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ABSTRACT

This paper studies the impact of severance pay generosity on workers' voluntary mobility decisions. The identification strategy exploits a major labor market reform in Spain in February 2012 together with the exposure of some workers to a layoff shock. 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 expected to be displaced in the near future more likely to voluntarily leave their employers. The results indicate that policies targeting employers may also affect workers' behavior. They further reveal the relevance of taking into account interactions between employment protection and unemployment insurance.

Keywords*:* Employment protection, Severance Pay, Job mobility, Quits, Plant closures, Mass layoffs.
JEL codes: J62, J63, J65.

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. An extensive literature points to employment protection legislation as one of the most relevant institutions hindering labor reallocation (Bentolila and Bertola, 1990; Lazear, 1990; 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 labor relationships (Kugler and Pica, 2008; Garibaldi and Pacelli, 2008; Marinescu, 2009). Hiring decisions are also affected if employers anticipate future costs associated with workforce adjustments (Kugler and Pica, 2008; Cahuc, Malherbet, and Prat, 2019).

In practice, one of the key components of employment protection legislation raising firms' adjustment costs are severance packages —lump-sum transfers from the firm to the worker upon dismissal (OECD, 2013b). These transfers increase the opportunity cost of job quit, thereby making workers more reluctant to voluntarily leave their employers to avoid losing the benefits they are entitled to (Mortensen, 1977; Burdett, 1978; Mitchell, 1982; Light and Omori, 2004). This suggests an additional channel through which employment protection may alter labor reallocation, i.e. workers' mobility out of the firm. 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 paper, I exploit a major labor market reform in Spain to investigate the link between employment protection legislation and workers' voluntary mobility.

The Spanish labor market reform was implemented in February 2012. The reform substantially changed the provisions of the employment protection legislation for permanent contracts by reducing monetary compensations for unfair dismissals, reshaped the definition of economic redundancies, and removed the requirement of administrative authorization for collective dismissals. The goal of this paper is to exploit the reduction in the stringency of employment protection induced by the 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 pay entitlements. In my identification strategy, I exploit this variation to study the effect of severance pay on workers' voluntary mobility.

Voluntary mobility is not only determined by severance pay entitlements, but other factors like labor demand or supply decisions. To disentangle the reform effect from confounding factors, the ideal scenario would be to compare two groups of workers who face similar determinants of their labor mobility decisions, one of them affected by the reform (treatment) and the other 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 employed individuals and, thus, it is not possible to identify a comparable group of workers that 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 jobs. This strategy allows me to find a group of workers who experience a (plausible) exogenous layoff shock and for whom severance pay is the main mobility cost in the form of foregone income. One could then expect a larger behavioral response from these workers compared 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 thought experiment consists of modeling the event history of individuals during their job spells up to job quit. In this 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. Treatment effects are then 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 empirical analysis points to a behavioral response of workers to severance pay generosity. In fact, the results indicate that the job quit hazard rate for workers who might expect to be displaced in the near future increased by roughly 17.5 percent after the policy change compared to workers not affected by such layoff shock. This shift in the job quit hazard rate is larger for workers who were more exposed to the reform, implying that mobility costs are an important source hindering workers' voluntary mobility.

Under my identification strategy, the identified reduced form parameter quantifies the effect of severance pay generosity on workers' voluntary mobility that arises from both labor supply and demand forces. The evidence is thus consistent with a labor market model with search frictions and separations that are either employee-initiated (quits) or employer-initiated (layoffs). In this class of models, severance pay distorts workers' voluntary mobility decisions by directly affecting the expected utility of being laid-off: the value of unemployment. In addition, by decreasing the expected value of posting a vacancy, lower firing costs may further ease workers' voluntary mobility through its impact on labor demand.

This paper adds to different strands of the literature. Firstly, the study contributes to the empirical literature on employment protection legislation and labor mobility by shifting the focus of the analysis from the impact on firms' decisions to the effect on workers' behavior. Closely related to this paper 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 cross-country evidence that job quality is an important determinant of quits, but the strictness of the employment protection affects this relation: lower employment protection levels translate into higher quit rates. The current analysis complements this study by exploiting within-country variation in the levels of the stringency of employment protection to provide a causal estimate of the effect of severance pay on workers' voluntary mobility.

Kettemann, Kramarz, and Zweimüller (2017) build 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. In 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. In this new system, workers are 60 percent more likely to make a job-to-job transition. Contrary to Kettemann, Kramarz, and Zweimüller (2017), I exploit a labor market reform that creates exogenous variation across job spells to provide direct evidence provides direct evidence on the role of the stringency of employment protection on workers' quit behavior. In addition, the dynamic nature of my empirical strategy allows me 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, the current 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). The dynamic approach also permits me to consider only job spells that started before the reform moment. This is key in the current setting, as the matching process between firms and workers may change as a consequence of the reform and, hence, affect the future development of new labor relationships beyond severance pay entitlements.

This paper also contributes to our understanding of the effect of job loss insurance schemes on worker mobility. Empirical research on this issue has mostly focused on the role of unemployment insurance on job turnover (Light and Omori, 2004; Rebollo-Sanz, 2012; Martins, 2016; Albanese, Ghirelli, and Picchio, 2019). These studies show that eligibility and generosity of unemployment benefits distorts the utility associated with future unemployment, thereby deterring job quits and increasing layoff rates once the eligibility condition is met. In this paper, I show that the stringency of the employment protection in the form of severance pay levels also has an impact on workers' job quit behavior similar to that of unemployment benefits. This revealed relationship further points to the relevance of jointly designing employment protection legislation and the unemployment insurance system, as they insure the same type of risk (Blanchard and Tirole, 2008; Parsons, 2014)

Finally, an extensive literature looking at the consequences of job loss relies on mass layoff and plant closing events to show that (exogenously) displaced workers incur in large and long-lasting losses related to lower re-employment probabilities and future earnings (Jacobson, LaLonde, and Sullivan, 1993; Davis and von Wachter, 2011; Lachowska, Mas, and Woodbury, 2018). By using large employment contractions in the empirical strategy, this paper adds to the sizeable literature on job displacement by showing that the prevalent institutional setting affects the composition of workers who are ultimately displaced. In particular, my results suggest that severance pay generosity affects the selection process of workers before the employer event, which determines the costs of job displacement (Lengermann, Vilhuber, et al., 2002; Schwerdt, 2011).

The rest of the paper is organized as follows: Section 2 describes the institutional setting. Section 3 outlines the identification strategy and identification assumptions. Section 4 provides a description of the data. Section 5 discusses the implementation of the empirical strategy and the results. Section 6 concludes.

2 Institutional setting

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.

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). 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

[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.

maximum of 12 monthly wages. Objective grounds include negative economic conditions ("actual or expected losses or loss of competitiveness"), technical, productive or organizational reasons, as well as employee's misbehavior. Collective dismissals fall under the same regulation as "fair" dismissals, but employers must also obtain administrative authorization and have the obligation of good-faith negotiations with unions before undertaking them.[2] Importantly, a worker fired for a fair reason could subsequently sue the firm and a legal process would begin, where employers must pay interim wages between the layoff date and the final court ruling.[3] This led many employers to 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 procedural costs.

2.2 The 2012 policy change

On February 12th, 2012, the Spanish Government passed an unexpected and deep labor market reform in the middle of a double-dip recession.[4] The reform modified important aspects of firing and hiring procedures to boost employment creation and reduce the existing gap in employment protection levels between open-ended and fixed-term contracts.[5]

The policy change reduced monetary compensation for unfair dismissal to 33 daily wages per year of tenure, with a maximum of 24 monthly wages, resulting in a decline of 12 daily wages per year of tenure. Noteworthy, this reduction in severance pay was not discontinuous: all individuals employed at the moment of the reform were affected. Workers hired after the reform date are subject to this new regulation, whereas those hired before the policy change are under a dual regime wherein the amount of severance pay is proportional to the length of the employment spell before and after the legislative

[2] A dismissal of the entire workforce also falls under the collective dismissal rules if more than 5 employees are affected. In the case of business shut-down, the firm must deplete all their resources to secure workers' severance pay entitlements. If the company provides reliable information on the inability of meet such obligations, severance payments will be funded by the Social Security administration through a wage guarantee fund (FOGASA).

[3] The ambiguity of the term "negative economic conditions" left judges with a great deal of discretion, and they often re-ruled as unfair most dismissals that ended up in court (Jimeno, Martínez-Matute, and Mora-Sanguinetti, 2020).

[4] Appendix 2.3 provides an overview of the labor market situation in Spain between 2005 and 2018.

[5] Garcia-Perez and Domenech (2019) evaluate the impact of the reform on unemployment inflows and outflows. Their findings point to a mild but positive effect of the reform increasing unemployment outflows, reducing the layoff probability of workers under fixed-term contracts, and easing the transition rate from temporary to permanent contracts.

change.

Severance pay for fair dismissals remained unchanged. However, the new legislation introduced in the definition of negative economic reasons 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", removed the worker's right to interim wages during any legal procedure, and abolished the requirement of administrative authorization to carry out collective redundancies. Generally speaking, the reform sought to limit the administrative intervention to verify the existence of objective causes for dismissal and compliance with the procedural rules.[6] Consequently, the improvement in the definition of objective causes might have translated into a larger reduction in severance payments if it increased the likelihood that employers carry out economic redundancies.

The policy change also modified other aspects of labor market legislation that can influence worker mobility and the job matching process. On the one hand, a new permanent contract for firms with fewer than 50 employees was introduced. This new contract includes several hiring incentives and fiscal rebates, and allows for a probationary period of one year.[7] On the other hand, the reform aimed to align labor costs more closely with firm idiosyncratic productivity, by giving priority to agreements at the 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.[8]

Unfortunately, estimating the effect of each element separately is not feasible, as all the provisions took place at the same time and most of these changes affected all workers and firms in the same way. The identification strategy is thus designed to mitigate the influence of these additional legislative changes. However, it is important to bear in mind that the estimated effect ultimately captures the impact of all provisions included in the labor market reform on workers' voluntary mobility decisions.

[6]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 are not required to assess whether the employer's decision is an appropriate managerial decision.

[7]The reform also extended the existing subsidy equivalent to 40% of ordinary severance pay (8 days per year of service, paid by FOGASA) to all cases of fair dismissal in the case of firms with fewer 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.

[8]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.

2.3 The Spanish labor market, 2005-2018

Table A.1 summarizes key features of the Spanish labor market situation over the period under analysis. One can clearly differentiate three sub-periods of economic growth and contraction.[9] The deterioration of the economic activity with the onset of the Great Recession in 2008 translated into massive employment losses that led the unemployment rate to skyrocket from a historically low 8 percent in 2007 to 26 percent in 2013. By the end of 2013, economic conditions started to improve, leading to continuous employment gains. Moreover, both the quit rate and the fixed-term contract rate exhibit a marked pro-cyclical behavior. Importantly, the evidence suggests that the decrease in firing costs induced by the 2012 reform did not contribute to reducing the duality of the Spanish labor market.[10]

Figures A.2 and A.3 reveal that firing costs as well as severance payments recognized by labor courts exhibit a hump-shaped evolution over the period under analysis, increasing during the recessionary period and showing a significant decrease from 2012 onwards. Moreover, the reform may also have affected the type of firm's response to negative shocks. Firstly, the reform reduced firms firing costs, which might translate into a lower firm exit rate (Blanchard and Portugal, 2001). Secondly, it considerably eased the requirements to carry out collective redundancies. Thirdly, it introduced the possibility of implementing changes in working conditions, which can reduce the need for layoffs. Notice, however, that this latter provision could also affect voluntary mobility, if changes in working conditions translate into higher on-the-job search effort. Figures A.4 and A.5 show the counter-cyclical movement of both firm exit and collective redundancies. Similarly, Figure A.6 shows that newly registered unemployment benefits' recipients who were fired due to economic reasons by their employers also exhibit a counter-cyclical evolution.[11]

[9] 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.

[10] Figure A.1 shows that the percentage of newly signed temporary contracts over the all new contracts each month did not change after the policy change, which further suggests that the reform did not reduce the labor market duality.

[11] Jimeno, Martínez-Matute, and Mora-Sanguinetti (2020) find some evidence of an increase in proportion of dismissals being ruled as fair by labor courts after the labor market reform.

3 Identification strategy

3.1 Evaluation framework

Individual event history. Consider an individual starting a (permanent) job at a given calendar time (t_0) who might experience different events at any time during her spell. At the moment of job exit, the worker belongs to a specific state determined by the occurrence of the events of interest. To identify the effect of severance pay on workers' quit decisions, the strategy is to model the event history of individuals during their job spell up to job quit. In this setting, identification relies on the availability of the dates of the occurrence of the relevant events in the data. At those dates, the job quit 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).[12]

Events of interest. Two main events are considered to identify the effect of severance pay generosity on workers' quit behavior. The first event of interest is the February 2012 Spanish labor market reform (r). The reform cut severance pay entitlements for all workers employed under permanent contracts from the reform moment onwards. As the legislative change took place at a fixed point in time, workers are differently affected over their job spells by the reform based on their hiring date. This feature creates exogenous variation in severance pay entitlements across job spells at a given realized duration in the job spell beyond tenure and wages. This variation in entitlements is used to identify the effect of severance pay on workers' mobility decisions.

The second event is an exogenous information shock to workers about individual job loss probabilities (s). 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 main mobility cost.[13] The shock should hence trigger a decision from this group of workers on whether to leave voluntarily, or wait to be dismissed and collect severance pay. Throughout the paper, I label this information shock about future layoff as *layoff shock*. To define the layoff shock, I follow early literature on job displacement (e.g. Jacobson,

[12] This type of approach has been mainly 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, Pohlan, and Uhlendorff, 2017).

[13] For instance, firm-specific capital may represent another type of mobility cost. However, this and other types of mobility costs converge to zero as the layoff probability increases.

LaLonde, and Sullivan, 1993; Davis and von Wachter, 2011) and exploit mass layoffs and plant closings to identify (plausible) exogenous firm-specific conditions affecting workers' expectations of losing their job. Then, I assume a worker becomes aware of the upcoming event one year in advance, in line with evidence on job loss expectations and workers' behavioral responses (Stephens, 2004; Hendren, 2017).[14] The strategy is to model labor mobility over the year prior to the start of the large employment contraction to identify the mobility response of workers to severance pay generosity.

The policy change and the layoff shock are taken together to identify the role of severance pay generosity on workers' mobility decisions. Specifically, both events are combined to isolate the effect of severance pay generosity from labor demand and supply factors that can drive 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. Thus, the decrease in severance pay induced by the reform should generate a higher probability of workers voluntarily leaving their employers compared to workers not affected by the layoff shock.

Reduced-form specification. 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 distributions 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 characterized by its hazard rate, $h(t|s, r, X, V)$.[15] The transition probability is assumed to vary with individual heterogeneity as well as the elapsed job duration t. Additionally, the probability of voluntarily leaving the job depends on treatment status (r, s, $r \times s$, or none of the above) at time t. The reduced-form specification for the job quit hazard rate at date t conditional on (s, r, X, V) is then

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

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

[15]Roughly, 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.

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).[16] 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 compared to 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 of workers who are not affected by the layoff shock, before and after the occurrence of the policy change.

Equation 1 has a mixed proportional specification except for the components related to the incidence of a given event.[17] More precisely, observed characteristics and the unobserved heterogeneity term are assumed to act multiplicatively on the baseline hazard, whereas the effect of a given event works on the hazard rate from the moment the event is realized onwards. With variation over time in observed characteristics the proportional hazard assumption is not crucial for identification, as time-varying covariates act as exclusion restrictions in the selection process (Brinch, 2007; Gaure, Roed, and Zhang, 2007). Intuitively, workers with the same observed characteristics in period t but different values in the past should only have a different transition probability if the composition with respect to unobserved heterogeneity is unequal. Identification of the effect of the events of interest requires additional key assumptions on concerning the treatment assignment process and the forward-looking behavior of individuals. These assumptions are discussed below.

3.2 Identification assumptions

No selection of treatment assignment. Individuals are assumed to be affected by the policy change or the layoff shock randomly. In other words, there is not an endogenous selection of workers into any of the events considered. Treated individuals are then

[16]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.

[17]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) discuss identification of dynamic discrete time models.

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 beforehand. 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.[18] In sum, this assumption implies that treatment effects can be captured by exogenous time-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.[19] 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). The randomness in the moment the layoff shock is realized arises due to variation in the moment when the firm event occurs. Similar to the case of the reform, the fact that the layoff shock is realized at different 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

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

[19]Note that spells starting after a given event is realized are assigned to treatment in a deterministic way. Thus, the estimation only considers spells initiated before a given event to guarantee that the moment of treatment assignment is random.

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, 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).[20] In my setting, anticipation of the policy change is unlikely to occur, as the reduction in severance pay for permanent contracts was not foreseen by workers (OECD, 2013a). 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. To rule out this possibility, the strategy considers a year prior to when the large employment contraction begins to determine the moment from which a worker is affected by the layoff shock.[21] Identification of the main effect of interest relies thus on the assumption of no pre-treatment effects: in the absence of the reform, differences in the job quit hazard rate between workers who are affected by the layoff shock and those who are not affected by it would have remained constant.

4 Data

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 and linked to the Residents' Registry and Tax Records since 2005 up to 2018.[22] The MCVL is a representative 4 percent

[20]The no-anticipation assumption does not exclude that individuals know the probability distribution of future events conditional on observable and unobservable characteristics.

[21]Figures A.7 shows that plants do not start downsizing until around the year before the start of the observed plant event. This supports the strategy of beginning a year prior to the onset of employment contraction to identify potential information flows about the current situation of the employer that may affect workers' expectations about job loss and, hence, trigger their mobility decision.

[22]The first version of the MCVL corresponds to 2004. This wave is discarded as most of the information structure differs from that available for subsequent years.

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 who subsequently remains registered with the Social Security administration remains as a sample member. Additionally, the dataset is refreshed each year, and it remains representative of both the stock and flows 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 2018. Importantly, the MCVL provides precise information on the reason of termination of each labor relationship, as it is compulsory to report the type of separation to the Social Security administration to determine whether the worker qualifies for unemployment benefits. This information allows me to differentiate between employer and employee initiated separations, i.e. layoff and quits, respectively. Thus, I can unambiguously identify voluntary separations that entail no legal severance payments. Worker, job, and employer characteristics are also observed for each employment spell.[26]

4.1 Employer events

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. 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, as employer information is observed at that level of disaggregation.[27] For each of the establishments, plant-size is observed annually at the

[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).

[24]Individuals who stop working remain in the sample while they receive unemployment benefits or other welfare benefits (e.g. retirement pension). Individuals leave the sample when they die 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 B provides a detailed description of the variables.

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

data extraction moment, y.[28] I organize the plant records in a yearly panel to exploit changes in plant-size between two consecutive years to identify the employer events.[29]

Plant closure. To be coded as a plant closure, 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 re-coded 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.[30] The closing month is then defined as the first month of the employment event year.

Mass layoff. Mass layoff events are defined following the Spanish collective dismissals regulation. To fall within the scope of this legislation, employers must plan to dismiss a minimum of 10 to 30 employees, depending on the size of the company, within a period of 90 days . The minimum thresholds for collective dismissals are: 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 with 300 or more employees. Thus, an employer experiences a mass layoff event if: (i) plant-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. Conditions (iii) and (iv) rule out temporary fluctuations in plant-level employment.[31] Similarly to closing plants, I re-code mass layoff plants if I observe 10 or more jobs being

[28]This implies that before 2005, plant-size information is not available. Moreover, if the plant is no longer in operation after 2005, the observed plant-size is zero.

[29]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 (around 8 percent of all plants exhibit at least one plant-size gap). 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.

[30]To minimize the impact of this reclassification, re-coded plants are left out of the analysis (roughly 4 percent of all plant closings).

[31]Given the widespread use of fixed-term contracts in the Spanish labor market, these conditions seek to minimize variations in plant-size driven by the amount of temporary worker.

created during the mass layoff year.[32] The mass layoff month corresponds to the first month of the employment event year.

Layoff shock. The layoff shock time window is considered to start one year before the firm event period begins. Hence, the large employment contraction takes place any time from the 13th month after the layoff shock time period starts.[33] Any worker still employed after the onset of the layoff shock period is thus assumed to be aware of an increase in her job loss probability due to the upcoming employer event.

4.2 Estimation sample

Sample restrictions. The initial sample consists of all job spells observed in private sector establishments belonging to the General Regime of the Social Security between 2005 and 2018.[34] This initial sample consists of 793,183 workers observed over 3,978,702 job spells. The following constraints are imposed to select the estimation sample.[35]

The analysis sample considers only establishments that ever had 5 or more employees between 2005 and 2018 (836,584 job spells discarded). At the moment of separation, workers must be (i) age 50 or younger, and (i) hold a permanent contract with more than six months of tenure (2,743,077 spells dropped). Condition (i) prevents the influence of early/partial retirement schemes on mobility decisions, whereas condition (ii) ensures that workers qualify for severance pay. Lastly, the estimation sample considers only job starters for whom the incidence of a given event occurs once they already qualify to collect severance pay (221,820 job spells deleted). Notice that this latter restriction is crucial for two reasons. Firstly, it ensures that workers do not start their jobs being already affected by a given event, i.e. workers experience the events randomly over their job spells. Secondly, it avoids including jobs created after the reform. This is key in the current setting as the matching process between firms and workers may have changed, thus affecting the future development of new labor relationships beyond severance pay entitlements.

[32] This implies a re-coding of 5.6 percent of plants which exhibit a mass layoff, which are excluded from the analysis.

[33] There are some cases that a mass layoff event occurs immediately before the plant closing event. In such situations, the events are treated as a unique, longer, event.

[34] Establishments linked to Special Regimes of the Social Security cover the primary sector and household activities, which exhibit different labor regulation.

[35] Table A.2 reports an overview of the effect of the constraints on relevant variables.

The constraints yield an estimation sample of 152,526 workers observed over 177,221 job spells starting between January 2005 and August 2011. In this sample, around 43 percent of the job spells are still observed after the reform date (February 2012), and 23 percent of them are affected by the layoff shock. Note also that some individuals may start more than one job over these period: I observe two or more job spells for around 10% of the individuals.

Descriptive statistics. Table A.3 presents summary statistics of observed characteristics for the whole sample and for workers who separately experience a given event. Characteristics are measured at job start. Around 45 of the workers are women. A large share of the sample is formed by Spanish workers (77 percent), and almost 22 percent of the workers hold a university degree. Workers are on average 31 years old at job entry. These workers were employed 58 percent of the time since they entered the labor market, and 46 percent start their new job after an non-employment spell. Regarding job characteristics, around 81 percent start a full-time job and roughly 16 percent start their job in a high-skill occupation. Average entry-level real daily wage is around 50 euros. Hiring establishments are mostly in the service sectors (73 percent), and 44 percent are located in the four biggest cities in Spain.[36] On average, these firms have been in business for 9 years and have around 33 employees. Importantly, there is 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. Finally, there are also 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 less likely to be found in the service sector and employ more workers. Figure A.7 also shows that plants start downsizing around one year before the large employment contractions. This suggests that the year prior the start of the plant-event is a sufficiently long time span to be capturing potential information

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

flows about the current situation of the firm that may affect workers' expectations about job loss and, hence, trigger their mobility decision.[37]

Job quit duration. Table A.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 24 months. There is also variation in the moment at which workers experience the policy change, with an average of 33 months after job start. This ample variation allows me to isolate the effect of the event of interest from duration dependence. Notice also that spells for which the incidence of a given event is observed are consistently longer than spells not affected, as it takes time before the event is realized.

Job quits around the reform date. Figure A.10 shows the time series of quit flows for no layoff shock and layoff shock workers' groups. The flows is the number of quits (voluntary separations) observed each each year divided by the total number of workers observed that year. In the case of workers affected by the layoff shock, quit flows are computed over the year before the large employment contraction starts. The figure shows that quits decreased for both groups of workers as aggregate conditions started to worsen after 2008. Importantly, the evolution of both series reveal that the quits follow similar paths for both groups before the occurrence of the policy change (vertical line), suggesting the absence of pre-treatment/anticipation effects. Around the reform date, the paths seem to diverge. However, these rates do not account for any of the observed composition differences 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 and take into account right-censoring of the spells. This point is key in the evaluation framework, as the likelihood of quitting decreases with time on the job and only workers hired before the reform date are considered. The duration model allows to account for this issue and to accommodate right-censored spells in a natural way.

[37] There are still differences between the evolution of plant-size between mass layoffs events and plant closings (see Figures A.8 and A.9): mass layoffs seem to be more of a sudden event, whereas plant closings tend to be preceded by a "shadow of death" (Fackler, Müller, and Stegmaier, 2018).

5 The impact of severance pay on voluntary mobility

5.1 Estimation of a discrete time duration model

To implement the identification strategy discussed in Section 3, the spell data is expanded so that the spell length of each individual determines a vector of binary responses. Thus, I estimate a reduced-form discrete time duration model, where the individual job quit hazard rate is specified to take the complementary log-log link form (Jenkins, 2005).[38]

To balance observed differences in the length of job spells and to ensure 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 84 months. Thus, only quits occurring up to the 84th month contribute to identify the effect of interest. In the case of workers affected by the layoff shock, observations are right-censored at the moment the large employment contraction begins. In other words, only observations within the first 12 months after the onset of the layoff shock are relevant for identification.

Hazard rate. The individual job quit hazard rate is given by[39]

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

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 compared 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. $\lambda(t)$ represents the baseline hazard, specified to be piecewise constant with cut-off points selected to match the Q_T-quantiles of the observed job

[38] The model fits the discrete time analogue to the continuous time proportional hazards model when the data is grouped at monthly intervals from daily frequency durations (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.

[39] When estimating the job quit hazard rate, I treat layoffs and other types of separation as right-censored.

quit duration ($Q_T = 10$).[40] The baseline hazard thus accounts for the fact that job exit probabilities change over time spent employed, capturing the (negative) duration dependence pattern of job quit duration.[41] Importantly, the nature of the layoff shock implies that the composition of the workforce changes after its realization. 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 (time-varying) characteristics including gender, age, a dummy for college graduates, the share of time employed since labor market entry, the immediately prior employment state (3 categories: non-employment, temporary job, permanent job), indicators for full-time job and high-skill occupations, (log) real daily wages, employer size and age, and sector of activity (6 industries).[42] The estimation also accounts for demand side effects through the quarterly provincial unemployment rate and a nation-wide monthly economic activity index. Hiring year fixed effects are also included to control 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).[43]

V stands for the unobserved heterogeneity term. Differences between groups of individuals at different times then reflect behavioral differences as well as selection effects. In the current framework workers affected by a given event stay, on average, longer than those not affected. Thus, 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 might not be a random (Lancaster, 1990). Unobserved determinants are specified as random effects, which are assumed to follow a Gamma distribution (Abbring and van den Berg, 2007).

[40] 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.

[41] Figures A.11 to A.13 report empirical hazard rates.

[42] The dataset provides a natural starting point of each individual labor market history, thereby allowing to introduce past labor market outcomes as explanatory variables in the model (van den Berg, 2001).

[43] Recall that the period under analysis embeds a full economic cycle with enough variation in aggregate conditions over time and within workplace locations that allow me to isolate the reform effect from varying macroeconomic factors.

5.2 Job quit decisions

Table A.5 presents the main estimation results on the effect of the reduction in severance pay generosity induced by the 2012 Spanish labor market reform on the job quit hazard rate. The findings indicate a positive effect of the policy change on the job quit hazard rate of workers affected by the layoff shock compared to individuals who did not experience such a shock. Specifically, the estimate of the treatment effect in Column 1 in Table A.5 indicates that the job quit hazard rate increased by 17.5 percent after the policy change.[44] In Column 2, I rely on a more restrictive definition for voluntary mobility by considering only job quits that end up in a job-to-job transition within a month after separation. This finding points to a slightly stronger effect, suggesting that the increase in job quits after the reform is driven by workers being more likely to switch employers in the face of an exogenous increase in their layoff probability.

To shed light on the effect of severance pay generosity on workers' voluntary mobility decisions, I investigate the heterogeneity of the results with respect to treatment intensity. Column 3 reports the estimates of treatment effect differentiating workers who are affected by the layoff shock shortly after the reform (short-run: 2012 or 2013) and those individuals affected after 2014 (medium-run). Column 4 reports heterogeneous treatment effects based on the time employed after the reform compared to the overall job duration: low incidence (less than 30 percent of the time) vs high incidence (more than 30 percent of time). The findings from both specifications indicate that the treatment effect increases with time after the reform. In other words, workers who experience a larger drop in severance pay entitlements (mobility costs) exhibit a larger probability of voluntarily leaving their employer.

To illustrate the importance of the effect, I perform a simple exercise to compute the elasticity of job quit with respect to severance pay generosity. The measured elasticity is given by the estimated shift in the job quit rate (17.5 percent) divided by the average severance pay decrease experienced by job quitters affected by the layoff shock. To compute this reduction in severance payments, I assume that the behavioral change of workers is driven by the change in the severance pay linked to "unfair dismissals" (from 45 days per year of tenure to 33 days per year of tenure). This implies that the computed elasticity would be an upper bound, provided that the reform may have also eased the

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

ability for firms to carry out economic dismissal that entail lower firing costs. Under this assumption, the average time a job quitter spent employed after the reform was 29 months out of a total average duration of 60 months. This implies a 11.5 percent decrease in severance pay entitlements (or around 1500 euros) compared to a similar worker who spend the 60 months employed during the pre-reform period. The estimated change in the job quit hazard rate along with the average reduction in severance pay imply an elasticity of 1.52: for each 1 percent decrease in severance pay generosity, equivalent to roughly 2.5 daily wages decrease, the job quit hazard rate increases by 1.52 percent.

The results are in line with previous empirical work that found a negative link between severance pay and voluntary mobility (Gielen and Tatsiramos, 2012; Kettemann, Kramarz, and Zweimüller, 2017). The effect is however significantly smaller than in Kettemann, Kramarz, and Zweimüller (2017), who analyzed the substitution of the severance pay system in Austria by an occupational pension scheme. Thus, one could expected a lower mobility response of workers, as the 2012 Spanish labor market reform only reduced the opportunity cost of job quit by reducing severance pay generosity, whereas the occupational pension scheme completely removed such opportunity cost.

The findings can be rationalized by a simple on-the-job search two-period model with separations that take the form of either employee-initiated (quits) or employer-initiated (layoffs) in the spirit of Light and Omori (2004). In the first period, all workers are employed and endowed with non-work hours that can devote to look for a new job (search effort) or to leisure. A worker choosing to search for a new job in period 1 receives an outside job offer, but incurs in a cost in the form of foregone leisure. At the end of the first period, individuals face the probability of receiving an exogenous layoff notice. In the second period, workers may be in three different states: employed in the starting job, switch to a new job (job quitters), or become unemployed (laid-off workers). If a worker is laid off, she is entitled to severance pay, which is determined by her tenure and wage level in the firm. In this model, severance pay affects workers' voluntary mobility decisions by directly affecting the expected utility of being laid off: the value of unemployment. The positive effect of the reduction in severance pay generosity on workers' voluntary mobility arises from two forces due to the decrease of the value of unemployment: (i) an increase in on-the-job search effort, increasing the probability of receiving a job offer, and (ii) a decrease in reservation wages, making workers more likely to accept a job offer and switch to a new employer.

A complementary explanation is linked to the potential impact of the reform on labor demand. Lower severance payments, and therefore firing costs, may further ease workers' voluntary mobility by increasing both the arrival rate and the distribution of job offers. The decrease in firing costs may increase the expected value of posting a vacancy, which would translate into a higher job offer arrival rate. At the same time, if wages were negatively affected by firing costs (Leonardi and Pica, 2013), the decrease induced by the reform would shift the wage-offer distribution to the right, making workers more likely to move to a new job as the opportunity cost of becoming unemployed increases. Thus, the identified reduced form parameter quantifies the effect of severance pay generosity on workers' voluntary mobility that arises from both labor supply and demand forces.

5.3 Sensitivity analysis

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 mass layoff. To analyze this issue, I perform two complementary exercises. Firstly, I use matching as a selection mechanism. More precisely, for each job started in a firm experience a large employment contraction, I look for exact matches in terms of the following characteristics: hired in the same quarter, same gender, same education level, same industry, same location, and by a firm in the same quartile of the plant-size distribution. If there are multiple matched subjects, I take the one with the nearest propensity score based on workers' age, employment history, and previous employment state. This criterion allows me to find a valid pair for 81 percent (Column 1). I then repeat the exercise but only look for valid controls for individuals who are ultimately affected by the layoff shock. This criterion yields a valid control for almost 85 percent of the workers (Column 2).[45]

Secondly, I rely on inverse probability weights to account in the benchmark model for differences in the probability of workers being affected by a given event (Hirano, Imbens, and Ridder, 2003). On the one hand, the weights are given by the predicted probability an individual starts a job in a firm that is bound to fail or experience a mass layoff using as a explanatory variables the quarter of hiring, gender, education, dummies for

[45]Tables A.6 reports observed characteristics for the matched samples.

industry, province, and quartile of the size distribution (Column 3). On the other hand, for each realized duration, I estimate the probability that an individual is affected by the layoff shock using as predictors gender, age, a dummy for college graduates, share of time employed since labor market entry, and the immediately prior employment state, indicators for full-time and high-skill occupations, (log) real daily wages, plant size and age, categorical variables for industry, and year of job start, the quarterly provincial unemployment rate, and the monthly national activity index. Then, I use these duration-specific predicted probabilities as inverse probability weights in the benchmark model (Column 4). Table A.7 compares the point estimates of the treatment effect between the benchmark sample and the different specifications described above. The results suggest that worker selection does not compromise the identification strategy.

Layoff shock arrival moment. The identification strategy relies on the assumption that the arrival of information regarding the upcoming employer event (layoff shock) is realized one year before the large employment contraction starts. This choice is based on previous literature on workers' expectations about future job loss (Stephens, 2004; Hendren, 2017). However, one year is still an arbitrary number. To test the validity of the results to the time horizon assumed, I repeat the original experiment, but using alternative time lengths to define the moment when the layoff information shock is realized. The results in Table A.8 indicate that the treatment effect varies smoothly around the benchmark starting point. However, the results also show that the effect increases, the closer is moment assumed for the arrival of the information shock to the start of the firm event. This may imply that the narrower the period, the more likely it is to capture the realization of some decisions made by workers earlier in time, which may upward bias the treatment effect. Altogether, the findings are indicative of non-anticipation effects.

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 A.10 already suggests 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 perform a series of placebo tests that "anticipate" the actual reform date. Table A.9 shows the results for a selected set of placebo reform dates. The point estimate for the effect of interest is at most 18 percent of the actual reform effect, and is never significantly different from zero. This is direct evidence of no pre-treatment effects.

Additional robustness checks. When estimating the job quit hazard rate, I assume a complementary log-log link for the hazard rate, and 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. Table A.10 shows the sensitivity of the results to (i) assuming a logit specification for the job quit hazard rate, (ii) a competing risk model using quits and layoffs as competing events of employment outflows with a shared unobserved heterogeneity component, and (iii) a competing risk model with separate but correlated unobserved heterogeneity terms. The results show that the main parameter of interests remains virtually unchanged under any of these alternative specifications.

The benchmark model introduces unobserved heterogeneity by means of job-level random effects, which are assumed to follow a Gamma distribution. In Table A.11, I test the sensitivity of the treatment effect of interest to alternative assumptions. Firstly, I show in Column (2) that the exclusion of unobserved factors in the benchmark model does not have a large impact on the identified reduced form parameter. This is due to (i) the flexible form assumed for the baseline hazard paired with a rich set characteristics used, and (ii) exploiting job spells that are interrupted by the policy change mitigates the impact of unobserved factors in the identification of the parameter of interest, as pointed out by van den Berg, Bozio, and Costa-Dias (2018). Secondly, the results are robust to different levels at which the unobserved components are shared, i.e. worker, plant, or firm. Finally, I also re-estimate the model by estimating the unobserved heterogeneity distribution non-parametrically. In particular, I follow Heckman and Singer (1984) and use a discrete mixture distribution with two mass points to summarize the unobserved heterogeneity. Altogether, the results indicate that the assumption regarding how the unobserved components are modeled is not crucial to identify the effect of interest.

In Table A.12, I test the robustness of the results with respect to the definition of the employer event. On the one hand, I exclude from the estimation sample mass layoffs to mitigate selection issues related to the underlying process determining leavers and stayers in mass layoff events, since the probability of a worker being or not displaced due to a mass layoff might be endogenous to individual labor market outcomes. In the case of employer closures, all workers will be eventually displaced and, hence, selection in the process should be less of a concern. On the other hand, I re-estimate the model using alternative definitions of mass layoffs based on employment contractions typically used in the job displacement literature: 30 or 50 percent of employer's workforce within a year. The results are robust to these tests, but suggest that the bigger the magnitude of the treatment effect, the more salient the firm-specific shocks.

Finally, Table A.13 shows that the results are robust to include only (i) single-establishment organizations to mitigate the impact of internal labor markets, (ii) limited liability companies where owner-worker relationships are less personal, (iii) workers' first observed spell to avoid the influence of repeated job spells in the estimation, or (iv) job spells started before June 2010 to avoid the inclusion of special contracts with lower firing costs introduced by the 2010 labor market reform. All these checks indicate that the results are robust to additional restrictions to the data.

6 Conclusion

This paper studies the job mobility 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 other words, employment protection in the form of a lump-sum transfer from the firm to the worker upon dismissal distorts workers' mobility decisions, as it increases the opportunity cost of job quit. The results indicate that the severance pay reduction induced by the labor market made workers who expected to be displaced in the near future more likely to voluntarily leave their employers. In fact, the job quit hazard rate for workers exposed to this layoff shock increased by 17.5 percent after the policy change compared to workers not affected by such shock. The evidence is consistent with a frictional job search model and endogenous separations that are either employee-initiated (quits) or employer-initiated (layoffs). In this class of models, severance pay distorts workers' voluntary mobility decisions by directly affecting the expected utility of being laid off: the value of unemployment. Moreover, lower firing costs may translate into higher (and plausibly better) job offer arrival rates, further easing workers' voluntary mobility.

The current findings have implications for labor market policy. Firstly, they demonstrate that policies targeting firms behavior may also impact workers' behavior, which should be taken into account in the design of policies that alter the economic incentives embedded in the employment relationship. 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. The results thus suggest that employment protection and unemployment insurance should be jointly designed (Blanchard and Tirole, 2008).

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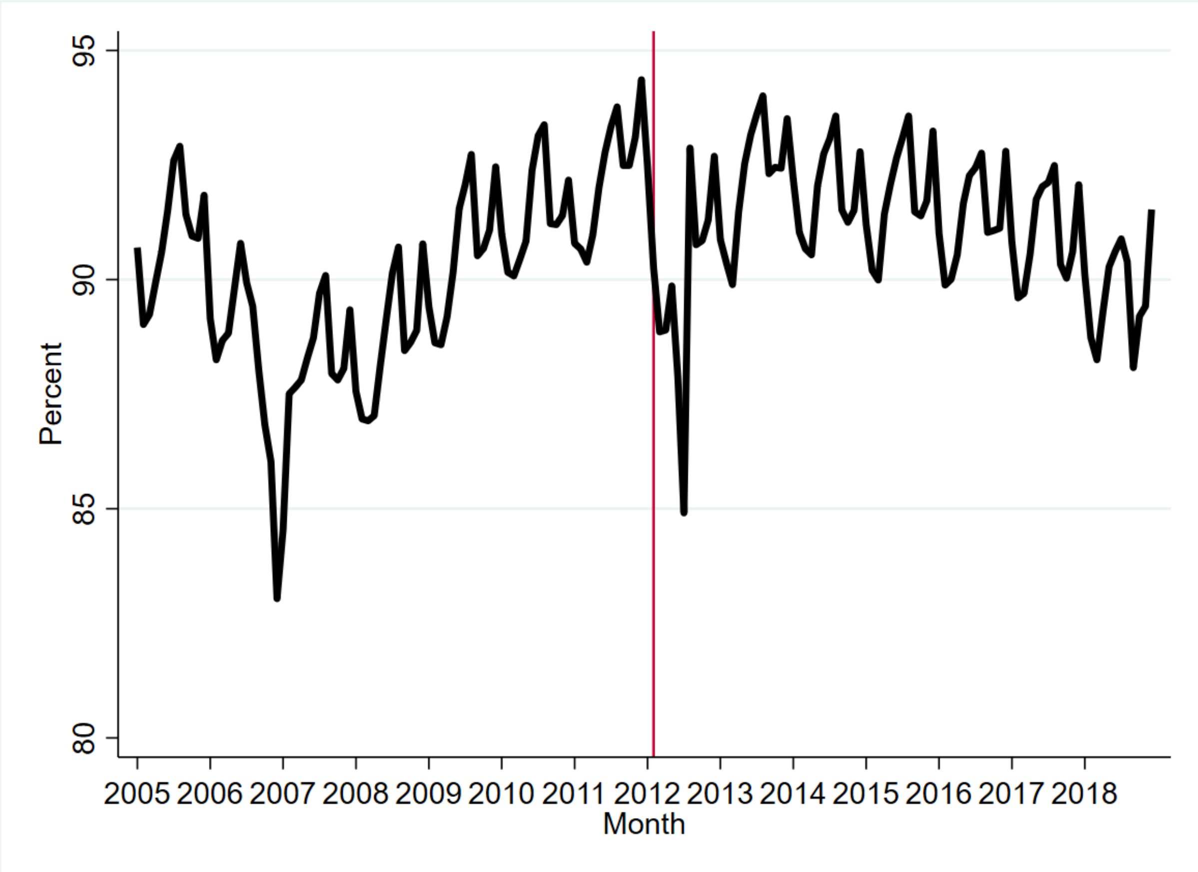
A Tables and figures

Table A.1: Spanish labor market, 2005-2018

Year	GDP Growth	Unemp. Rate	Emp. Growth	Quit rate	FTC rate
2005	3.72	9.15	4.65	38.01	33.4
2006	4.17	8.45	4.98	41.52	33.9
2007	3.77	8.23	3.84	41.74	31.5
2008	1.13	11.25	-1.23	29.78	29.1
2009	-3.57	17.86	-6.80	18.37	25.2
2010	0.02	19.86	-1.97	18.53	24.7
2011	-1.00	21.39	-1.66	16.57	25.1
2012	-2.93	24.79	-4.51	15.00	23.4
2013	-1.70	26.10	-4.03	15.91	23.2
2014	1.38	24.44	1.59	22.10	24.0
2015	3.65	22.06	3.80	25.93	25.2
2016	3.17	19.64	3.64	28.04	26.1
2017	2.98	17.22	4.46	31.29	26.7
2018	2.35	15.26	4.05	32.90	26.8

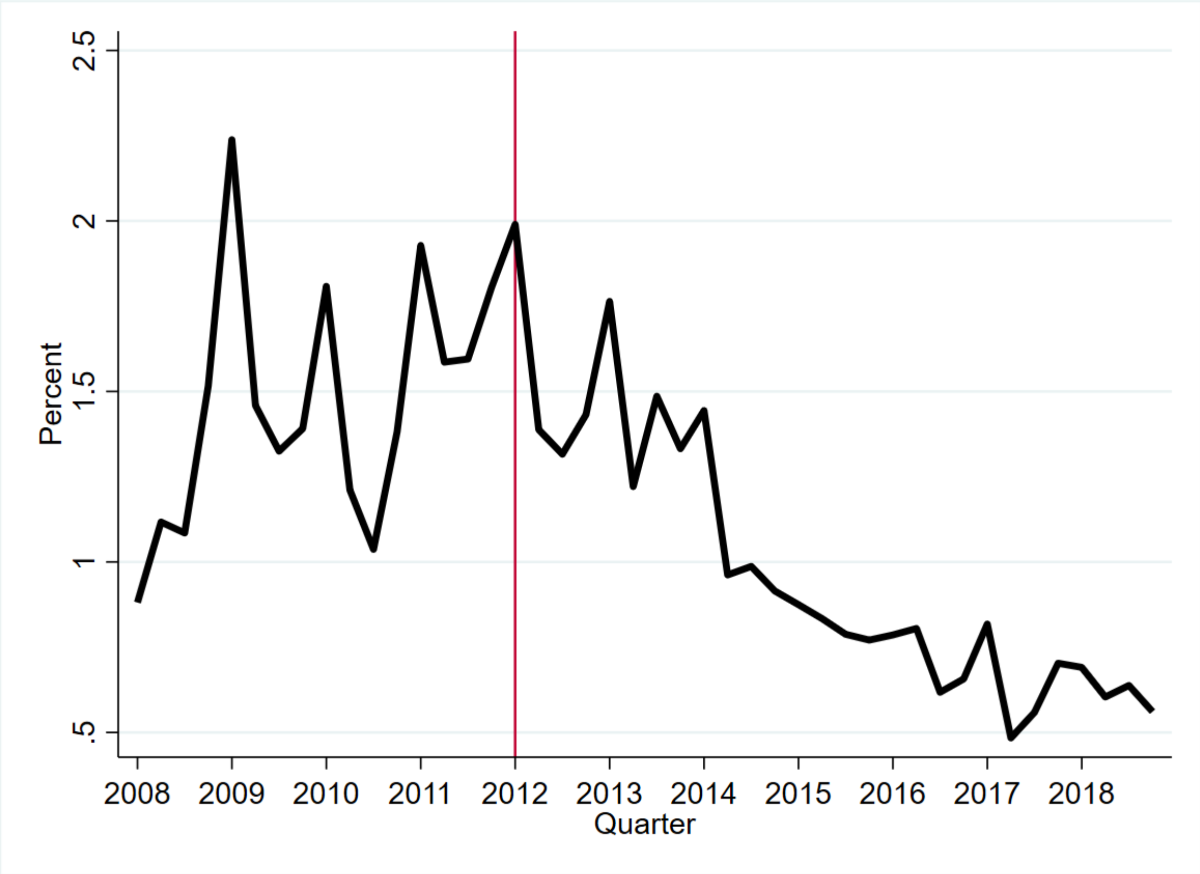
Sources: GDP growth, unemployment rate, and fixed-term contract (FTC) rate (*Instituto Nacional de Estadistica*). Employment growth and quit rate (*Ministerio de Inclusion, Seguridad Social y Migraciones*).

Figure A.1: Newly-signed fixed-term contracts, 2005-2018



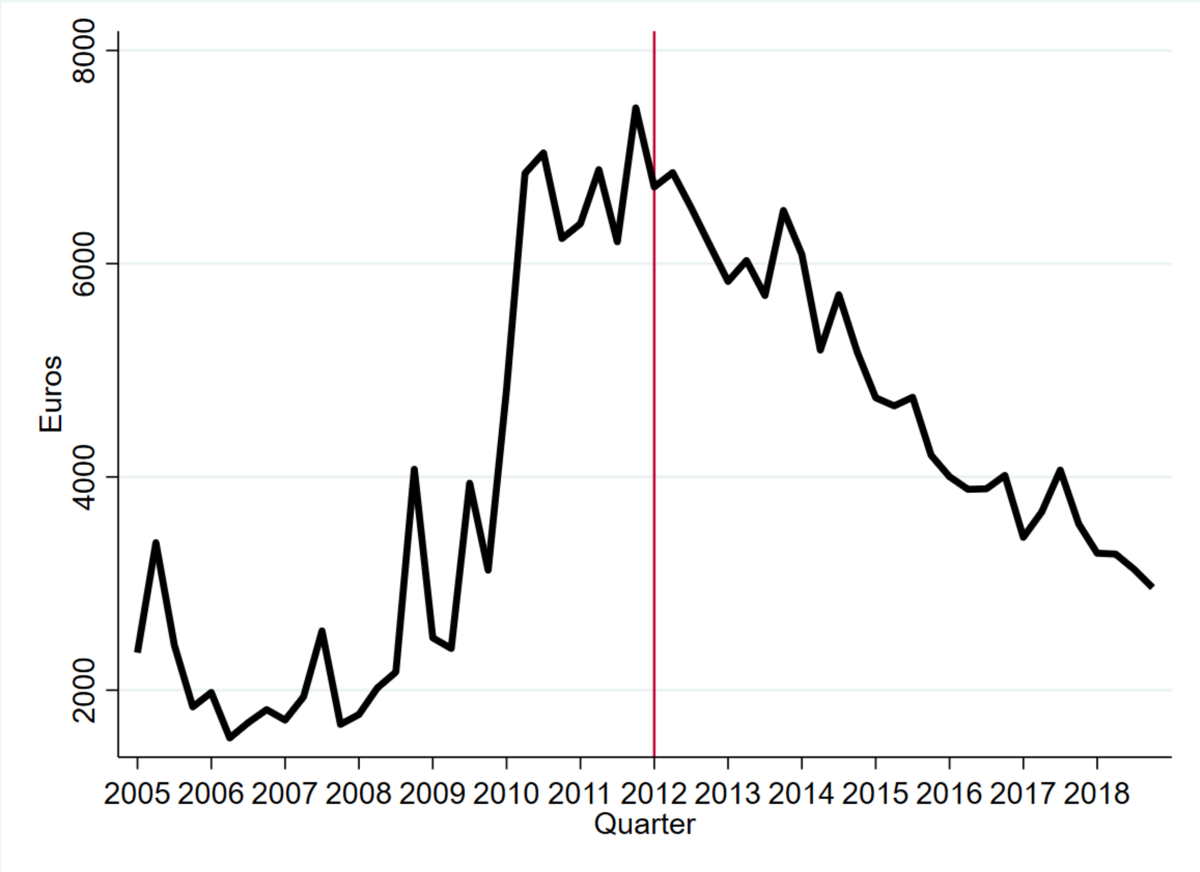
Notes: Figure displays the percentage of newly signed fixed-term contracts over all newly signed contracts each month. The vertical line identifies the reform moment (February 2012). Source: *Ministerio de Inclusion, Seguridad Social y Migraciones*.

Figure A.2: Firing costs over total labor costs, 2008-2018



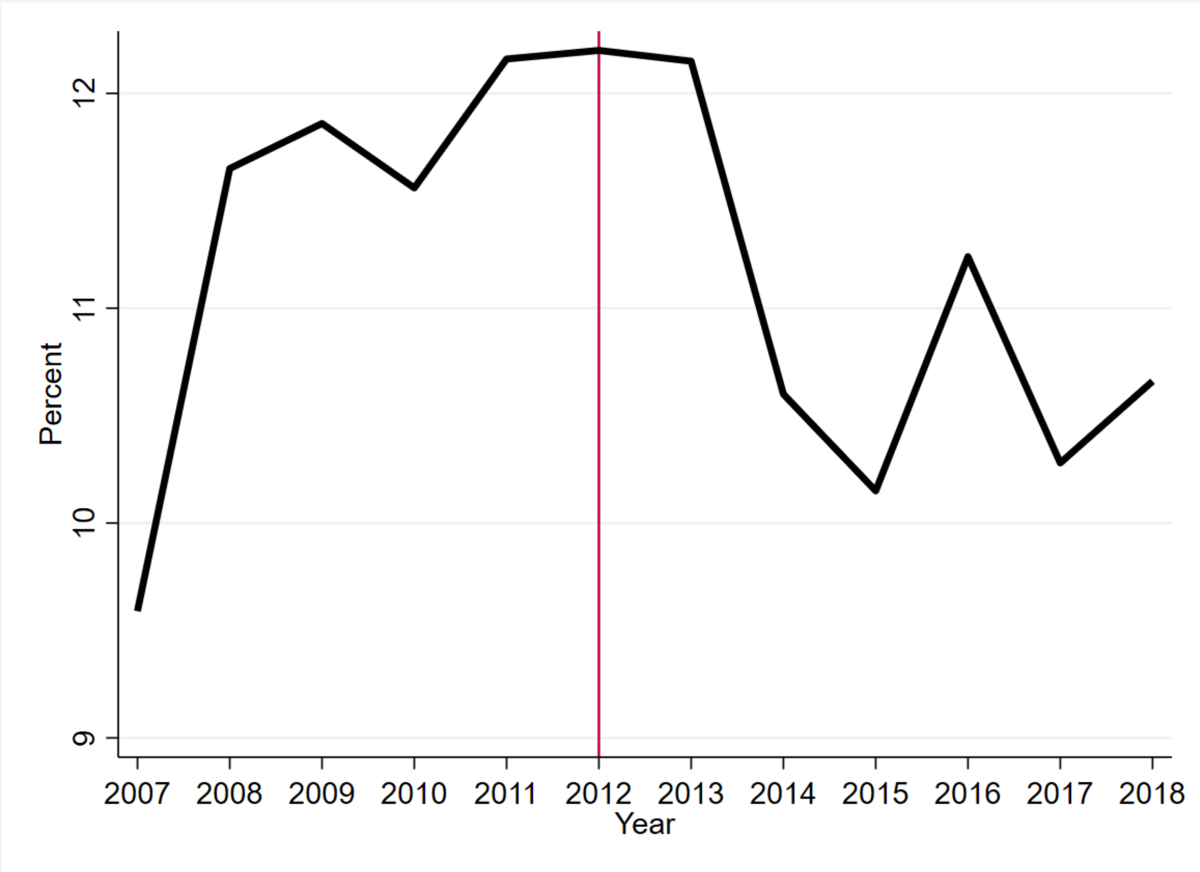
Notes: Figure displays the ratio of effective firing costs over total labor costs. The vertical line identifies the reform moment (February 2012). Source: *Instituto Nacional de Estadistica*.

Figure A.3: Severance pay recognized by labor courts, 2005-2018



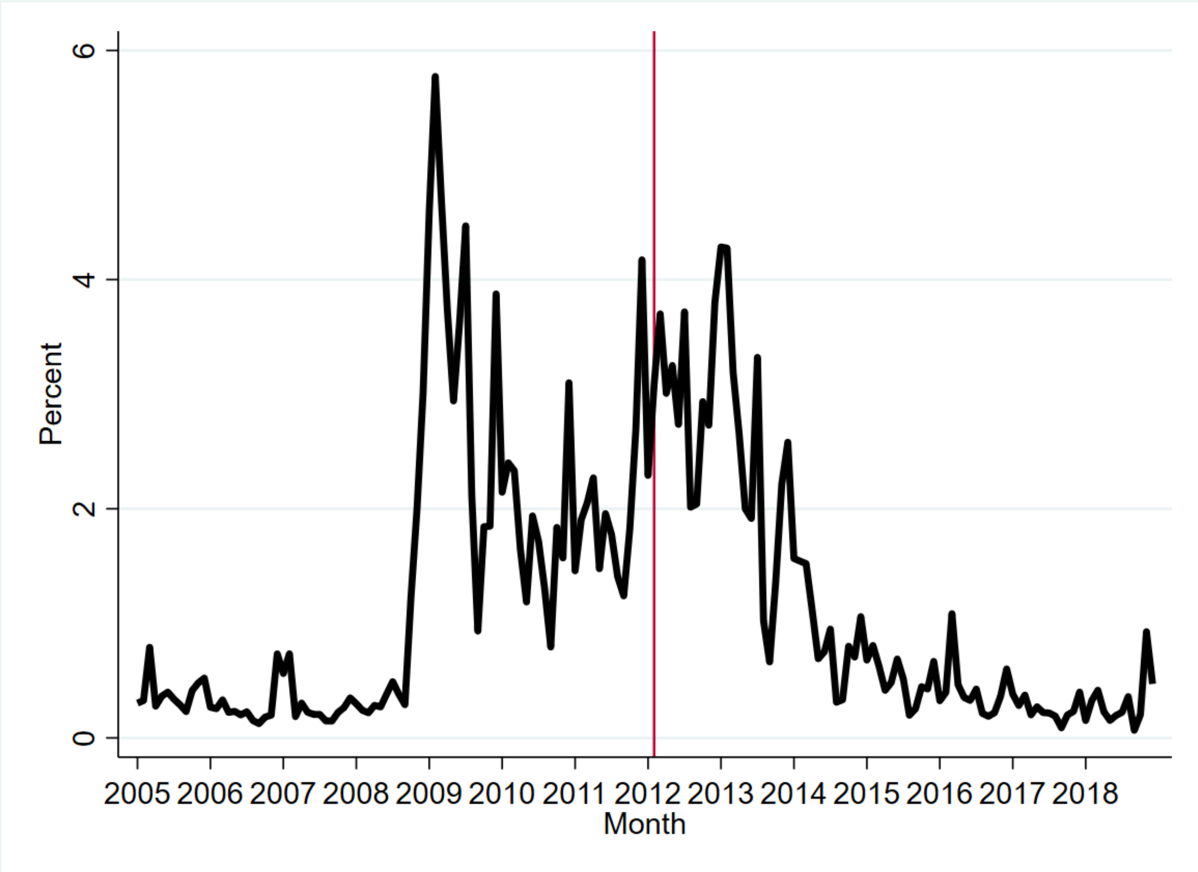
Notes: Figure displays severance pay amounts received by workers who appealed their dismissal in court. The vertical line identifies the reform moment (February 2012). Source: *Ministerio de Inclusion, Seguridad Social y Migraciones*.

Figure A.4: Firm exit rate, 2007-2018



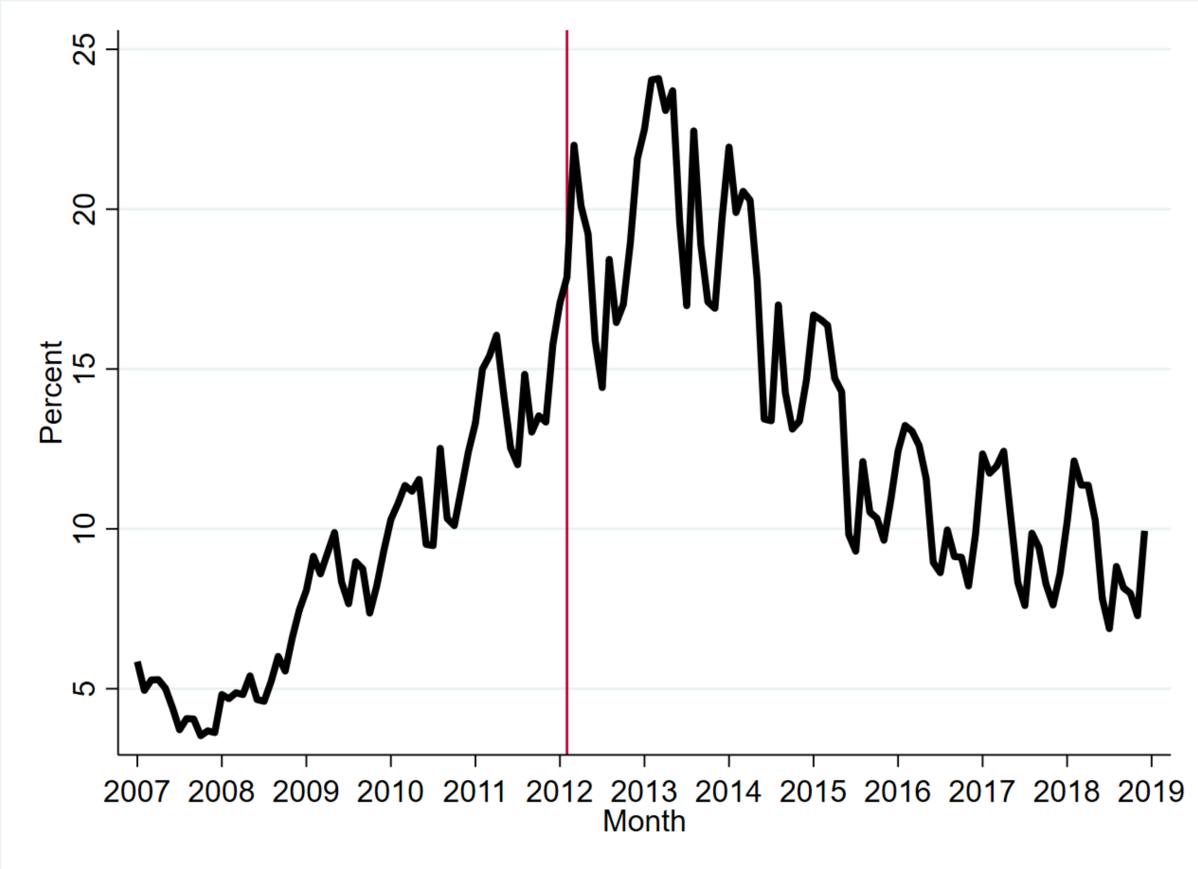
Notes: Figure displays the percentage of firms going out of business relative to total number of firms each year. The vertical line identifies the reform moment (February 2012). Source: *Directorio Central de Empresas (DIRCE)*.

Figure A.5: Collective redundancies, 2007-2018



Notes: Figure displays the percentage of workers affected by collective redundancies over all workers laid off each month. The vertical line identifies the reform moment (February 2012). Source: *Ministerio de Inclusion, Seguridad Social y Migraciones*.

Figure A.6: New unemployment benefits' recipients due to economic reasons, 2007-2018



Notes: Figure displays the percentage of newly registered unemployment benefits' recipients that were fired due to economic reasons (*fair dismissals*) by their employer. The vertical line identifies the reform moment (February 2012). Source: *Ministerio de Inclusion, Seguridad Social y Migraciones*.

Table A.2: Descriptive statistics: Sample constraints

	Initial Sample	2005-2018	Plant-size ≥ 5	Workers' Age	Qualify SP	Final sample
Outcome variables[a]						
Completed	0.946	0.918	0.912	0.921	0.647	0.826
Quit	0.178	0.167	0.164	0.171	0.354	0.333
Layoff	0.816	0.828	0.830	0.825	0.612	0.636
Other reasons	0.006	0.005	0.006	0.004	0.033	0.031
Duration (months)	13.12	9.76	10.41	9.564	41.66	53.01
Right-censored spells	62.92	39.90	41.70	39.06	58.37	125.2
Completed spells	10.26	7.08	7.41	7.02	32.54	37.85
Policy variables[a]						
After reform	0.334	0.556	0.554	0.535	0.691	0.425
Layoff shock	0.103	0.164	0.208	0.205	0.254	0.225
Job characteristics[a]						
Permanent contract	0.152	0.190	0.189	0.181	1	1
Full-time job	0.737	0.660	0.678	0.673	0.777	0.814
High-skill	0.077	0.063	0.068	0.068	0.171	0.157
Real daily wage	36.41	34.25	35.82	35.26	50.03	50.06
Worker characteristics[b]						
Female	0.468	0.468	0.463	0.467	0.451	0.454
Age	25.97	32.06	32.12	30.01	31.59	30.91
Spanish	0.791	0.749	0.752	0.742	0.777	0.776
College	0.158	0.145	0.150	0.155	0.236	0.217
Employer characteristics[c]						
Services	0.665	0.711	0.683	0.685	0.747	0.731
Biggest 4 cities	0.347	0.349	0.371	0.371	0.436	0.438
Plant Age	4.27	5.89	7.51	7.55	8.99	9.22
Size	7.69	9.31	16.84	17.33	27.06	32.94
No. spells	7,716,223	3,978,702	3,142,118	2,848,269	399,041	177,221
No. workers	1,052,528	793,183	720,348	651,574	286,276	152,526
No. plants	1,839,941	1,086,775	565,959	540,947	210,136	112,914

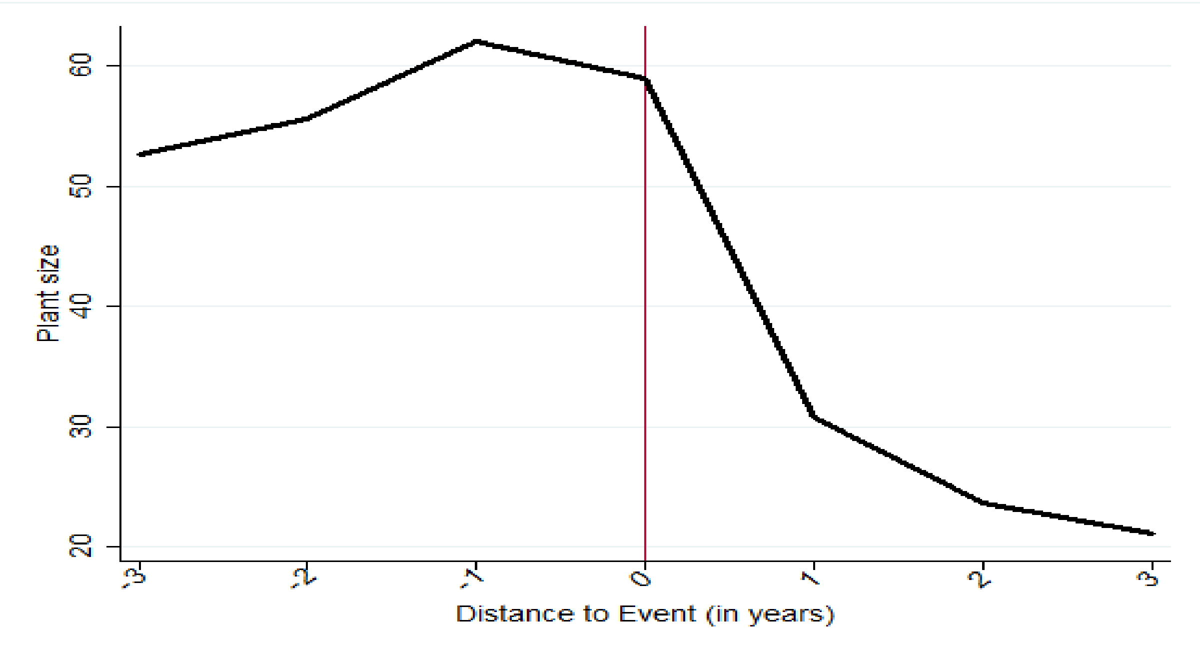
Notes: [a]Variables averaged over job spells at the moment of job start. [b]Variables averaged across workers' first job spells observed. [c]Variables averaged across plant's first observation. After reform identifies job spells with at least one month observed after the reform date (February 2012). Layoff shock applies to workers still employed 12 months before the start of the year of the large employment contraction. Real daily wages refer to the starting daily wage and are deflated using the 2018 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. Estimation sample includes only job starters for whom the incidence of a given event occurs once they already qualify to collect severance pay.

Table A.3: Descriptive statistics: Estimation sample

	All	No layoff shock	Layoff shock	No reform	Reform
Worker-level variables					
Female	0.450	0.459	0.414	0.448	0.449
Age	30.95	30.84	31.59	31.05	31.07
Spanish	0.772	0.763	0.808	0.734	0.826
College	0.215	0.213	0.242	0.201	0.253
Employment history	0.579	0.573	0.628	0.563	0.628
Non-employment	0.462	0.479	0.375	0.475	0.423
Job-level variables					
Full-time job	0.814	0.800	0.862	0.809	0.821
High-skill	0.157	0.151	0.177	0.139	0.180
Real daily wage	50.06	48.49	55.43	47.79	53.11
Employer-level variables					
Services	0.731	0.751	0.668	0.725	0.755
Biggest 4 cities	0.441	0.440	0.472	0.462	0.431
Plant Age	9.16	9.11	9.68	8.81	10.32
Size	31.20	27.38	59.74	34.13	41.66
No. spells	177,221	137,324	39,897	101,821	75,400
No. workers	152,526	120,907	38,501	90,907	75,400
No. plants	112,914	93,119	25,628	74,374	51,681

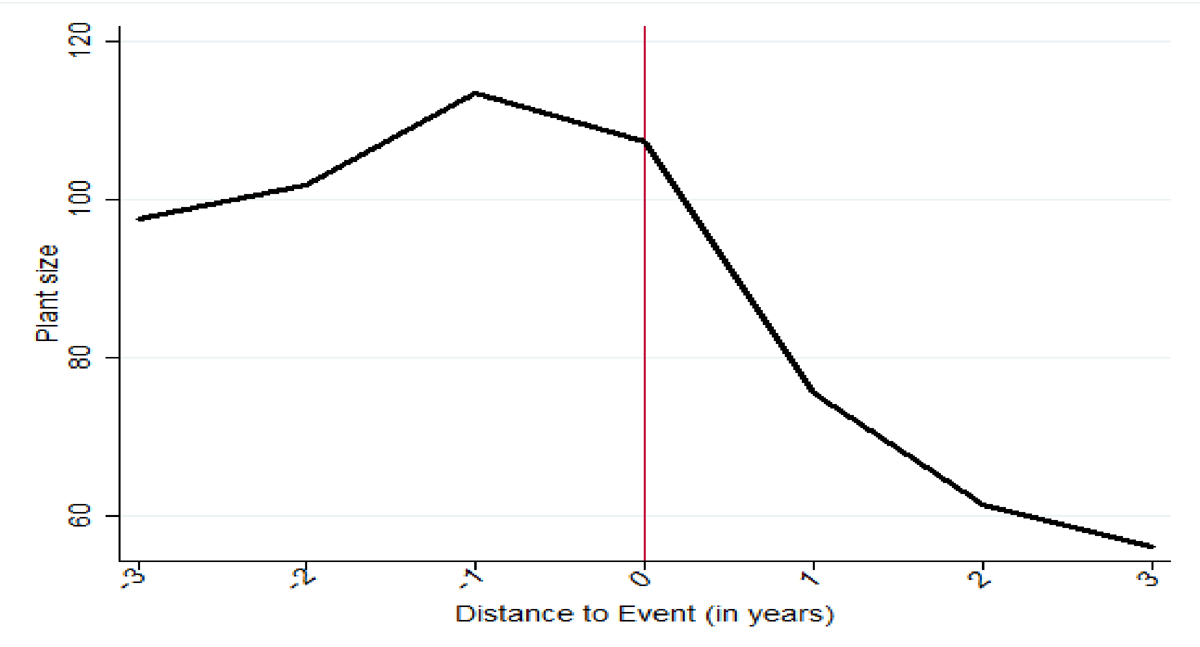
Notes: All characteristics are measured at the moment of job start. Worker characteristics are summarized across worker observations. Job characteristics are averaged across job spells. Employer characteristics are computed at the plant-level. Layoff shock applies to workers still employed 12 months before the start of the year of the large employment contraction. 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 2018 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Figure A.7: Evolution of plant-size around event start moment



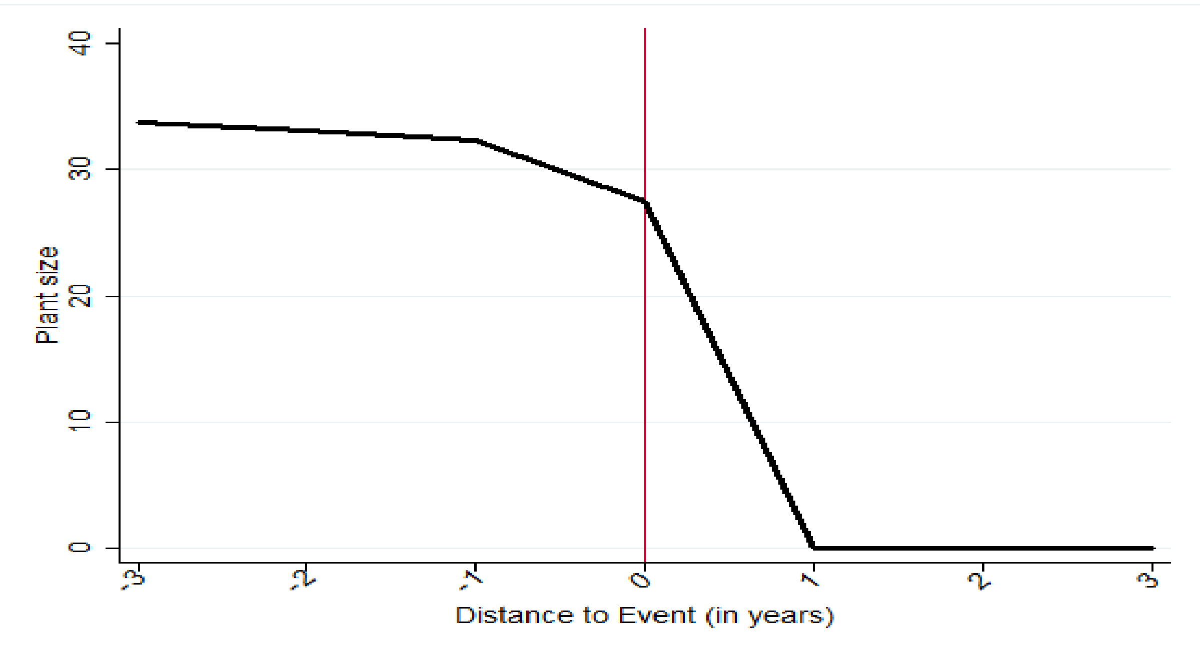
Notes: The figure depicts the evolution of plant size around the start of the large employment contraction (vertical line).

Figure A.8: Evolution of plant-size around event start moment (mass layoff plants)



Notes: The figure depicts the evolution of plant size around the start of the large employment contraction (vertical line) for mass layoff plants.

Figure A.9: Evolution of plant-size around event start moment (closing plants)



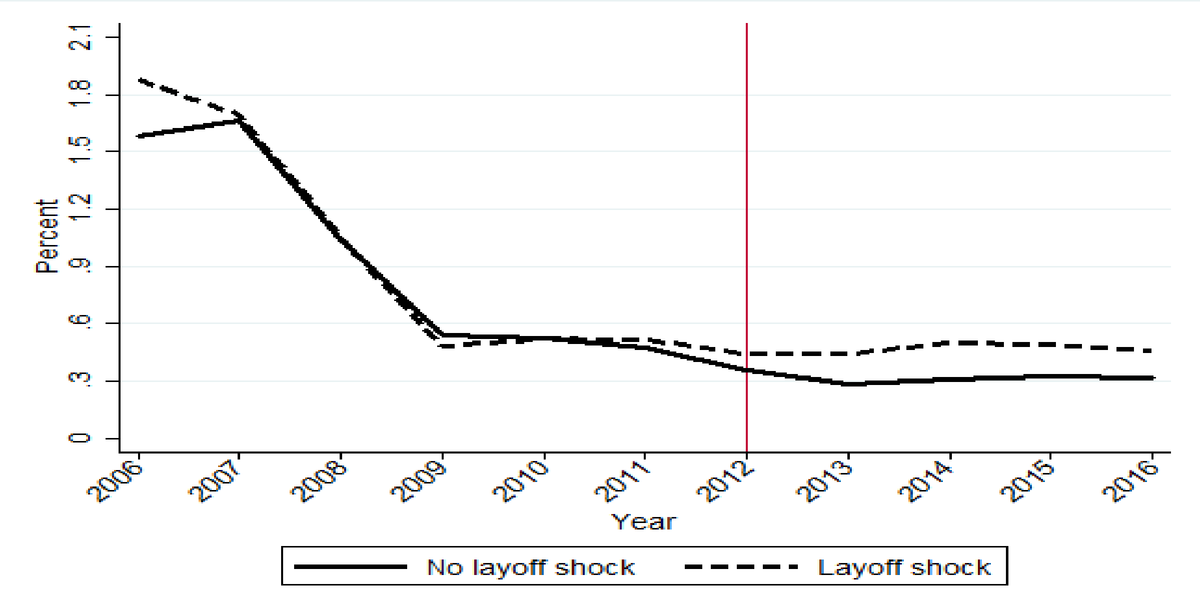
Notes: The figure depicts the evolution of plant size around the start of the large employment contraction (vertical line) for closing plants.

Table A.4: Distribution of job quit duration (in months)

	Mean	Q10	Q25	Q50	Q75	Q90
S if layoff shock	23.6	7	10	17	29	50
R if reform	32.7	9	15	27	48	65
Y	29.8	9	13	20	36	67
No layoff shock						
Y if no reform	19.3	8	11	16	24	35
Y if reform	63.8	22	36	60	87	111
$Y - R$	32.3	4	11	28	50	69
Layoff shock						
Y if no reform	28.7	13	18	26	36	49
Y if reform	67.0	28	41	64	89	111
$Y - R$	29.4	4	10	25	45	63

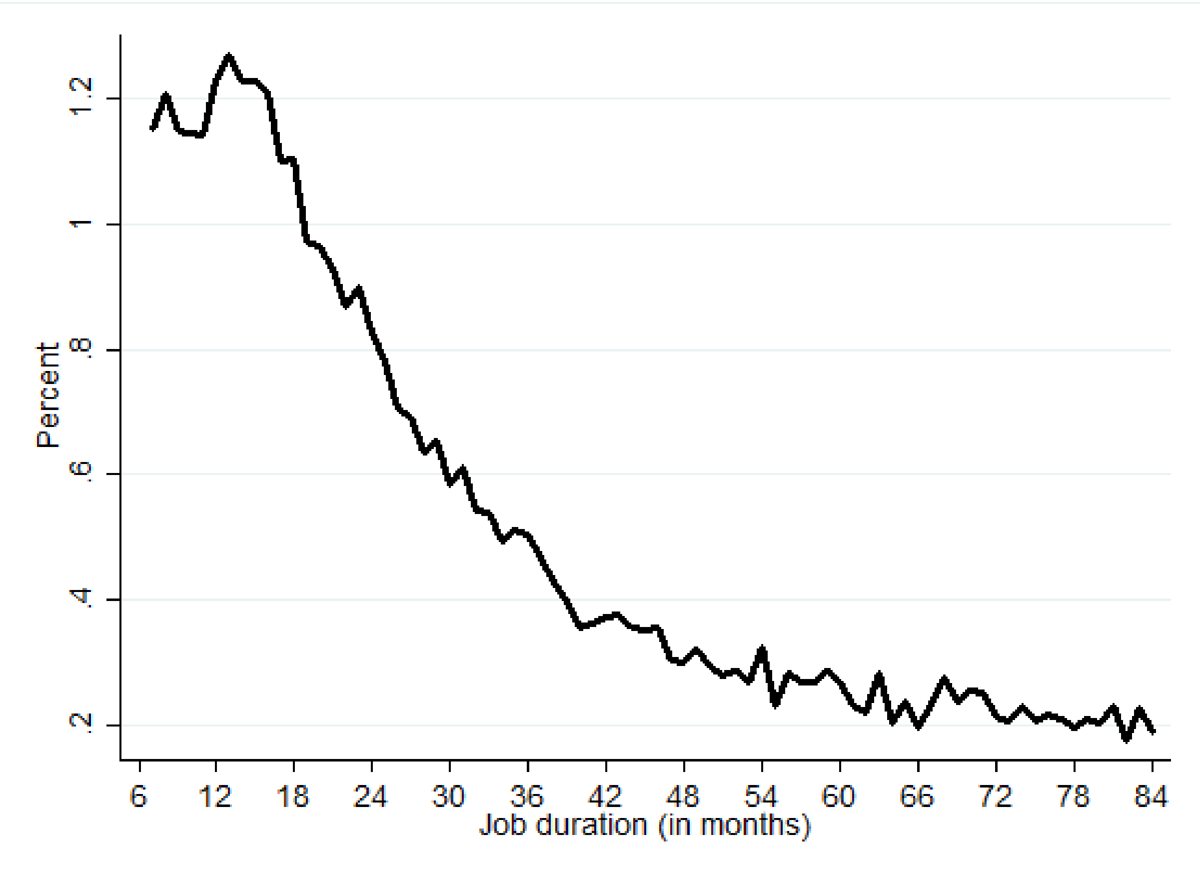
Notes: Layoff shock applies to workers still employed 12 months before the start of the firm event year. The reform moment is February 2012. S denotes the observed duration up to the moment the layoff shock time window starts. R refers to the realized duration up to the reform. Y stands for the observed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

Figure A.10: Job quit flows by layoff shock state



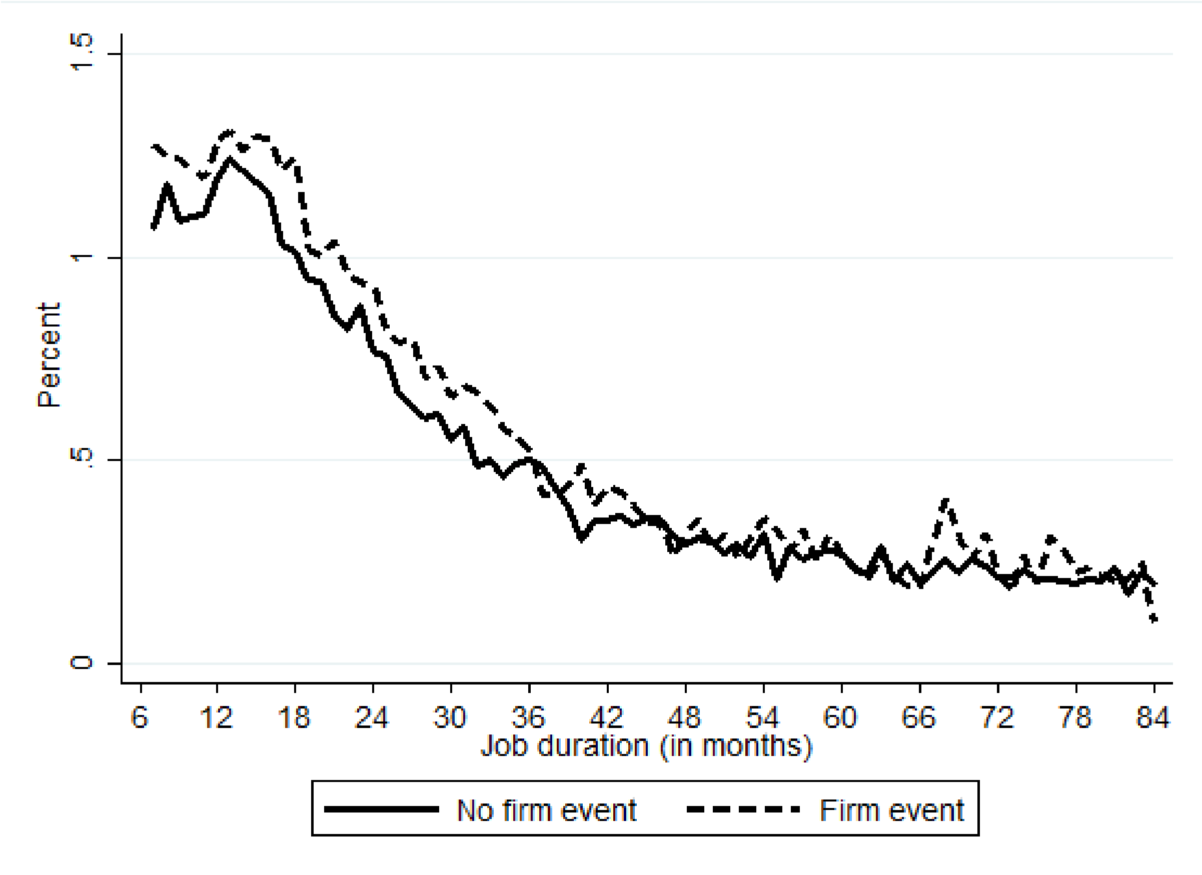
Notes: Percentage of workers who voluntarily leave their employer each year. Layoff shock applies to workers who voluntarily leave their employer the year before the large employment contraction starts. The vertical line identifies the reform date (February 2012).

Figure A.11: Empirical quit hazard rate



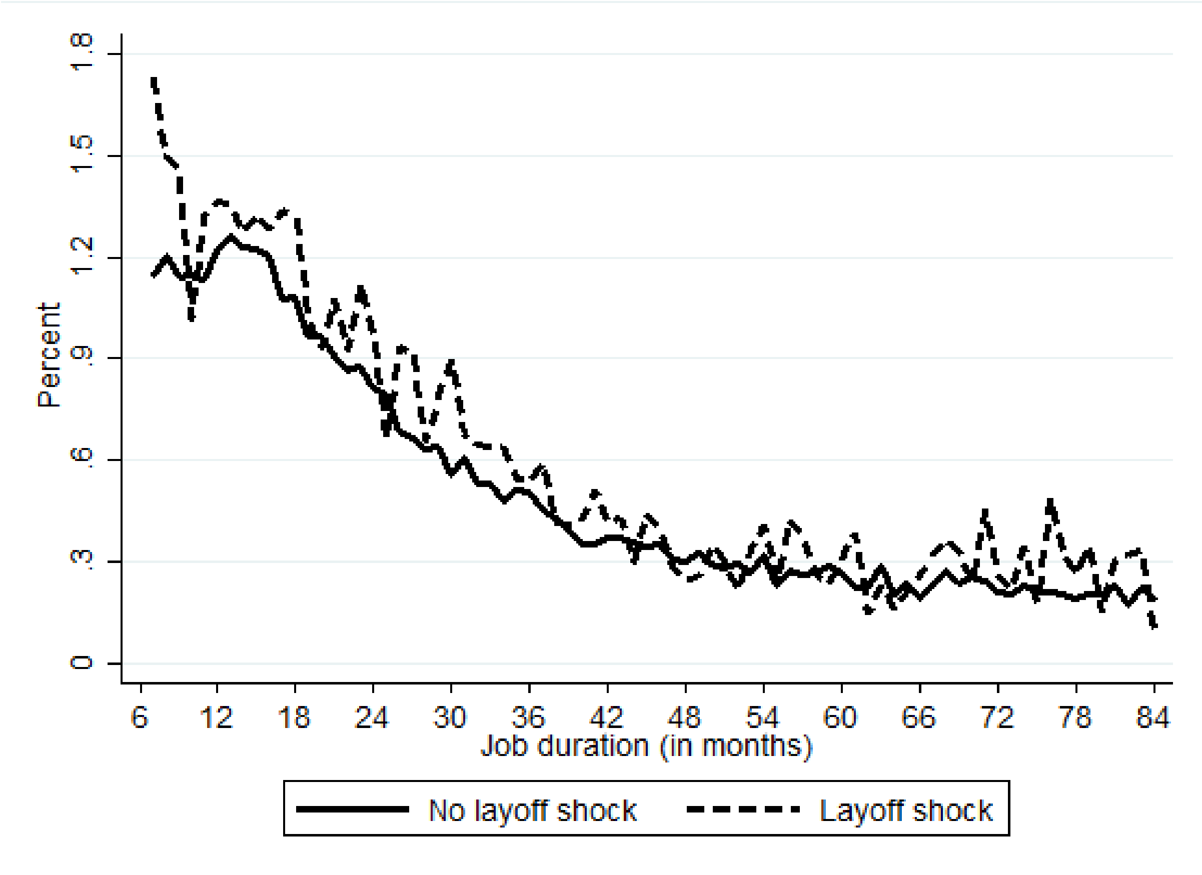
Notes: The figure depicts the job quit hazard rate, which exhibits a negative duration dependence pattern, i.e. the quit rate decreases with time employed. The empirical job quit hazard rate represents the share of workers quitting at a given realized duration over the total number of workers still employed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

Figure A.12: 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 A.13: Empirical quit hazard rate by layoff shock state



Notes: The figure depicts the job quit hazard rate for both workers affected by the layoff shock and workers not affected by the layoff shock. 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.

Table A.5: Job quit hazard rate

	(1)	(2)	(3)	(4)
Treatment effect	0.1619***	0.1693**		
	(0.0538)	(0.0660)		
Treatment effect (short-run)			0.1285**	
			(0.0605)	
Treatment effect (medium-run)			0.2720***	
			(0.1010)	
Treatment effect (low-incidence)				0.1496*
				(0.0807)
Treatment effect (high-incidence)				0.1698**
				(0.0662)
Observations	6,300,972	6,300,972	6,300,972	6,300,972
No. spells	177,221	177,221	177,221	177,221
No. workers	152,526	152,526	152,526	152,526
Baseline hazard	Yes	Yes	Yes	Yes
Observed characteristics	Yes	Yes	Yes	Yes
Unobs. heterogeneity	Yes	Yes	Yes	Yes

Notes: Columns(1), (3), and (4) consider as job exit all voluntary (employee-initiated) separations. Column (2) uses as job exit only voluntary separations that ended up in a job-to-job transition within a month after separation. Column (3) divides the treatment effect into "short-run" and "medium-run". Short-run refers to workers who are affected by the layoff shock before 2014. Medium-run includes workers affected by the layoff shock from 2014 onwards. Column (4) divides the treatment effect with respect to the incidence of the reform based on the relative time under the reform compared to total job duration. Low incidence refers to job spells with less than 30 percent of overall job duration under the reform. High-incidence refers to job spells with more than 30 percent of overall job duration under the reform. Baseline hazard stands for the piece-wise constant function with 10 step points to match the deciles of the distribution of job quit duration. Observed 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), indicators for full-time and high-skill occupations, (log) real daily wages, plant size and age, categorical variables for industry (6), dummy variables for year of job start (7), the 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 Column (1) of Gamma var. = 0 (92.02). Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A.6: Descriptive statistics for benchmark and matched samples

	Matched Sample 1		Matched Sample 2	
	No event	Event	No layoff shock	Layoff shock
Worker-level variables				
Female	0.442	0.423	0.427	0.409
Age	30.76	31.28	31.13	31.56
Spanish	0.762	0.762	0.771	0.800
College	0.206	0.206	0.217	0.219
Employment history	0.593	0.606	0.615	0.632
Non-employment	0.437	0.406	0.404	0.371
Job-level variables				
Full-time job	0.833	0.838	0.839	0.864
High-skill	0.176	0.166	0.177	0.173
Real daily wage	52.28	52.89	53.06	55.52
Employer-level variables				
Services	0.751	0.713	0.719	0.684
Biggest 4 cities	0.535	0.555	0.544	0.540
Plant Age	9.88	9.36	10.23	9.81
Size	42.13	50.53	61.98	63.87
No. spells	55,315	55,315	33,810	33,810
No. workers	31,985	51,305	24,302	32,745
No. plants	24,794	32,655	20,112	21,824

Notes: Matched Sample 1 select controls for workers starting a job in plants that experience a large employment contraction based on exact matches in terms of the following characteristics: hired in the same quarter, same gender, college degree, same industry , same province, and same quartile of the plant-size distribution. If there are multiple matched controls, the one with the closest propensity score based on age, employment history, and previous employment state is chosen. Matched Sample 2 repeats the same exercise but selects only valid controls for individuals that are ultimately affected by the layoff shock. All characteristics are measured at the moment of job start. Worker characteristics are summarized across worker observations. Job characteristics are averaged across job spells. Employer characteristics are computed at the plant-level. Non-employment identifies workers coming from non-employment at job start. Wages are deflated using the 2018 Consumer Price Index. Biggest 4 cities are Madrid, Barcelona, Sevilla, and Valencia, the metropolitan areas with over 1 million inhabitants.

Table A.7: Exogeneity of employer event

	Benchmark	(1)	(2)	(3)	(4)
Treatment effect	**0.1619*** **	0.1829***	0.1624**	0.1944***	0.1483**
	(0.0538)	(0.0712)	(0.0657)	(0.0615)	(0.0632)
Observations	6,300,972	3,848,102	2,424,458	6,300,972	6,300,972
No. spells	177,221	110,172	67,806	177,221	177,221
No. workers	152,526	79,648	55,604	152,526	152,526

Notes: Column (1) selects controls for workers starting a job in plants that experience a large employment contraction based on exact matches in terms of the following characteristics: hired in the same quarter, same gender, same education level, same industry, same province, and by a firm in the same quartile of plant-size distribution. If there are multiple matched controls, the one with the closest propensity score based on age, employment history, and previous employment state is chosen. Column (2) repeats the same exercise but selects only valid controls for individuals that are ultimately affected by the layoff shock. Column (3) weights the benchmark model with the estimated propensity score for the probability of starting a job in an event firm using as controls dummies for the quarter of hiring, gender, education, dummies for industry, province, and quartile of the size distribution. Column (4) uses duration-specific inverse probability weights based on the predicted probability of being affected by the layoff shock. All columns include the same set of controls as in Table A.5. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A.8: Layoff shock arrival moment

τ = first month of event year							
	$\tau-6$	$\tau-9$	$\tau-11$	$\tau-12$	$\tau-13$	$\tau-15$	$\tau-18$
Treatment effect	0.2515***	0.1428**	0.1581***	**0.1619*** **	0.1332**	0.1313**	0.1513**
	(0.0742)	(0.0631)	(0.0565)	**(0.538)**	(0.0558)	(0.0586)	(0.0632)
Observations	6,300,972	6,300,972	6,300,972	6,300,972	6,252,826	6,166,506	6,034,598
No. spells	177,221	177,221	177,221	177,221	175,390	172,621	167,972
No. workers	152,526	152,526	152,526	152,526	151,265	149,292	146,096

Notes: Benchmark model corresponds to column $\tau-12$. All columns include the same set of controls as in Table A.5. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A.9: Placebo reforms

	Feb2012	Feb2010	Nov2009	Aug2009	May2009	Feb2009
Treatment effect	**0.1619*** **	0.0294	0.0073	-0.0182	-0.0756	-0.0748
	(0.0538)	(0.0531)	(0.0546)	(0.0555)	(0.0569)	(0.0583)
Observations	6,300,972	4,051,600	3,807,343	3,521,100	3,180,103	2,996,578
No. spells	177,221	137,668	133,191	128,435	123,344	116,787
No. workers	152,526	124,210	120,797	117,241	113,149	107,883

Notes: Benchmark model corresponds to column Feb2012. All columns include the same set of controls as in Table A.5. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A.10: Hazard rate specification

	Benchmark	Logit	CRisk shared	CRisk correlated
Treatment effect	**0.1619*** **	0.1626***	0.1671***	0.1590***
	(0.0538)	(0.0541)	(0.0541)	(0.0548)
Observations	6,300,972	6,300,972	6,300,972	6,300,972
No. spells	177,221	177,221	177,221	177,221
No. workers	152,526	152,526	152,526	152,526

Notes: Benchmark column refers to the complementary log-log link specification of the hazard rate. Logit column assumes instead a logit functional form specification for the job quit hazard rate. CRisk shared column considers quits and layoff as alternative exits from employment where competing exits shared the same unobserved component. CRisk correlated column refers to a competing risk model assuming different but correlated unobserved determinants for the competing exits. All columns include the same set of controls as in Table A.5. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A.11: Unobserved heterogeneity

	Benchmark	No unobs.	Worker level	Plant level	Firm level	HS 2 mass points
Treatment effect	**0.1619*** **	0.1577***	0.1675***	0.1423**	0.1533***	0.1624***
	(0.0538)	(0.0533)	(0.0544)	(0.0552)	(0.0551)	(0.0538)
Observations	6,300,972	6,300,972	6,300,972	6,300,972	6,300,972	6,300,972
No. spells	177,221	177,221	177,221	177,221	177,221	177,221
No. workers	152,526	152,526	152,526	152,526	152,526	152,526

Notes: Benchmark column stands for a job-level random effects model specified to follow a Gamma distribution. No unobs. column refers to the benchmark model without accounting for unobserved heterogeneity. Worker, plant, and firm-level columns use random effects following a Gamma distribution that are shared at the worker, plant, and firm-level, respectively. HS 2 mass points column relies on a discrete mixture distribution to summarize unobserved individual heterogeneity as in Heckman and Singer (1984). All columns include the same set of controls as in Table A.5. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A.12: Employer events

	Benchmark	No mass layoffs	Mass layoff 30%	Mass layoff 50%
Treatment effect (δ)	**0.1619*** **	0.2342***	0.1724***	0.2100***
	(0.0538)	(0.0712)	(0.0538)	(0.0557)
Observations	6,300,972	5,339,775	7,035,824	7,477,997
No. spells	177,221	142,561	189,260	199,169
No. workers	152,526	125,444	161,432	168,391

Notes: No mass layoffs column excludes from the analysis establishments that carry out a mass layoff from the estimation sample. Mass layoff 30% and 50% columns modify the mass layoff definition to select plants whose employment contracts by more than 30 or 50 percent within a year, respectively. All columns include the same set of controls as in Table A.5. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A.13: Further sensitivity tests

	Benchmark	Single plant	LLC	First spell	No Ref2010
Treatment effect (δ)	**0.1619*** **	0.1572***	0.1728***	0.1781***	0.1718**
	(0.0538)	(0.0548)	(0.0578)	(0.0621)	(0.0761)
Observations	6,300,972	6,069,634	5,496,856	5,422,244	5,312,973
No. spells	177,221	171,216	153,990	152,526	153,162
No. workers	152,526	148,040	134,128	152,526	135,685

Notes: Single plant column excludes multi-establishment firms. LLC column refers to limited liability companies and excludes from the analysis sole proprietor employers. First spell column only considers the first job spell observed for each worker between January 2005 and July 2011. No Ref2010 column excludes from the analysis job spells created after the LM reform in June 2010. All columns include the same set of controls as in Table A.5. Standard errors in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

B 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.[46]

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 me to distinguish between individuals with Spanish nationality (N00 code) and other 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 for workers born after 1950.

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

[46] 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

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 more than 30 days. For employment state, I distinguish between temporary and permanent contracts.

Contract type. The MCVL contains a long list of contracts (+100 types) that are summarized in two broad categories, in accordance with the contract's 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. 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 considered 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.[47] 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. 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 the 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.[48]

Industry. 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

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

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

activity before. I use the CNAE09 classification when available, and CNAE93 otherwise exploiting the correspondence table provided by the Spanish National Statistical Office.[49] Then, I aggregate the three-digit industry information into 6 categories: manufacturing (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), and professional services, which includes information and communication technologies (581 to 639); finance, insurance, and real estate (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).

Plant size. Corresponds to the number of 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/

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