7265	0.8694	0.8529
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the logarithm of plant-level employment. Unemp. rate stands for one-year lagged national unemployment rate. Plant's Size >50 identifies establishment that have ever been larger than 50 between 2005 and 2016. Plants' Age > 10 stands for plants that reach the 10 years in business over the period under analysis. Single unit firms exclude multi-establishment organizations from the sample. Column 1 to 5 specifications include a quadratic time trend, a dummy for multi-establishment firms, and indicator variables for plant's age (3). Column 6 uses the same set of organizations except for the multi-establishment dummy. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

3.7.2 Labor income

Tables 3.3 and 3.4 present the results on the cyclical evolution of labor income in CFs and WFs, where dependent variables are either the logarithm of total real annual earnings or real full-time equivalent daily wages, respectively. The coefficients attached to the national unemployment rate and its interaction with the cooperative indicator represent the estimates of interest, capturing the overall cyclicality and the differential response for cooperatives, respectively.[36] Controls in the worker fixed effects wage regression include a polynomial in age deviations of degree three, a quadratic polynomial in tenure, occupation-skill category (3 groups), a dummy for workers with a temporary contract, (ln) size of the current establishment, employer age categories (3), dummies for sector of activity (11) and workplace location (50 provinces), and a quadratic time trend.[37] In specifications with no worker-fixed effects, I

[36]Notice that wages in Spain are set by collective agreements at least six months to one year in advance, there is a delayed relationship between aggregate conditions and wages. Thus, as in my specification I rely on the one-year lagged unemployment rate to identify the conditions that trigger the responses in my outcome variables, I can be sure that I account for this delayed relationship.

[37]Since Mincer (1974), it is common to include as a control a polynomial in age or potential experience. However, the inclusion of year effects raises an identification problem because age can be computed as calendar year minus birth year. Hence, this leads to the classic problem of disentangling age, year, and cohort effects, where cohort effects are understood to load into the person effects. Following Card et al. (2013), I restrict the age profile to be flat at age 40 (see

also control for gender, Spanish nationaly, and education level (3 categories).[38]

Table 3.3 reports the estimation for the real annual earnings regression, where each covariate of interest is introduced sequentially. The results in Column 1 show that the earnings gap between CFs and WFs remains after accounting for observed heterogeneity, but these differences vanish when included worker-level unobserved heterogeneity. With respect to the cyclicality of real annual earnings, Columns 2 and 3 present the estimates of the semi-elasticity of annual earnings with respect to the unemployment rate with and without fixed effects, respectively. The comparison of these two specifications highlight the importance of the composition bias. Namely, accounting for worker unobserved heterogeneity removes a countercyclical bias in the composition of employment generated by the fact that low-skilled workers are more likely to lose their jobs during recessions. The estimate in Column 3 that accounts for this composition bias implies that a one percentage point increase in the unemployment rate leads to a 0.90 percent decline in real annual earnings. Interestingly, the results in Column 4 indicate that the semi-elasticity of annual earnings with respect to the unemployment rate is equal between CFs and WFs when worker time invariant unobserved heterogeneity is accounted for.

Card and Cardoso (2012) for evidence on the age profile of wages for Portuguese men that tends to be relatively at after age 40). Then, to obtain identification the model includes a cubic polynomial in (age minus 40) omitting the linear term.

[38]Table 3.C13 in Appendix 3.C, I run the same set of specifications but including plant fixed effects instead of worker fixed effects. The results of these alternative specifications are consistent with the main findings.

Table 3.3: Annual earnings

	(1)	(2)	(3)	(4)
Cooperative	-0.0374***	-0.0374***	-0.0119	-0.0355**
	(0.0139)	(0.0139)	(0.0144)	(0.0163)
Unemp. rate		-0.0058***	-0.0090***	-0.0090***
		(0.0002)	(0.0001)	(0.0001)
Unemp. rate x Coop				0.0001
				(0.0006)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.5386	0.5389	0.7514	0.7514
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of annual earnings perceived from a given employer. Annual earnings are computed as the sum of all earnings received by an individual from a given employer in a year. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include a polynomial in age deviations of degree three, a quadratic polynomial in tenure, occupation-skill category (3 groups), a dummy for workers with a temporary contract, (ln) size of the current establishment, employer age categories (3), dummies for sector of activity (11) and workplace location (50 provinces), and a quadratic time trend. In the plant fixed-effects regression sector of activity and workplace location indicators are excluded from the set of controls as there is no time variation within plants. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.4 instead presents the cyclicality of full-time equivalent real daily wages. The results point to a lower variability of full-time equivalent real daily wages compared to real annual earnings, which stresses the relevance of working-time driving cyclical differences in total remuneration of workers. As before, the comparison between Columns 2 and 3 highlights the relevance of accounting for the heterogeneity of the workforce in differences phases of the business cycle. The estimate of the semi-elasticity of full-time equivalent daily wages in Column 3 reveals the low cyclicality of real wages in Spain. More specifically, a one percentage point increase in the unemployment rate leads to a decrease of full-time equivalent real daily wages of 0.57 percent.[39] Column 4 indicates that the cyclicality of full-time equivalent real daily wages is similar between individuals working in either of the employers category.

The analysis of real annual earnings as well as full-time equivalent real daily wages points that CFs and WFs adjusted the remuneration of their employees in a similar way over the business cycle, even though the volatility of employment was lower. These findings contrast to those in the previous literature on partners of worker-owned firms, which found that their earnings are more responsive to changes in the environment than the earnings of wage-earners in conventional enterprises. In the next section I analyze the cyclicality of working-time to investigate whether the lower employment volatility is driven by WFs adjusting other margins or it is just the result of disparities in the objectives of the two types of organizations.

[39] De La Roca (2014) finds a semi-elasticity of real daily wages in the private sector with respect to the quarterly national unemployment rate for the Spanish economy of -0.4 for the period 1998-2011. The responsiveness of wages in Spain is particularly low compared to existing evidence for other countries. For the United States a one percentage point decrease in the unemployment rate is correlated with a real wages increment of 1.3-1.5. For European countries (Germany, Italy, Portugal, United Kingdom) the estimated real wage cyclicality is even greater at 2.0-2.2 (Pissarides, 2009; Carneiro et al., 2012).

Table 3.4: Full-time equivalent daily wages

	(1)	(2)	(3)	(4)
Cooperative	-0.0216***	-0.0216***	-0.0014	-0.0049
	(0.0064)	(0.0065)	(0.0056)	(0.0056)
Unemp. rate		-0.0047***	-0.0062***	-0.0062***
		(0.0001)	(0.0000)	(0.0000)
Unemp. rate x Coop				-0.0001
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.5607	0.5615	0.8853	0.8853
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of the full-time equivalent daily wage rate perceived from a given employer. Full-time equivalent daily wages are calculated as the ratio of total annual earnings over the total number of days worked times the share of time worked in a year. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include the same controls as in Table 3.3. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

3.7.3 Working-time

In this section, I present the results for the analysis of evolution over the business cycle of working-time. The regression models incorporate the same set of controls as in the analysis of earnings carried out in Section 3.7.2. The main coefficients of interest correspond those attached to the national unemployment rate and its interaction with the cooperative indicator.

Table 3.5 depicts the estimates of the semi-elasticity of working hours, expressed as (log) of the percentage of time worked by an individual relative to a full-time worker in the same establishment, with respect to the national unemployment rate.[40] The regression model indicates that workers in WFs tend to work less hours over a year relative to employees of CFs. With respect to

[40]In Column 1 of Table 3.C11 in the Appendix 3.C, I alternatively estimate a linear probability model using as a dependent variable a binary indicator for the probability of having a full-time job, i.e. percentage of time worked equal 100, and the results are equivalent.

its cyclicality, the estimates point that a one percentage point increase in the unemployment rate implies a 0.20 percent reduction in working hours in CFs. The results in Column 4 indicate that WFs also adjust hours over the business cycle, but they do it to a lesser extent compared to CFs.

Table 3.5: Working-hours

	(1)	(2)	(3)	(4)
Cooperative	-0.0263***	-0.0263***	-0.0083*	-0.0229***
	(0.0097)	(0.0097)	(0.0045)	(0.0056)
Unemp. rate		-0.0020***	-0.0020***	-0.0020***
		(0.0001)	(0.0000)	(0.0000)
Unemp. rate x Coop				0.0009***
				(0.0002)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.1477	0.1480	0.7211	0.7211
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of the percentage of hours worked in a year relative to a full-time worker in the same establishment. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include the same controls as in Table 3.3. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.6 looks at the cyclicality of the number of days worked per year.[41] The results point to no significant differences in the number of days worked by employees of CFs or WFs in a given year. Column 2 and 3 shows the cyclicality of this margin. The comparison between these two columns highlights that composition biases play a prominent role in the case of the cyclicality of working-days. More precisely, the results show that including worker fixed effects completely offsets the pro-cyclicality of days observed in Column 2. This relevance of the composition bias in terms of days might be associated

[41]In Column 2 of Table 3.C11 in the Appendix 3.C, I instead use a linear probability model for the likelihood an individual works a complete year and the results hold.

with the dual nature of the Spanish labor market, where workers under fixed-term contracts are over-represented during booms (Bentolila et al., 2012). The estimate of the semi-elasticity in Column 3 indicates that one percentage point in the unemployment rate leads to 0.08 percent decrease in the number of days worked in a year. Moreover, Column 4 suggests that the volatility of days is akin between the two employer categories.

Altogether, the findings indicate that both types of firms promote adjustments on labor income and working-time over the business cycle. This similar adjustment can be driven by the constraints imposed by labor regulations, which are similar for employees of both type of enterprises. Yet, the differential evolution in employment suggests that differences in the goals of both organizations might be behind the employment resilience of worker-owned firms.

Table 3.6: Working-days

	(1)	(2)	(3)	(4)
Cooperative	0.0105	0.0105	-0.0022	-0.0076
	(0.0073)	(0.0073)	(0.0094)	(0.0123)
Unemp. rate		0.0009***	-0.0008***	-0.0008***
		(0.0001)	(0.0001)	(0.0001)
Unemp. rate x Coop				0.0003
				(0.0005)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.3606	0.3607	0.5419	0.5419
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of total days worked in a given establishment. Number of days worked is computed exploiting the difference between ending and starting dates of each job spell and, then, aggregating over the year. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include the same controls as in Table 3.3. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

3.8 Conclusion

This chapter investigates differences between mainstream capitalist enterprises and worker-owned firms. The goal is to understand whether the praised employment resilience of partners in worker-owned firms is shared with conventional employees of these organizations. The comparative analysis between capital- and worker-owned firms is performed in Spain, one of the countries where the worker-owned firms' culture is more widespread.

The analysis shows that employment of individuals hired by worker-owned firms is more stable over the business cycle compared to their capitalist counterparts. This suggests that the employment resilience of partners of worker-owned firms are to some extent shared by all workforce in those organizations. The parallel analysis of labor income and working-time of workers employed in worker-owned firms relative to employees of conventional organizations indicates that the volatility of these margins is similar between both type of organizations. These findings contrast to those in the previous literature on partners of worker-owned firms, which found that their earnings are more responsive to changes in the environment than the earnings of wage-earners in conventional enterprises.

The current results can be rationalized by the presence of similar labor regulations and differences in the objectives of the two types of organizations. On the one hand, firms are constrained by regulations, such as collective bargaining, on the adjustments that can impose on wages and working hours. On the other hand, due to its social nature, worker-owned firms tend to protect employment to a greater extent, even if this employment stability comes with lower profits (Perotin, 2013). The findings suggest that social economy enterprises might contribute to "create the conditions for a more competitive, sustainable and inclusive economy with more and better conditions of employment" (European Commission, 2010, 2011).

Appendix 3.A Variables definition

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

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

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

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

Contract type. The MCVL contains a long list of contracts (+100 types) that are summarized in two broad categories, according to its permanent or temporary nature. Permanent contracts include regular permanent contracts (*contrato indefinido fijo*). Temporary contracts include seasonal permanent contracts (*indefinido fijo-discontinuo*), specific project or service contracts (*temporal*

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

por obra o servicio), temporary increase in workload (*eventual de produccion*), and substitution contracts (*interinidad o relevo*).

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

Plant. A plant is defined by its Social Security contribution account (*codigo de cuenta de cotizacion*). Each firm is mandated to have as many accounts as regimes, provinces, and relation types with which it operates. The contribution accounts are assigned by the Social Security administration, and they are fixed and unique for each treble province-Social Security regime-type of employment relation.

Sector of activity. The MCVL provides information on the main sector of activity at a three-digit level (*actividad economica de la cuenta de cotizacion, CNAE*). Due to a change in the classification in 2009, the MCVL contains CNAE93 and CNAE09 for all plants observed in business from 2009 onwards, but only CNAE93 for those which stop their activity before. I use the CNAE09 classification when available, and CNAE93 otherwise exploiting the correspondence table provided by the Spanish National Statistical Office.[43] Then, I aggregate the three-digit industry information in 14 categories: agriculture and extraction (CNAE09 codes 1 to 99); manufacturing and utilities (100 to 399); construction (411 to 439); wholesale and retail trade (451 to 479); transportation and storage (491 to 532); accommodation and food services (551 to 563); information and communication technologies (581 to 639); financial, insurance and real estate activities (641 to 683); professional, scientific and technical activities (691 to 750); administrative, support and other services (771 to 829 and 950 to 970); education, health and social work (851 to 889); entertainment (900 to 949); public sector/social security (841 to 843) and international organizations (990).

Plant creation date. Date when the first employee was registered in the contribution account. I use this date as a proxy for the plant creation date to compute the age of the plant.

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

Unemployment rate. Refers to the either the national or the provincial yearly unemployment rate retrieved from the National Statistical Office. This variable can be downloaded from `http://ine.es/jaxiT3/Tabla.htm?t=3996&L=0`

Activity index. Measured using the FEDEA Index that summarizes the evolution of economic activity in Spain using information available from many different sources (GDP, industrial production, indices of economic sentiment,

[43] `http://www.ine.es/daco/daco42/clasificaciones/rev.1/cnae2009_cnae93rev1.pdf`

etc.). For a more detailed description of the index, see `http://www.fedea.net/indice/`

Appendix 3.B Censoring correction

The MCVL reports data on monthly labor income from Social Security contribution bases throughout 2005-2016, but they are either bottom or top-coded. In the final sample, roughly 1.47 and 9.80 percent of the log real daily wages of the worker-month observations are bottom or top-coded, respectively.

Following other studies that face censored earnings in administrative data (Dustmann et al., 2009; Card et al., 2013; Bonhomme and Hospido, 2017), I correct the upper tail by fitting cell-by-cell Tobit models to log real daily wages.[44] Each cell, c, is defined according to occupational groups (3 categories), age groups (4 categories), and years (12) for a total of 144 cells. Consistent with a vast literature that finds that log-normality provides a reasonable approximation to empirical wage distributions, within each cell, log-daily wages are assumed to follow a Gaussian distribution with cell-specific mean and variance, i.e. $log\ w \sim N(X\beta_c, \sigma_c^2)$.[45]

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

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

where w_{it} represents real daily wages of individual i in plant j in moment t (a worker-year pair), $\bar{w}$ is the maximum cap, $cens_{ijt} = 1$ if the observation is top-coded. X_{ijt} is a set of controls such as age, categorical variables for females, temporary contract, full-time job, and worker-owned firms; a polynomial of degree two in size, a dummy for establishments with more than 50 employees,

[44]The lower tail is not corrected due to the existence of a national minimum wage.

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

and monthly dummies (12). Following Card, Heining and Kline (2013), I also include individual-specific components of the wages using the mean log daily wages in other year, fraction of censored wages in other years, and a dummy for individuals observed only once as additional controls. For individuals who are only observed once, I set the mean log daily wages in other years to the sample mean, and the fraction of censored wages in other years equal to the share of censored earnings in the sample.

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

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

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

Table 3.B1: Censored and imputed wage distributions

Percentiles	Censored	Imputed
5th	2.78	2.78
10th	3.17	3.17
25th	3.64	3.64
50th	3.93	3.93
75th	4.31	4.31
90th	4.70	4.75
95th	4.73	5.13

Notes: Wages refer to log real daily wages earned by workers in a given employer each month.

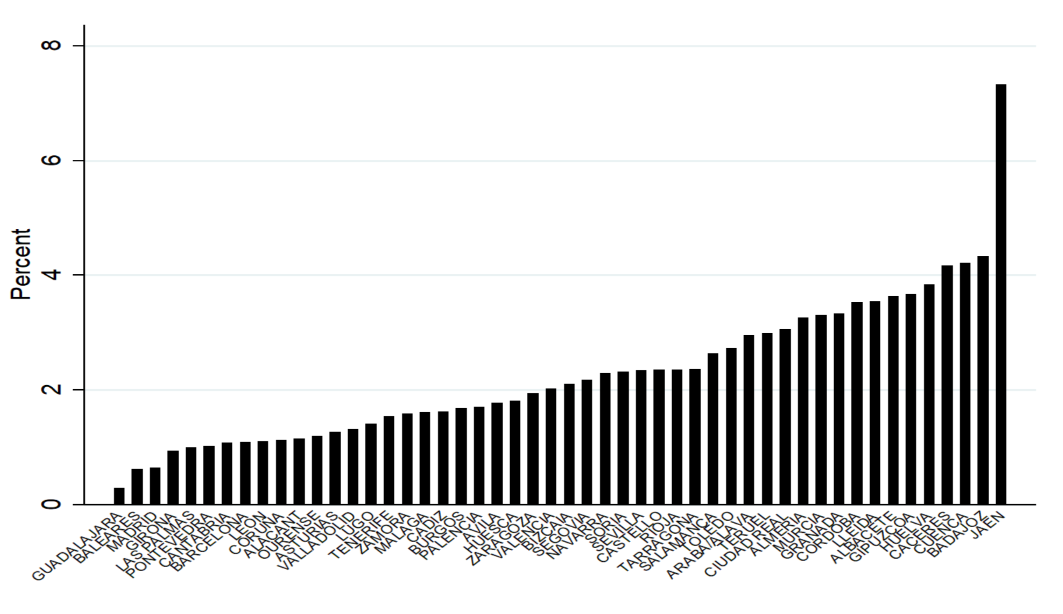
Appendix 3.C Supplementary tables and figures

Table 3.C1: Plant characteristics

	Capitalists		Cooperatives	
Variables	Mean	Std. Dev.	Mean	Std. Dev.
Size	25.55	119.0	23.40	71.68
(ln) Size	2.24	1.27	2.15	1.28
Multi-establishment	0.204		0.141	
Plant age < 1	0.003		0.002	
Plant age > 5	0.814		0.895	
Manufacturing	0.171		0.282	
Construction	0.181		0.097	
Biggest 4 cities	0.382		0.255	
Plants		458,497		7,116
Plant-Year obs.		3,152,167		56,239

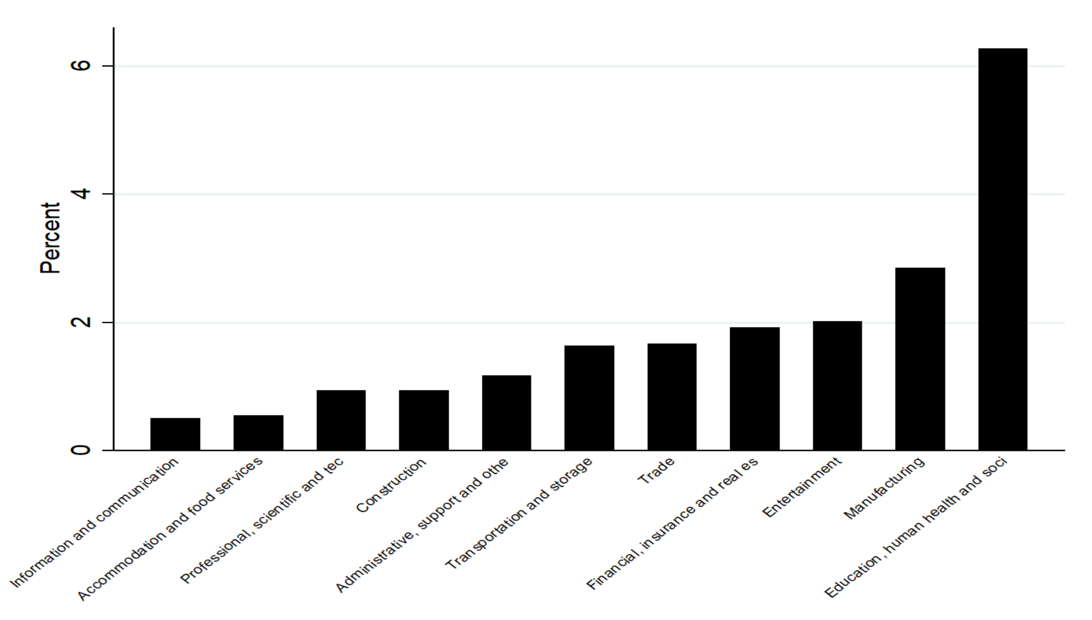
Notes: Descriptive statistics are computed over plant-year observations. Multi-establishment is a dummy variably identifying plants that belong to multi-establishment firms. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Figure 3.C1: Cooperatives by province, 2005-2016



Notes: Percentage of cooperative plants over total plants in each province.

Figure 3.C2: Cooperatives by sector of activity, 2005-2016



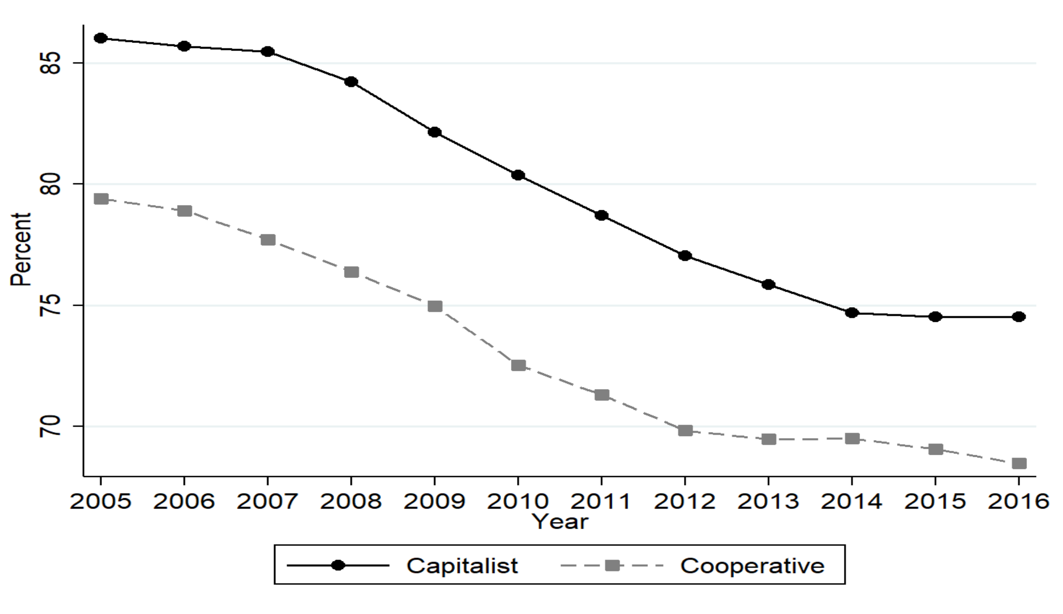
Notes: Percentage of cooperative plants over total plants in each sector.

Table 3.C2: Worker characteristics

	Capitalists			Cooperatives	
Variables	Mean	Std. Dev.		Mean	Std. Dev.
(ln) Real annual earnings	9.39	1.02		9.25	1.07
(ln) Real ft-equiv. daily wages	4.03	0.49		3.99	0.47
(ln) Part-time percentage	4.49	0.31		4.43	0.41
(ln) Number of days	5.48	0.64		5.43	0.66
(ln) No. Jobs	0.077	0.27		0.14	0.35
(ln) Full-time days per job	5.25	0.84		5.09	0.94
Age (yr)	37.85	9.88		37.93	10.16
Female	0.407			0.484	
Spanish	0.902			0.948	
Secondary ed.	0.289			0.271	
Tertiary ed.	0.178			0.217	
Tenure (yr)	3.71	5.12	4.11	6.17	
Temporary contract	0.312			0.423	
Mid-skill	0.347			0.311	
High-skill	0.140			0.172	
Workers		648,027			20,793
Worker-Year obs.		4,483,681			74,259

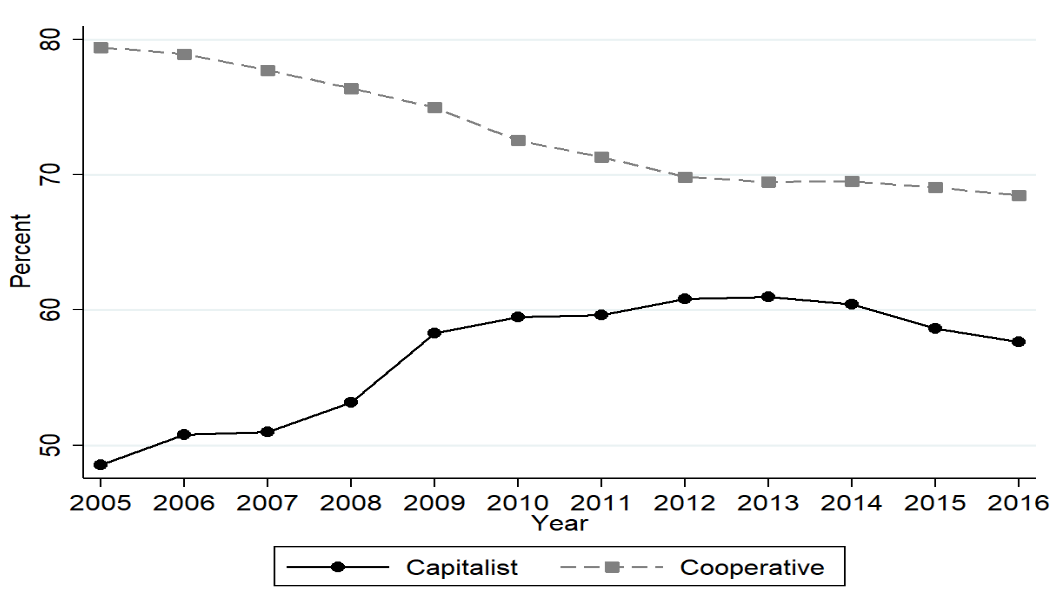
Notes: Descriptive statistics are computed over worker-plant-year observations. Earnings correspond to censoring-corrected earnings. Full-time equivalent daily wages are calculated as the ratio of total annual earnings over total days worked in a year times the share of hours worked. Labor income measures are deflated using 2016 Consumer Price Index. Working hours represent the average percentage of time an employee worked in a year relative to a full-time worker in the same establishment. Working days are the total number of days an individual worked for the same employer during a year. Temporary contract includes both fixed-term contracts and intermittent (seasonal) permanent contracts.

Figure 3.C3: Full-time workers in CFs and WFs, 2005-2016



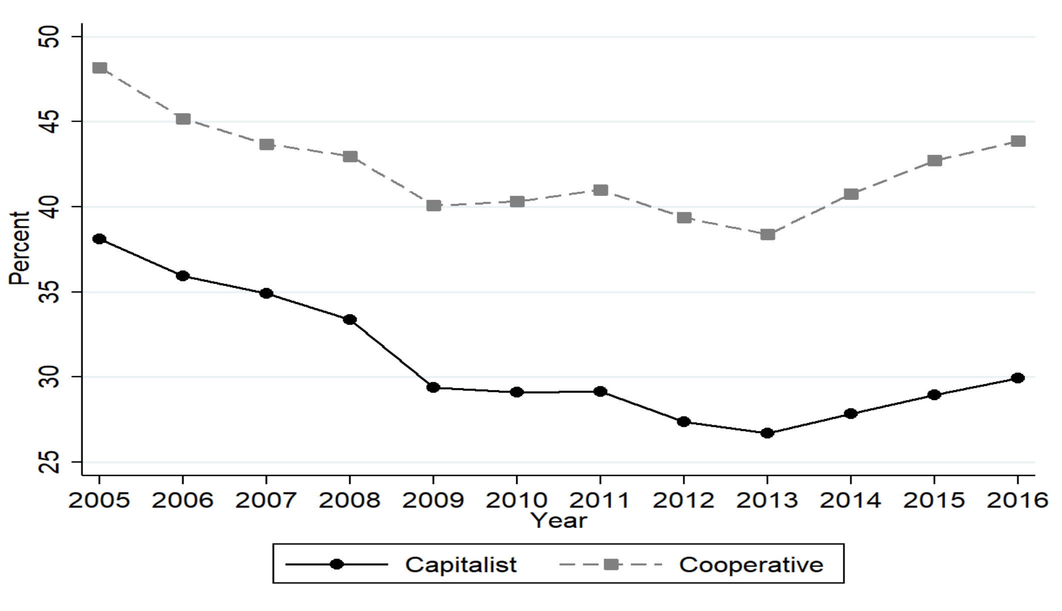
Notes: Full-time workers are individuals who have the percentage of hours worked in a year equal to 100.

Figure 3.C4: Full-year workers in CFs and WFs, 2005-2016



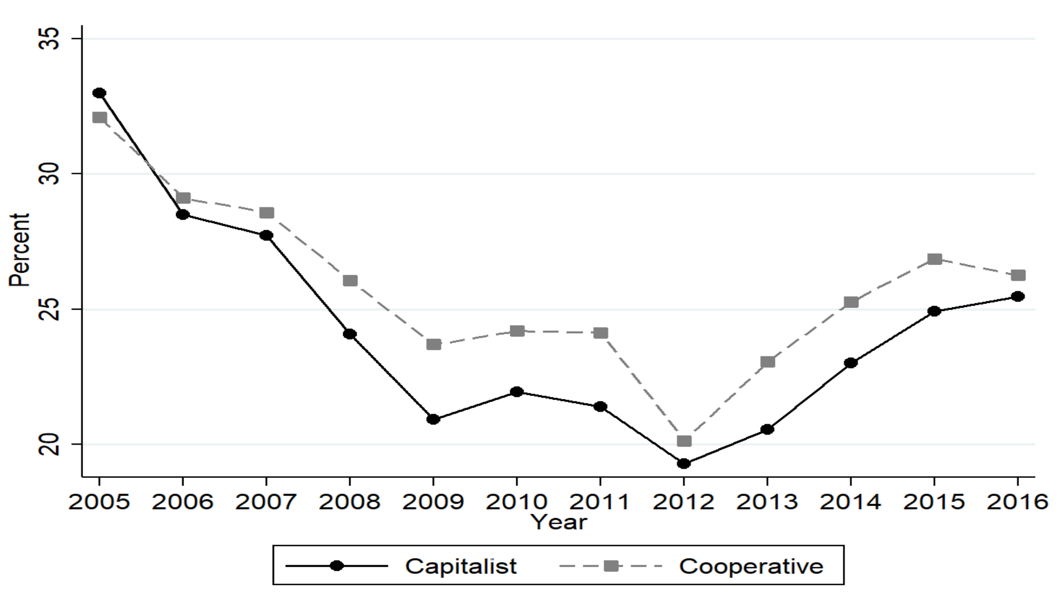
Notes: Full-year workers are individuals who have worked the whole year with the same employer.

Figure 3.C5: Workers under temporary contracts in CFs and WFs, 2005-2016



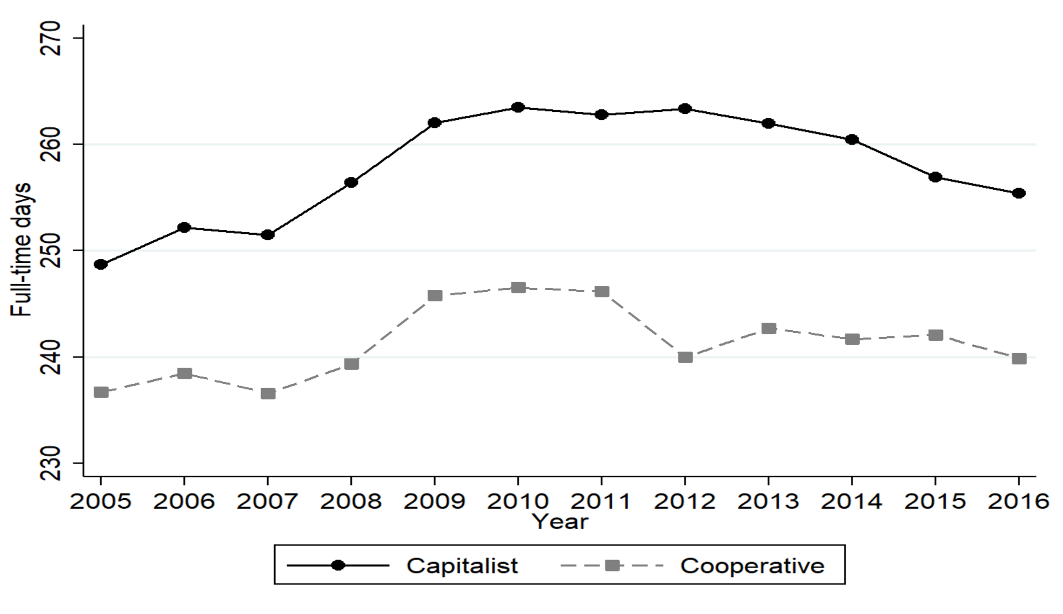
Notes: Temporary contracts stand both for fixed-term contracts and seasonal permanent contracts.

Figure 3.C6: Newcomers in CFs and WFs, 2005-2016



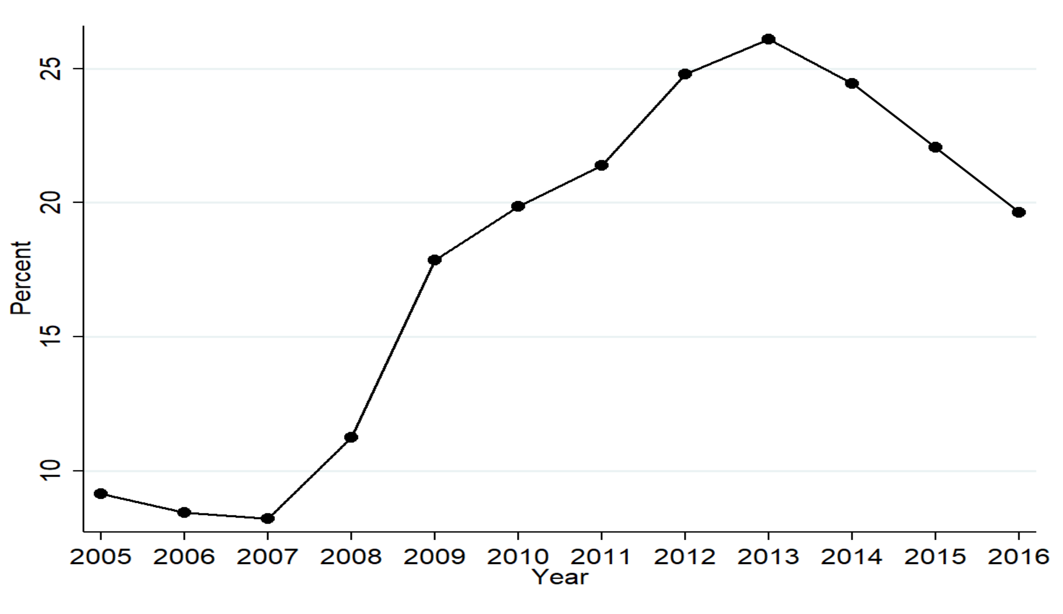
Notes: Newcomer refers to workers with less than a year of tenure.

Figure 3.C7: Full-time equivalent days in CFs and WFs, 2005-2016



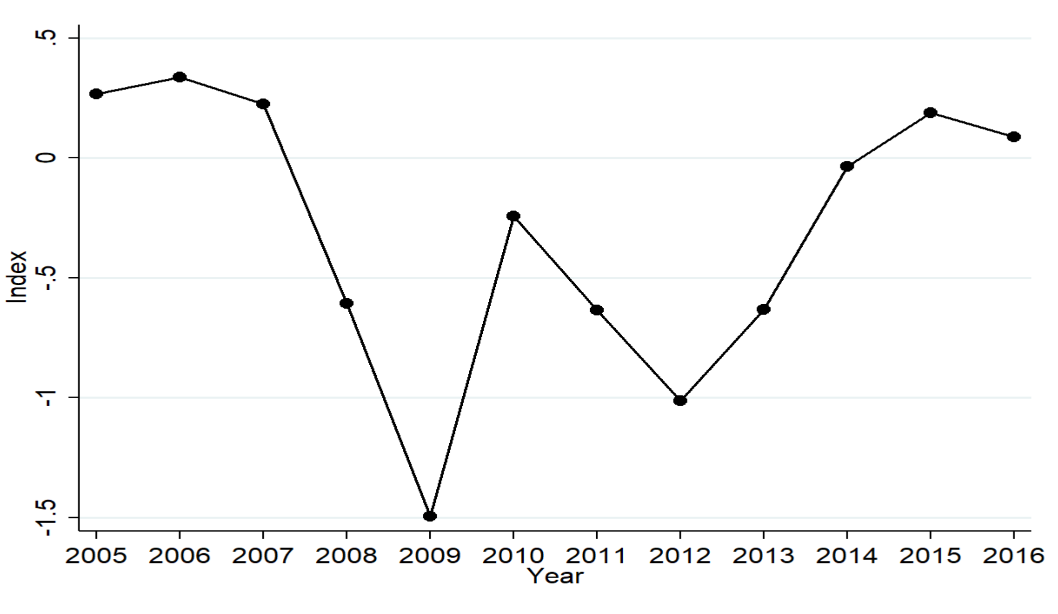
Notes: Full-time equivalent days are computed as the total days worked in a year times the share of time worked.

Figure 3.C8: National unemployment rate, 2005-2016



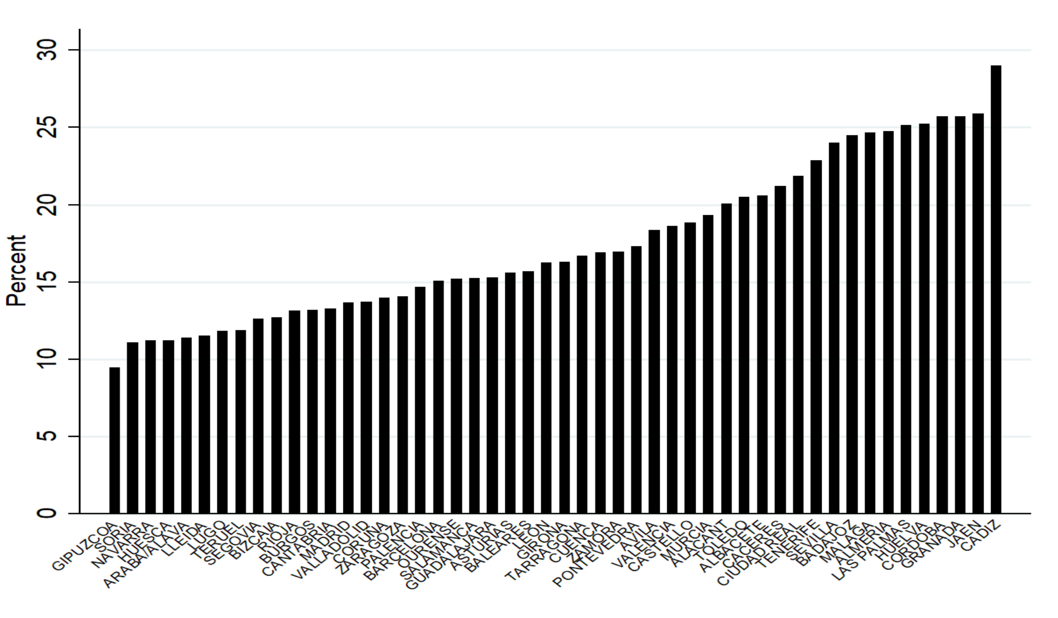
Source: *Instituto Nacional de Estadistica*.

Figure 3.C9: FEDEA index of economic activity, 2005-2016



Source: *FEDEA.*

Figure 3.C10: Average unemployment rate by province, 2005-2016



Source: *Instituto Nacional de Estadistica.*

Table 3.C3: Plant-level employment cyclicality: Full specification

	(1)	(2)	(3)	(4)
Cooperative	-0.1086***	-0.1085***	-0.2218***	
	(0.0156)	(0.0156)	(0.0213)	
Unemp. rate		-0.0100***	-0.0102***	-0.0141***
		(0.0001)	(0.0001)	(0.0001)
Unemp. rate x Coop			0.0067***	0.0098***
			(0.0010)	(0.0007)
Multi-establishment firm	0.8861***	0.8861***	0.8860***	0.1732***
	(0.0053)	(0.0053)	(0.0053)	(0.0058)
Plant age < 1	-0.2673***	-0.2206***	-0.2209***	-0.0965***
	(0.0118)	(0.0119)	(0.0119)	(0.0085)
Plant age > 5	0.2511***	0.2531***	0.2532***	0.0176***
	(0.0030)	(0.0030)	(0.0030)	(0.0020)
Trend	-0.1266***	-0.1039***	-0.1039***	-0.0651***
	(0.0008)	(0.0008)	(0.0008)	(0.0006)
Trend squared	0.0065***	0.0061***	0.0061***	0.0035***
	(0.0001)	(0.0001)	(0.0001)	(0.0000)
Constant	2.8068***	2.8487***	2.8506***	2.6622***
	(0.0213)	(0.0213)	(0.0213)	(0.0026)
Plants	465,497	465,497	465,497	465,497
Plant-Year obs.	3,208,406	3,208,406	3,208,406	3,208,406
R-squared	0.1330	0.1335	0.1335	0.8704
Sector FE (11)	Yes	Yes	Yes	No
Province FE (50)	Yes	Yes	Yes	No
Plant FE	No	No	No	Yes

Notes: Dependent variable is the (log) of plant-level employment. Unemp. rate stands for one-year lagged national unemployment rate. Multi-establishment firm identifies plants that belong to multi-establishment organizations. Standard errors clustered at the plant-level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C4: Plant-level employment cyclicality: Sensitivity to time specification

	Benchmark	FEDEA Index	Province UR	Year FE	Sector Trend	SectorxTime FE
Unemp. rate	-0.0141***				-0.0146***	0.0026
	(0.0001)				(0.0001)	(0.0033)
Unemp. rate x Coop	0.0098***			0.0098***	0.0054***	0.0052***
	(0.0007)			(0.0007)	(0.0007)	(0.0007)
FEDEA Index		0.0789***				
		(0.0006)				
FEDEA Index x Coop		-0.0787***				
		(0.0051)				
Unemp. rate (prov)			-0.0142***			
			(0.0001)			
Unemp. rate x Coop (prov)			0.0101***			
			(0.0007)			
Plants	465,497	465,497	465,497	465,497	465,497	465,497
Plant-Year obs.	3,208,406	3,208,406	3,208,406	3,208,406	3,208,406	3,208,406
R-squared	0.8704	0.8701	0.8708	0.8707	0.8750	0.8756
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Time trend (quadratic)	Yes	Yes	Yes	No	Yes	No

Notes: Dependent variable is the (log) of plant-level employment. Column 1 and 5-6 use the one year-lagged national unemployment rate as a measure for aggregate conditions. Column 2 relies on the one-year lagged FEDEA Index of economic activity as a proxy for the business cycle. Column 3 substitute the national unemployment rate by the provincial unemployment rate. In Column 4, the non-interacted unemployment rate term is replaced by genuine year dummies (13). Columns 5 and 6 include sector specific time effects using a quadratic time trend or year dummies, respectively. All specifications include indicator variables for plant's age (3) and multi-establishment firms. Multi-establishment firm identifies plants that belong to multi-establishment organizations. Standard errors clustered at the plant-level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C5: Plant-level employment cyclicality: Sensitivity to including all plants

	Benchmark	All plants	Excl. Entry&Exit	Δ_t(log) Size	Pr(Entry$_t$=1)	Pr(Exit$_t$=1)
Unemp. rate	-0.0141***	-0.0132***	-0.0117***	-0.0139***	-0.0012***	0.0042***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0000)	(0.0000)
Unemp. rate x Coop	0.0098***	0.0099***	0.0079***	0.0055***	0.0007***	-0.0009***
	(0.0007)	(0.0006)	(0.0006)	(0.0006)	(0.0001)	(0.0001)
Plants	465,613	686,827	329,814	586,036	686,827	686,827
Plant-Year obs.	3,208,406	4,351,939	2,647,663	3,665,112	5,033,872	5,033,872
R-squared	0.8704	0.8808	0.9041	0.2049	0.0072	0.0125
Plant FE	Yes	Yes	Yes	Yes	No	No

Notes: Columns 1 to 3 use as dependent variable the (log) of plant-level employment stock. Unemp. rate stands for one-year lagged national unemployment rate. In Column 4, the dependent variable is the change in (log) plant-size between $t-1$ and t, and the unemployment rate refers to the change in the national unemployment rate between $t-1$ and t. Columns 1 to 4 include indicator variables for plant's age (3) and multi-establishment firms, and a quadratic-time trend as controls. Multi-establishment firm identifies plants that belong to multi-establishment organizations. Columns 5 and 6 estimate linear probability models for the likelihood of observing a new plant (entry) and a plant for the last time (exit) in a given year t, respectively. These linear probability models include as controls the indicator for cooperative employers, sector of activity (11 dummies) and location (50 indicators), and genuine year dummies. Standard errors clustered at the plant-level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C6: Annual earnings: Full specification

	(1)	(2)	(3)	(4)
Cooperative	-0.0374***	-0.0374***	-0.0119	-0.0355**
	(0.0139)	(0.0139)	(0.0144)	(0.0163)
Unemp. rate		-0.0058***	-0.0090***	-0.0090***
		(0.0002)	(0.0001)	(0.0001)
Unemp. rate x Coop				0.0001
				(0.0006)
Age dev.^{2}/100	-0.0496***	-0.0498***	-0.0654***	-0.0654***
	(0.0008)	(0.0008)	(0.0009)	(0.0009)
Age dev.^{3}/100	0.0039***	0.0039***	0.0028***	0.0028***
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Female	-0.2551***	-0.2552***		
	(0.0023)	(0.0023)		
Spanish	0.1124***	0.1141***		
	(0.0028)	(0.0028)		
Secondary ed.	0.0676***	0.0678***		
	(0.0018)	(0.0018)		
Tertiary ed.	0.1366***	0.1371***		
	(0.0028)	(0.0028)		
Mid-skill	0.1125***	0.1124***	0.0472***	0.0472***
	(0.0037)	(0.0037)	(0.0032)	(0.0032)
High-skill	0.6058***	0.6055***	0.3143***	0.3143***
	(0.0046)	(0.0046)	(0.0040)	(0.0040)
Temporary contract	-0.7479***	-0.7472***	-0.6772***	-0.6771***
	(0.0034)	(0.0034)	(0.0025)	(0.0025)
Tenure (yr)	0.0901***	0.0903***	0.0687***	0.0687***
	(0.0007)	(0.0007)	(0.0006)	(0.0006)
Tenure^2	-0.0025***	-0.0025***	-0.0026***	-0.0026***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(log) Plant size	0.0695***	0.0694***	0.0696***	0.0696***
	(0.0018)	(0.0018)	(0.0012)	(0.0012)
Plant age < 1	-0.1957***	-0.1718***	-0.1585***	-0.1585***
	(0.0085)	(0.0085)	(0.0083)	(0.0083)
Plant age > 5	-0.0573***	-0.0572***	-0.0759***	-0.0759***
	(0.0026)	(0.0027)	(0.0015)	(0.0015)
Constant	9.4033***	9.4308***	9.6084***	9.6087***
	(0.0145)	(0.0146)	(0.0181)	(0.0181)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.5386	0.5389	0.7514	0.7514
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of annual earnings perceived from a given employer. Annual earnings are computed as the sum of all earnings received by an individual from a given employer in a year and are expressed in Euros of 2016.. Unemp. rate stands for one-year lagged national unemployment rate. All specifications also include dummies for sector of activity (11) and workplace location (50 provinces), and a quadratic time trend. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C7: Full-time equivalent daily wages: Full specification

	(1)	(2)	(3)	(4)
Cooperative	-0.0201**	-0.0202**	-0.0053	-0.0205***
	(0.0092)	(0.0092)	(0.0044)	(0.0054)
Unemp. rate	-0.0023***	-0.0023***	-0.0023***	-0.0026***
	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Unemp. rate x Coop			0.0009***	0.0006**
			(0.0002)	(0.0003)
Age dev.^{2}/100	-0.0142***	-0.0143***	-0.0352***	-0.0352***
	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Age dev.^{3}/100	0.0021***	0.0021***	0.0007***	0.0007***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	-0.1575***	-0.1576***		
	(0.0015)	(0.0015)		
Spanish	0.0637***	0.0651***		
	(0.0017)	(0.0017)		
Secondary ed.	0.0777***	0.0778***		
	(0.0013)	(0.0013)		
Tertiary ed.	0.1555***	0.1559***		
	(0.0021)	(0.0021)		
Mid-skill	0.0850***	0.0849***	0.0172***	0.0172***
	(0.0026)	(0.0026)	(0.0013)	(0.0013)
High-skill	0.5489***	0.5487***	0.2027***	0.2026***
	(0.0034)	(0.0034)	(0.0020)	(0.0020)
Temporary contract	-0.1202***	-0.1196***	-0.0878***	-0.0878***
	(0.0016)	(0.0016)	(0.0008)	(0.0008)
Tenure (yr)	0.0264***	0.0265***	0.0175***	0.0175***
	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Tenure^2.	-0.0005***	-0.0005***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(log) Plant size	0.0496***	0.0496***	0.0288***	0.0288***
	(0.0014)	(0.0014)	(0.0004)	(0.0004)
Plant age < 1	-0.0022	0.0172***	0.0174***	0.0174***
	(0.0042)	(0.0042)	(0.0026)	(0.0026)
Plant age > 5	-0.0310***	-0.0309***	-0.0134***	-0.0134***
	(0.0017)	(0.0017)	(0.0006)	(0.0006)
Constant	3.9225***	3.9448***	4.0540***	4.0540***
	(0.0104)	(0.0104)	(0.0079)	(0.0079)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.5607	0.5615	0.8853	0.8853
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of the full-time equivalent daily wage rate perceived from a given employer. Full-time equivalent daily wages are calculated as the ratio of total annual earnings over the total number of days worked times the share of time worked in a year and are expressed in Euros of 2016. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include the same controls as in Table 3.C6. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C8: Working-hours: Full specification

	(1)	(2)	(3)	(4)
Cooperative	-0.0263***	-0.0263***	-0.0083*	-0.0229***
	(0.0097)	(0.0097)	(0.0045)	(0.0056)
Unemp. rate		-0.0020***	-0.0020***	-0.0020***
		(0.0001)	(0.0000)	(0.0000)
Unemp. rate x Coop				0.0009***
				(0.0002)
Age dev.^{2}/100	-0.0166***	-0.0167***	-0.0010***	-0.0010***
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Age dev.^{3}/100	0.0004***	0.0004***	0.0009***	0.0009***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	-0.1157***	-0.1158***		
	(0.0013)	(0.0013)		
Spanish	-0.0115***	-0.0109***		
	(0.0015)	(0.0015)		
Secondary ed.	-0.0019**	-0.0018**		
	(0.0008)	(0.0008)		
Tertiary ed.	-0.0046***	-0.0044***		
	(0.0013)	(0.0013)		
Mid-skill	0.0342***	0.0342***	0.0212***	0.0212***
	(0.0019)	(0.0019)	(0.0015)	(0.0015)
High-skill	0.0494***	0.0493***	0.0532***	0.0532***
	(0.0019)	(0.0019)	(0.0018)	(0.0018)
Temporary contract	-0.0626***	-0.0624***	-0.0335***	-0.0335***
	(0.0018)	(0.0018)	(0.0010)	(0.0010)
Tenure (yr)	0.0040***	0.0041***	0.0022***	0.0022***
	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Tenure^2	-0.0001***	-0.0001***	-0.0000***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(log) Plant size	0.0045***	0.0044***	0.0049***	0.0049***
	(0.0006)	(0.0006)	(0.0004)	(0.0004)
Plant age $<$ 1	0.0142***	0.0222***	0.0081***	0.0081***
	(0.0028)	(0.0028)	(0.0019)	(0.0019)
Plant age $>$ 5	-0.0004	-0.0004	0.0012**	0.0012**
	(0.0011)	(0.0011)	(0.0006)	(0.0006)
Constant	4.6270***	4.6363***	4.5675***	4.5677***
	(0.0092)	(0.0093)	(0.0065)	(0.0065)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.1477	0.1480	0.7211	0.7211
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of the percentage of hours worked in a year relative to a full-time worker in the same establishment. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include the same controls as in Table 3.C6. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C9: Working-days: Full specification

	(1)	(2)	(3)	(4)
Cooperative	0.0105	0.0105	-0.0022	-0.0076
	(0.0073)	(0.0073)	(0.0094)	(0.0123)
Unemp. rate		0.0009***	-0.0008***	-0.0008***
		(0.0001)	(0.0001)	(0.0001)
Unemp. rate x Coop				0.0003
				(0.0005)
Age dev.^{2}/100	-0.0189***	-0.0188***	-0.0292***	-0.0292***
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
Age dev.^{3}/100	0.0014***	0.0014***	0.0012***	0.0012***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	0.0182***	0.0182***		
	(0.0009)	(0.0009)		
Spanish	0.0601***	0.0599***		
	(0.0014)	(0.0014)		
Secondary ed.	-0.0082***	-0.0082***		
	(0.0008)	(0.0008)		
Tertiary ed.	-0.0144***	-0.0144***		
	(0.0011)	(0.0011)		
Mid-skill	-0.0067***	-0.0067***	0.0088***	0.0088***
	(0.0013)	(0.0013)	(0.0021)	(0.0021)
High-skill	0.0075***	0.0075***	0.0584***	0.0584***
	(0.0014)	(0.0014)	(0.0026)	(0.0026)
Temporary contract	-0.5651***	-0.5652***	-0.5559***	-0.5559***
	(0.0019)	(0.0019)	(0.0017)	(0.0017)
Tenure (yr)	0.0597***	0.0597***	0.0490***	0.0490***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Tenure sq.	-0.0019***	-0.0019***	-0.0021***	-0.0021***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(log) Plant size	0.0154***	0.0154***	0.0360***	0.0360***
	(0.0004)	(0.0004)	(0.0008)	(0.0008)
Plant age $<$ 1	-0.2077***	-0.2112***	-0.1840***	-0.1840***
	(0.0064)	(0.0064)	(0.0072)	(0.0072)
Plant age $>$ 5	-0.0259***	-0.0259***	-0.0637***	-0.0637***
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Constant	5.4590***	5.4549***	5.5921***	5.5922***
	(0.0060)	(0.0061)	(0.0131)	(0.0131)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.3606	0.3607	0.5419	0.5419
Worker FE	No	No	Yes	Yes

Notes: The dependent variable is the logarithm of the number of days worked in a year. Unemp. rate stands for one-year lagged national unemployment rate. All specifications include the same controls as in Table 3.C6. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C10: Sensitivity to labor income measures

	Annual earnings			Full-time equiv. daily wages		
	Imputed	Censored	Tax records	Imputed	Censored	Tax records
Cooperative	-0.0355**	-0.0371**	-0.0228	-0.0049	-0.0066	0.0133*
	(0.0163)	(0.0161)	(0.0149)	(0.0056)	(0.0052)	(0.0074)
Unemp. rate	-0.0090***	-0.0082***	-0.0097***	-0.0062***	-0.0054***	-0.0065***
	(0.0001)	(0.0001)	(0.0002)	(0.0000)	(0.0000)	(0.0001)
Unemp. rate x Coop	0.0015**	0.0016***	0.0011*	0.0002	0.0004	0.0003
	(0.0006)	(0.0006)	(0.0006)	(0.0003)	(0.0003)	(0.0003)
Workers	691,730	691,730	628,470	691,730	691,730	628,470
Observations	4,557,940	4,557,940	3,825,374	4,557,940	4,557,940	3,825,374
R-squared	0.7514	0.7276	0.7670	0.8853	0.8529	0.7498
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables are in logarithms. All specifications include the same controls as in Table 3.3. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C11: Alternative working-time specifications

	Pr(Full-time$_t$=1)	Pr(Full-year$_t$=1)	(log) Full-time days
Cooperative	-0.0472***	-0.0151*	-0.0655***
	(0.0089)	(0.0092)	(0.0152)
Unemp. rate	-0.0028***	-0.0008***	-0.0046***
	(0.0001)	(0.0001)	(0.0001)
Unemp. rate x Coop	0.0012***	0.0007*	0.0016***
	(0.0003)	(0.0004)	(0.0005)
Workers	691,730	691,730	691,730
Observations	4,557,940	4,557,940	4,557,940
R-squared	0.7013	0.5900	0.6520
Worker FE	Yes	Yes	Yes

Notes: Full-time is a linear probability model for the likelihood a worker had a full-time job with her current employer. Full-year refers to a linear probability model for the likelihood an individual had worked the full year with the same employer. Full-time days stand for the total number of days times the share of hours worked in a year. All specifications include the same controls as in Table 3.3. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C12: Newly-hired workers

	Annual earnings	Ft-equiv daily wages	Hours	Days
Cooperative	-0.0189	-0.0054	-0.0246***	0.0111
	(0.0168)	(0.0054)	(0.0056)	(0.0133)
Unemp. rate	-0.0016***	-0.0054***	-0.0021***	0.0059***
	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Unemp. rate x Newly-hired	-0.0283***	-0.0029***	0.0002***	-0.0257***
	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Unemp. rate x Coop	0.0007	0.0001	0.0009***	-0.0003
	(0.0006)	(0.0003)	(0.0002)	(0.0006)
Unemp. rate x Coop x Newly-hired	-0.0007	0.0004**	0.0003	-0.0014*
	(0.0007)	(0.0002)	(0.0002)	(0.0007)
Workers	691,730	691,730	691,730	691,730
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.7706	0.8861	0.7211	0.5822
Worker FE	Yes	Yes	Yes	Yes

Notes: Annual earnings are all earnings received by an individual from a given employer in a year. Full-time equivalent daily wages are calculated as the ratio of total annual earnings over the total number of days worked times the share of time worked in a year. Earnings and wages are expressed in Euros of 2016. Hours stand for the percentage of hours worked in a year relative to a full-time worker in the same establishment. Days refer to the number of days worked in a year. All these dependent variables are expressed in logarithms. Unemp. rate stands for one-year lagged national unemployment rate. Newly-hired refers to workers with less than a year of tenure. All specifications include the same controls as in Table 3.3. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 3.C13: Main results on labor income and working-time using plant fixed effects

	Annual earnings	Ft-equiv daily wages	Hours	Days
Unemp. rate	-0.0048***	-0.0050***	-0.0015***	0.0016***
	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Unemp. rate x Coop	0.0009	-0.0003	0.0006**	0.0006
	(0.0009)	(0.0003)	(0.0003)	(0.0007)
Age dev.^{2}/100	-0.0423***	-0.0141***	-0.0088***	-0.0195***
	(0.0007)	(0.0003)	(0.0004)	(0.0004)
Age dev.^{3}/100	0.0032***	0.0016***	0.0004***	0.0011***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	-0.1528***	-0.1071***	-0.0643***	0.0185***
	(0.0020)	(0.0012)	(0.0011)	(0.0009)
Spanish	0.0276***	0.0148***	-0.0155***	0.0284***
	(0.0024)	(0.0011)	(0.0012)	(0.0017)
Secondary ed.	0.0085***	0.0263***	-0.0047***	-0.0131***
	(0.0015)	(0.0008)	(0.0007)	(0.0009)
Tertiary ed.	0.0263***	0.0584***	-0.0063***	-0.0258***
	(0.0023)	(0.0015)	(0.0011)	(0.0013)
Mid-skill	0.1194***	0.0897***	0.0145***	0.0152***
	(0.0032)	(0.0019)	(0.0018)	(0.0014)
High-skill	0.5279***	0.4926***	0.0209***	0.0144***
	(0.0041)	(0.0028)	(0.0019)	(0.0016)
Temporary contract	-0.7542***	-0.0997***	-0.0541***	-0.6003***
	(0.0029)	(0.0011)	(0.0013)	(0.0023)
Tenure (yr)	0.0820***	0.0234***	0.0041***	0.0545***
	(0.0008)	(0.0003)	(0.0002)	(0.0006)
Tenure^2	-0.0022***	-0.0004***	-0.0001***	-0.0017***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(log) Plant size	0.1760***	0.0217***	0.0020***	0.1522***
	(0.0022)	(0.0007)	(0.0006)	(0.0019)
Plant age < 1	-0.1404***	0.0220***	0.0048**	-0.1671***
	(0.0090)	(0.0028)	(0.0023)	(0.0080)
Plant age > 5	-0.0736***	-0.0107***	-0.0074***	-0.0555***
	(0.0020)	(0.0009)	(0.0008)	(0.0017)
Trend	-0.0050***	0.0109***	-0.0063***	-0.0095***
	(0.0007)	(0.0003)	(0.0002)	(0.0006)
Trend^2	-0.0003***	-0.0004***	0.0003***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	8.8510***	3.8768***	4.5826***	4.9967***
	(0.0097)	(0.0033)	(0.0032)	(0.0083)
Workers	691,730	691,730	691,730	691,730
Plants	465,497	465,497	465,497	465,497
Worker-Year obs.	4,557,940	4,557,940	4,557,940	4,557,940
R-squared	0.1477	0.1480	0.7211	0.7211

Notes: Annual earnings are all earnings received by an individual from a given employer in a year. Full-time equivalent daily wages are calculated as the ratio of total annual earnings over the total number of days worked times the share of time worked in a year. Earnings and wages are expressed in Euros of 2016. Hours stand for the percentage of hours worked in a year relative to a full-time worker in the same establishment. Days refer to the number of days worked in a year. All these dependent variables are expressed in logarithms. Unemp. rate stands for one-year lagged national unemployment rate. Standard errors clustered at the plant-level in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Appendix 3.D Population coverage

The population data comes from Firms Registered in the Social Security system (*Empresas Inscritas en la Seguridad Social*, in Spanish) dataset. The data can be downloaded from `http://www.empleo.gob.es/es/estadisticas/contenidos/anuario.htm`.[46] This database provides aggregate information on the number of firms, and their workers, registered in the Social Security system as of December, 31st of each year.[47] The concept of firm is that of a legal entity based on its tax identifier. As for the number of workers, it is obtained relying on the Social Security contribution accounts (plants) for each firm. For each firm the total number of workers is computed by summing the individuals registered in each plant belonging to the same firm.[48]

Table 3.D1 compares the population data with the MCVL dataset. In the MCVL, I compute total employment aggregating plant-level employment, measured at the data extraction moment, in each of the employer categories and I count the number of firms contributing to that total employment stock.[49] The table reveals that the MCVL sample captures an important share of the stock of workers and firms in capital- and worker-owned firms, and replicates the evolution of employment over time in both employer categories.[50] In Table 3.D2, I compare the population data with the MCVL, but looking at workers' job spells. Specifically, employment stock and the number of firms is obtained

[46] To obtain each year of data one has to perform the following steps. Firstly, select the year of interest. Secondly, choose firms registered in the Social Security (*empresas inscritas en la Seguridad Social*). Thirdly, download the excel file(s) that refer to firms and workers by firm type (*empresas y trabajadores según tipo de empresa*).

[47] After 2012 there is no aggregate information available that allows to differentiate firms in Social Security according to its legal nature.

[48] This implies that workers can be counted more than once if they hold different jobs during the period. Thus, the total number of workers is better understood as worker-plant matches.

[49] In the case of multi-establishment firms, I only count one observation as in the population data, but I aggregate the size observed in different establishments.

[50] The stock of employment and firms picked its maximum in 2007 in the population data, whereas its 2008 in the MCVL. This delay is explained by differences in the reference moment, employment and firms are measured on the last day of December of each year, whereas in the MCVL using plant-level information this occurs at the data extraction moment, i.e. March/April following the reference year of the MCVL.

by looking at workers employed on the last day of December each year. Altogether, the comparison suggests that the MCVL mirrors the evolution of employment in both employer categories and, hence, it is a suitable source to study cyclicality differences between capital- and worker-owned firms.

Table 3.D1: Population coverage of MCVL (sample of employers)

A.SS	**Workers**			**Firms**			**Workers/Firm**	
Year	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives
2005	9,429,117	220,903	2.34	682,539	16,089	2.36	13.81	13.73
2006	9,913,490	224,774	2.27	709,863	15,707	2.21	13.97	14.31
2007	10,199,125	227,357	2.23	727,870	15,361	2.11	14.01	14.80
2008	9,407,048	218,273	2.32	694,868	14,372	2.07	13.54	15.19
2009	8,758,787	208,590	2.38	653,524	13,497	2.07	13.40	15.45
2010	8,628,151	207,512	2.41	635,620	13,043	2.05	13.57	15.91
2011	8,451,762	203,653	2.41	615,388	12,488	2.03	13.73	16.31
2012	7,985,569	197,772	2.48	591,186	11,804	2.00	13.51	16.75
B.MCVL	**Workers**			**Firms**			**Workers/Firm**	
Year	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives
2005	7,634,959	155,037	2.03	309,022	7,398	2.39	24.71	20.96
2006	7,844,369	156,188	1.99	313,043	7,339	2.34	25.06	21.28
2007	8,105,688	170,864	2.11	337,860	7,602	2.25	23.99	22.48
2008	8,335,494	174,107	2.09	352,979	7,630	2.16	23.61	22.82
2009	7,654,670	170,771	2.23	347,331	7,437	2.14	22.04	22.96
2010	7,246,051	164,103	2.26	332,741	7,141	2.15	21.78	22.98
2011	7,104,477	164,006	2.31	324,595	6,969	2.15	21.89	23.53
2012	6,937,636	160,024	2.31	314,796	6,760	2.15	22.04	23.67
2013	6,585,533	154,573	2.35	300,578	6,444	2.14	21.91	23.99
2014	6,597,580	155,833	2.36	292,353	6,269	2.14	22.57	24.86
2015	6,859,117	160,038	2.33	293,323	6,219	2.12	23.38	25.73
2016	7,112,772	163,804	2.30	298,944	6,202	2.07	23.79	26.41
C.MCVL/SS	**Workers**			**Firms**				
Year	Capitalists	Cooperatives		Capitalists	Cooperatives			
2005	80.97	70.18		45.28	45.98			
2006	79.13	69.49		44.10	46.72			
2007	79.47	75.15		46.42	49.49			
2008	88.61	79.77		50.80	53.09			
2009	87.39	81.87		53.15	55.10			
2010	83.98	79.08		52.35	54.75			
2011	84.06	80.53		52.75	55.81			
2012	86.88	80.91		53.25	57.27			

Notes: Stocks in the population refer to workers and firms registered as of Dec, 31st of each year. Stocks in the MCVL refers to the extraction data of each t MCVL wave (March/April of $t+1$). Cooperative's employment in the population data includes both partners and conventional employees. Employment in the MCVL is calculated as the sum of the point-in-time (data extraction moment) employment stock at the plant level. Panel A shows the population of workers and firms registered in the Social Security system. The population data come from the *Firms Registered in the Social Security* database produced by the Spanish Ministry of Employment and Social Security. Panel B data based on own calculations from the *Muestra Continua de Vidas Laborales*. Panel C shows the ratio of workers and firms included in the MCVL sample of plants to their population counterparts. Ratios are expressed in percent.

Table 3.D2: Population coverage of MCVL (sample of workers)

A.SS	**Workers**			**Firms**			**Workers/Firm**	
Year	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives
2005	9,429,117	220,903	2.34	682,539	16,089	2.36	13.81	13.73
2006	9,913,490	224,774	2.27	709,863	15,707	2.21	13.97	14.31
2007	10,199,125	227,357	2.23	727,870	15,361	2.11	14.01	14.80
2008	9,407,048	218,273	2.32	694,868	14,372	2.07	13.54	15.19
2009	8,758,787	208,590	2.38	653,524	13,497	2.07	13.40	15.45
2010	8,628,151	207,512	2.41	635,620	13,043	2.05	13.57	15.91
2011	8,451,762	203,653	2.41	615,388	12,488	2.03	13.73	16.31
2012	7,985,569	197,772	2.48	591,186	11,804	2.00	13.51	16.75
B.MCVL	**Workers**			**Firms**			**Workers/Firm**	
Year	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives	Ratio	Capitalists	Cooperatives
2005	341,869	7,659	2.24	142,835	3,383	2.37	2.39	2.26
2006	338,068	7,631	2.26	135,705	3,169	2.34	2.49	2.41
2007	348,182	7,843	2.25	139,175	3,184	2.29	2.50	2.46
2008	331,929	7,526	2.27	131,700	2,967	2.25	2.52	2.54
2009	311,398	7,407	2.38	123,176	2,817	2.29	2.53	2.63
2010	305,243	7,222	2.37	119,119	2,634	2.21	2.56	2.74
2011	294,434	7,097	2.41	113,657	2,613	2.30	2.59	2.72
2012	281,578	6,505	2.31	106,756	2,442	2.29	2.64	2.66
2013	273,979	6,786	2.48	103,104	2,393	2.32	2.66	2.84
2014	280,383	6,879	2.45	104,442	2,349	2.25	2.68	2.93
2015	292,638	7,182	2.45	108,971	2,427	2.23	2.69	2.96
2016	306,736	7,031	2.29	114,046	2,458	2.16	2.69	2.86
C.MCVL/SS	**Workers**			**Firms**				
Year	Capitalists	Cooperatives		Capitalists	Cooperatives			
2005	3.63	3.47		20.93	21.03			
2006	3.41	3.39		19.12	20.18			
2007	3.41	3.45		19.12	20.73			
2008	3.53	3.45		18.95	20.64			
2009	3.56	3.55		18.85	20.87			
2010	3.54	3.48		18.74	20.19			
2011	3.48	3.48		18.47	20.92			
2012	3.53	3.29		18.06	20.69			

Notes: Stocks refer to workers and firms registered as of Dec, 31st of each year. Cooperative's employment in the population data includes both partners and conventional employees. Employment in the MCVL is calculated as the total number of workers employed on Dec, 31st of each year. Panel A shows the population of worker-plant matches and firms registered in the Social Security system. The population data come from the *Firms Registered in the Social Security* database produced by the Spanish Ministry of Employment and Social Security. Panel B data based on own calculations from the *Muestra Continua de Vidas Laborales*. Panel C shows the ratio of workers and firms included in the MCVL sample of workers to their population counterparts.

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