

# Staggered Contracts and Unemployment during the Great Recession: Evidence from Spain\*

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## Abstract

We study the impact of widespread downward wage rigidity on employment flows on the onset of the Great Recession. Downward wage rigidity is due to the fact that sector-level collective agreements in Spain are automatically extended to all firms, setting wage minima for workers in the same province-industry-skill cell. We identify the impact of wage rigidity on employment because, unlike settled ones, newly bargained contracts are able to adjust to aggregate shocks. Using Social Security data and various econometric methods we find that agreements bargained after the fall of Lehman Brothers settled an average wage growth of 1.8%, while agreements signed before September 15th, 2008 settled mean wage increases of 3.1%. Among workers subject to wage rigidity, the probability of job loss between 2009 and 2010 and that being unemployed by the end of 2012 was 1% higher than among the rest. Those findings are consistent with the hypothesis that the staggering of contracts and the inability to renegotiate contracts amplify aggregate shocks.

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# 1 Introduction

The relevance of wage rigidity in generating employment fluctuations has been subject to considerable debate both in academic and policy circles during the last recession. Macroeconomic studies stress that binding wage contracts signed in different moments of the business cycle are a source of wage inflexibility that explains aggregate fluctuations in output and employment -see Gertler and Trigari (2008) or Olivei and Tenreyro (2007, 2010). However, the evidence regarding the degree of wage cyclicality and its impact on the allocation of labor is mixed -see Pischke, 2017. In the policy arena, recent labor laws have been passed in Portugal, Greece or Spain with the explicit aim of facilitating wage adjustments. The rationale for such legal reforms was that the poor labor market performance in those economies was due to a high degree of wage rigidity –see OECD (2013).

This paper investigates empirically how widespread downward wage (growth) rigidity affected employment levels on the onset of the Great Recession. The source of wage rigidity stems from collective agreements bargained at the province-sector level that automatically apply to all firms in the bargaining unit –i.e., that are automatically extended. Those contracts specify minimum wage levels for ten skill groups for Spanish workers.

Our empirical strategy builds on the following insight from the macroeconomic literature: wage contracts are not renegotiated continuously, so the ability to adjust wage levels to aggregate shocks is confined to those employers and unions that bargain over new contracts. We use the fall of Lehman Brothers in September 15th, 2008 as an unanticipated demand shock that resulted in a large fall in employment. At a time of heavy employment losses, wages in already settled contracts were unable to adjust downward, possibly leading firms to lay-off workers. On the contrary, contracts bargained at that time had the possibility of mitigating nominal wage growth, possibly softening unit labor costs and employment drops. In sum, the automatic extension of sector-level contracts, together with the difficulty of opting out from existing ones, generates substantial cross-sectional variation in the degree of wage rigidity. That cross-sectional variation in wage growth provides a unique opportunity to estimate the role of downward wage (growth) rigidity on employment destruction after a large fall in aggregate demand.

We use a register that contains all collective agreements signed in Spain. The dataset contains information about the wage growth settled in the contract, the duration of the bargaining period and the date of signature. The dataset permits inferring the information set that employers and unions can incorporate in their bargaining strategy. We then match the information on collective contracts with longitudinal information data from a four percent sample of Spanish Social Security records. The resulting matched sample permits estimating the effect on employment flows of downward wage (growth) rigidity.

The results suggest that between 2009 and 2010 contracts signed before the fall of Lehman Brothers settled for wage growth between 1 and 2 percent higher than contracts signed afterwards. Workers covered by collective contracts signed before LB were 1% more likely to lose their job between 2009 and 2010 and, as a group, 1 percent ore likely to remain unemployed in 2012, four years after the aggregate shock. Those effects are driven by contracts whose duration extended wage rigidity at least two years into the recession. An analysis with a subsample of collective contracts with information about minimum wage levels suggests that the impact of date of signature on wage growth and on the transitions into non-employment is heterogeneous and mainly due to workers whose pre-recession earnings were close to the minimum wages set in collective contracts.

Overall, our results are consistent with the hypothesis that the degree of wage rigidity generated by the automatic extension of provincial agreements and contract staggering amplified employment destruction between 2009 and 2012. We conduct several robustness checks to control for other factors that may correlate with the date of signature and employment and wage growth outcomes. Firstly, we examine the stability of the results by varying the time window around the macroeconomic shock. Secondaly, we use contract-level fixed effects that absorb any factor that affects all workers covered by the same collective contract. Those models identify the differential employment and wage outcomes of workers with pre-recession earnings close to the minimum wage in the collective agreement relative to workers whose pre-recession earnings are further away from the minimum. Comparing those differential outcomes across contracts signed before and after the fall of Lehman Brothers we can infer the impact of wage rigidity on employment outcomes. Thirdly, we examine the particular case of construction, an industry where all province level agreements have set the same wage growth since

2002. We find no differential employment losses by date of contract signature in that industry, confirming that contracts with different signature dates but no differential wage growth do not feature differential employment losses. Overall, our findings suggest that contract staggering helps to propagate aggregate shocks by increasing flows into non-employment.

Our study contributes to the literature that assesses the extent of wage rigidity and its consequences. On one hand, a literature has inferred the relevance of wage rigidity by estimating the degree of cyclicalities of wage changes –see Haefke et al (2013) for the US, Elsby et al. (2016) for the UK, Martins et al. (2012) for Portugal or De la Roca (2014) for Spain. Those papers test if the cyclicalities of wages vary between new matches and incumbents, and adopt different strategies to control for the cyclical adjustment of match quality. Other studies infer wage rigidity from the distribution of longitudinal wage changes in administrative or survey data –see Altonji and Devereux (2000) or Dickens et al (2007). Our results complement those studies by establishing the impact of wage rigidities on employment outcomes. Among others, De Vicenti et al (2007) and Barwell and Sweitzer (2007) infer the impact of wage frictions on employment outcomes by comparing industries with a different level of (estimated) rigidity. Our study contributes to that literature in three dimensions. Firstly, the information about the bargaining periods of collective contracts naturally identifies the set of wages that are able to adjust to a large aggregate shock. Secondly, by using province-industry contracts, we can compare the degree of adjustment of wages set in the same three-digit industry across contracts bargained in different provinces. That is, we do not need to rely on cross-industry comparisons exclusively. Finally, we focus on the role of contract staggering, a widely used mechanism in the macroeconomic literature that features slow convergence to the wage levels desired by each firm and dispersion in wage growth. Other sources of wage rigidity –like absolute wage floors– would generate steady state differences in wages that are difficult to justify on theoretical grounds, see Elsby (2009). Regarding the possibility of extrapolating our results to other economies or periods of time, we note that collective bargaining can be regarded as a particularly visible form of contract staggering, an institution that is not confined to European labor markets.

Finally, we contribute to the literature that assesses the role of explicit forms of wage rigidity in shaping employment outcomes. Martins (2014) conducts a longitudinal study

of the evolution of firm-level employment levels in Portugal around the exact month when a collective contract is extended –i.e., when it becomes binding for all firms in the scope of the agreement. Guimaraes et al (2014), also using Portuguese data, compute the firm-specific increase in payroll that happens when a collective contract becomes binding for all firms –which they define as “upward nominal wage rigidity”. They find that one percent increase in payroll due to the extension of collective contracts reduce the number of employers in the firm by half a percentage point. Using Canadian data and a setup similar to ours, Card (1990) estimates the reaction of firm-level employment to changes in the real cost of labor caused by inflation surprises that do not translate into higher wages because contracts are already settled. We complement those studies in different directions. We can quantify how persistent job losses are, a key determinant of the impact of job losses on aggregate demand. This is possible because our data allows us to track workers over time and provide estimates of how likely it is that affected employees eventually find a job (changes in firm’s employment levels include job-to-job changes that leave aggregate employment constant). Secondly, the relatively large number of collective contracts covering large fractions of workers allows us to uncover the role of the length of the bargaining period (by comparing contracts with different durations) and the role of adjustment costs -by examining the labor market histories of workers with different degrees of employment protection.

The study is structured as follows. [Section 2](#) provides some institutional background and a framework for analysis. [Section 3](#) describes the data and the empirical strategy. [Section 4](#) details the empirical strategy. [Section 5](#) describes the results using the full sample of provincial contracts and [Section 6](#) focuses on a sample with information about minimum wage levels. [Section 7](#) concludes.

## 2 Institutional Background

A salient feature of the Spanish system of industrial relations is that sectoral collective contracts bargained by employer federations and unions are extended to all firms in an industry. In other words, upon publication, the terms and conditions in a sectoral contract become binding for all employers within the scope of the agreement regardless of the rate of worker’s unionization. The conditions for such extension were originally laid out in the 1984 Worker’s Act and require a minimum degree of representativeness of

the bargaining parties. On the side of the employers, the Worker's Act requires that the employers in the federation employ at least 10% of workers in the sector. Furthermore, the Worker's Act requires that the unions that sign the agreement have as affiliates 10% of all employee representatives in the sector –see Ministerio de Trabajo (2008, 2012). Thus, despite a relatively low rate of union membership (about 15%), the coverage of collective bargaining in Spain is very high (above 75%, according to OECD, 2012). While the precise terms vary across countries, extensions also occur in Portugal, the Netherlands, Germany and other countries (see Du Caju et al., 2008).

Extensions of sectoral contracts happen at various geographical levels. There are sectoral agreements covering employees the whole country, while the most disaggregated geographical level we are aware of is the municipality. However, the most common level of sectoral bargaining is the province. Card and De la Rica (2006) report that within the set of workers whose working conditions are covered by a collective agreement, 55% are subject to a province-sector one. There are 52 provinces in Spain, the average size being about one million inhabitants.

The content: Collective contracts establish not only minimum annual wages for a particular period, but also maximum working hours, the number of vacation days and the compensation for unusual working conditions, like extra time or night shifts. In principle, sectoral agreements could also regulate new hirings or the promotion of employees. However, it is typically argued that collective contracts mainly regulate wages and hours.

The extension procedure is achieved by publication in the Official State Gazette (Boletín Oficial del Estado), the same outlet where all legal acts are published. Publication guarantees that terms and conditions in a collective contract become binding for all firms in the sector. Furthermore, sectoral contracts establish minimum levels of annual earnings that vary according to each employee's skill level. Namely, whenever a new worker enters a firm, the employer must specify the position's skill requirement to determine the employee's contribution to the Social Insurance system. Collective contracts set minimum wages for each of those skill levels. Appendix Table A.1 provides an example of minimum wages set in the construction industry in Navarre in 2010. That collective contract establishes annual minimum wages for each skill level as well as its distribution in fourteen installments. We note that the monthly wages for the lowest skill group is 977 euros (fourth column, thirteenth row), well above the statutory

minimum wage for that year (633 euros).

*Duration:* Contracts set working conditions for a pre-specified period. Contract duration varies over the business cycle, but among contracts signed in 2008 and 2009, 88% of all contracts had a validity period of two years at least. It is well known that infrequent bargaining may increase the degree of nominal inertia of the economy (Layard et al. 1991). In addition, it is not uncommon that the pre-specified validity period of a collective contract expires without unions and employers having reached the agreement to renew it. The interpretation by the Supreme Court in such cases is that all firms within the scope of the agreement are still subject to the working conditions and minimum wages set in the expired agreement –see Ministerio de Trabajo, 2008.

*Opting out clauses:* The Worker’s Act mentions some conditions that permit a particular firm to opt-out from a collective contract. Namely, in a period of economic hardship, opting out from a sectoral agreement is possible if both parties agree. If parties disagree, a joint committee in charge of supervising the agreement decides on the convenience of the opt-out (Comisión de Seguimiento del Convenio Colectivo). We are not aware of precise information about the number of successful opting out procedures during our sample periods, as estimates started to be published only in 2011. However, the latest two reforms in Spain in 2010 and 2012 attempted to determine verifiable conditions that permit opt-outs, the reason being that the procedure was cumbersome.

On top of sectoral agreements, worker representatives and managers may negotiate wages and other working conditions in firm-level contracts. Around 15% of workers subject to an agreements are covered by firm-level agreements (according to the union’s reports or Card and De la Rica 2006). Previous researchers have documented that firm-level contracting is most common among large firms and they set wages above the minimum in sectoral agreements.

We focus on sector contracts with provincial coverage for three reasons. Firstly, province-sector contracts achieve wide coverage through automatic extension, potentially generating aggregate wage rigidity. Secondly, theoretical models argue that rigidities generated by the intermediate level of bargaining are most likely to have allocative effects. The reason is that nationwide agreements internalize the impact of wage growth, while firm-level bargaining is most responsive to idiosyncratic changes in the conditions of the worker and firm (see Calmfors and Driffill, 1988 or Jimeno and Thomas, 2013). Finally, two labor reforms in 2010 and 2012 have tried to weaken the automatic ex-

tension of sectoral agreements on the presumption that this contracting level prevents aggregate wage adjustments.

## 2.1 Framework

Consider a small firm that takes wages as exogenously fixed. The firm chooses employment levels so that the wage equals its marginal productivity –i.e., the firm has Right to Manage. A large aggregate demand shock such as the fall of Lehman Brothers translates fully into employment losses if the cost of labor remains fixed. However, if the bargained cost of labor fell as a response to the fall in aggregate demand, the reduction of the labor force would be mitigated –see Card (1990).

Following the previous example, collective contracts are not renegotiated on a continuous fashion, so firms under already settled agreements will experience severe employment losses after a large demand shock. In the data, those are the firms subject to contracts signed before the fall of Lehman Brothers. Conversely, the set of firms that can adjust to that aggregate shock provide a counterfactual employment growth under contract renegotiation –a set that we identify with those firms that are covered by collective contracts being bargained at the time of the shock. Under the assumption that all firms working in the same industry or region face (a) a similar demand shock and (b) similar technology, a comparison between the wage and employment changes of firms that are able to renegotiate contracts and those that are not permits recovering an estimate of the elasticity of the demand of labor with respect to its own wage.

Note that a key assumption here is that wages in collective agreements do adjust to aggregate shocks. Cahuc and Zybelberg (2004) review a set of static bargaining models between unions and employers where bargained wages do not react to changes in productivity. Those models would not allow us to identify a labor demand curve. Alternatively, within a search and matching dynamic framework, newly bargained wages depend both on current and expected aggregate conditions. Koenig, et al (2014) discuss that the sensitivity of newly bargained wages to expected aggregate conditions falls with the interest rate but increases with the length of the contract. Among others, Gertler and Trigari (2008) and Olivei and Tenreyro (2007) successfully calibrate macroeconomic models where the wage rigidities associated to contract staggering amplify aggregate shocks.

We assess empirically the relevance of contract staggering by estimating reduced form models where the contractual and the actual wage growth depends on the information set available at the time of the renewal of collective contracts.

$$\Delta w_c = \delta_0 + \delta_1 \Omega_c + a_c + u_c \tag{1}$$

$$\Delta L_c = \gamma_0 + \gamma_1 \Omega_c + a_c + \varepsilon_c \tag{2}$$

[Equation 1](#) describes the determination of collective contract wages as a function of the information set available at the time of the signature ( $\Omega_c$ ), while [Equation 2](#) describes the evolution of the number of employees covered by the contract  $c$ .  $a_c$  is an unobserved demand shifter. Comparing  $\Delta w_c$  and  $\Delta L_c$  across contracts signed with different information sets -and holding  $a_c$  constant- one can identify the slope of the labor demand curve as the ratio between  $\gamma_1$  and  $\delta_1$ .

One can think of three caveats when interpreting [Equation 1](#) and [Equation 2](#) as a reduced form version of a labor demand curve at the sector level. Firstly, collective agreements only set minimum standards, so contractual wage growth mainly affects those employees whose earnings are initially close to the minimum. Secondly, there may be unobserved factors that correlate both with the date of signature and with the demand of labor, like local demand shocks. Thirdly, contracts may be applied in a more lax manner during a recession. We deal with each issue below.

Firstly, we use a subsample of contracts with information of the minimum contractual wage that allows us to identify those workers who are most likely to be affected by collective contract staggering. In particular, we identify employees whose pre-recession earnings are closest to the minimum and who would be most likely to be displaced if the wage in the collective contract increases. Note that displacements would be composed mainly of existing matches with a very low surplus for the employer. However, it is not obvious how to identify the impact of collective contracts on job creation, because it is hard to identify workers most likely to be hired by an employer using pre-recession earnings.

Secondly, we use several strategies to estimate the local demand shocks embedded in the term  $a_c$ . The first strategy is to control for unrestricted province and three-digit industry dummies. In addition, when using employee-level outcomes we can also use contract-specific fixed effects –effectively, a fixed effect that interacts unrestricted

province and three-digit industry dummies. Contract-level fixed effects absorb any trend that affects all workers covered by a collective contract. Still, we can identify the impact of wage rigidity on employment outcomes by comparing workers whose pre-recession earnings were close to the minimum wage to workers covered by the same collective contract but whose earnings are further away from the minimum. If the dispersion of wages induced by collective contract staggering matters, job losses will be most prevalent –in relative terms– among workers close to the minimum and who are covered by contract signed before the fall of Lehman Brothers.

A third concern is that we do not have information on the number of firms that opted out from the agreement during the sample period or about the degree of enforcement of contracts during a recession. We can test however for the prevalence of “informal” opt-outs by examining actual wage growth among job stayers. The idea is that if informal opting out procedures were prevalent in the data, the wage growth of job stayers should depend neither on the contract signature date nor on the distance of pre-recession earnings from the minimum wage.

A separate concern refers to the endogeneity of the date of signature. Danziger and Neuman (2005) show that uncertainty may cause unions and employers to delay the renewal of collective contracts. We assume that unions and employer federations could not postpone reaching agreements in anticipation of Lehman Brothers’ fall - and provide evidence in support of that assumption. However, it is possible that, once the shock occurred, the variation in the date of signature is determined by idiosyncratic expectations about the duration and the severity of the recession. In what follows, we treat as exogenous whether or not an agreement was reached before or after Lehman Brothers fall, but the exact date of the agreement, if it happened after September 15th 2008, is not necessarily exogenous.

### 3 Data

We use two main datasets. The first one is the Census of Collective Agreements signed in Spain between 1990 and 2010 – *Registro de Convenios y Acuerdos Colectivos*. The second is a 4% sample of all employees with an active record with the Social Security in 2010. The sample is the Continuous Sample of Working Histories, CSWH 2012, a 4% random sample of Social Security records - *Muestra Continua de Vidas Laborales*

2012. We describe each source now.

Collective agreements must be registered at the Ministry of Labor using a pre-specified form to obtain legal validity. The form includes the type of agreement (sector or firm-specific), the period of validity and the agreed wage increase for that period. About 64% of contracts signed specify an ex-post adjustment if realized inflation exceeded a threshold specified in the contract. In such case, the staff of the Ministry updates the wage growth after communication with the unions and employer federations. In addition, the form includes an estimate of the number of workers covered by the agreement, as well as the industrial and geographical coverage (nation-, region-, province- or municipality- level). Particularly important for the purpose of the study, the Census of agreements includes information on the bargaining period and the day in which the agreement was signed. Other entries are not compulsory and unions do not always fill those. In particular, about 40% of forms included the skill-specific minimum wage in the agreements signed between 1994 and 2001. We updated that information to 2007 using the information in the agreement about annual wage growth.

The second sample is drawn from the 2012 Continuous Sample of Working Histories (CSWH), or *Muestra Continua de Vidas Laborales* a register that collects monthly information on the employment status, the earnings of each worker who had an active record with the Social Security System at some time in 2012. The information is recorded electronically and includes each worker's retrospective labor market history –potentially, dating back to 1988. Nonemployed individuals keep a link with Social Security records as long as they receive Unemployment Benefits -those who have worked for at least six months- so our sample is not composed only of employees.

All employers must assign to each employee above 18 years of age to one of ten possible skill levels, as contributions to Social Security differ across levels. The skill levels broadly correspond to those fixed in collective contracts, a feature that allows us to assign each worker with the corresponding minimum wage.<sup>1</sup> In addition, the CSWH sample tracks any individual who is present in one of the 2005-2012 waves and who

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<sup>1</sup>The Social Security classification combines educational attainment and occupation. The upper four tiers correspond to (1) workers with a 4-year college degree (2) workers with a 2-year college degree (3) administrative or workshop managers and (4) specialized assistants without a college degree. The following six levels are split into white or blue collar workers. White collar workers are classified in groups 5-7, corresponding to office clerks (5), clerk assistants (6) and entry-level clerks (group 7). Finally, blue-collar workers are also split into three levels according to the level of qualification. That classification is comparable to that settled in collective contracts, as we discuss below.

remains registered with the Social Security Administration. In addition, the sample is refreshed with new sample members so it is still representative of the population in each wave (Bonhomme and Hospido, 2016). We complement the 2012 CSWH with the labor market histories of individuals in the sample at some point between 2005 and 2011, to avoid possible sample selection biases caused by workers leaving the labor market at periods of large job destruction.

### 3.1 Linking Datasets

The Register of Collective Agreements contains information about the province and the two-digit industry that determine the coverage of the agreement. We read the text of provincial agreements to assign to each contract a three-digit industry. We then matched the Register of Collective Agreements and the Social Security records using the three-digit industry of economic activity and the province of the establishment where the individual worked in 2008.

#### Sample coverage

Our sample is composed of all employees in the sample who worked between December 2007 and December 2008 in an establishment belonging to an industry covered by a province-level agreement in 2009 signed between October 1st, 2007 and April 1st 201. The resulting sample covers 50% of employees in 2009. The exclusion may be due because employees who are not covered by any collective contract whatsoever - 20-25% of Spanish employees, according to the OECD. In addition, we exclude workers in industries regulated by nation or region-level contracts.<sup>2</sup>

The focus on provincial collective agreements allows is not overly restrictive. Firstly, agreements bargained at the province level for a given industry can only improve the conditions settled for that industry in national or regional agreements. Secondly, as already mentioned, the province-industry level is the most prevalent bargaining unit, as it regulates the working conditions of 55% of all workers covered by any collective contract -see Card and de La Rica (2006). Finally, some of the workers in our sample could be covered by firm-level contracts covering 10% of employees. Such contracts are not

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<sup>2</sup>For example, 5% of the employees in Social Security records work in Financial Services or in Real Estate, covered by a nationwide contract. That contract would provide little identification, as we control throughout by province dummies.

readily identifiable in Social Security records, because firm identifiers are anonymized. However, until 2012, firm-level union contracts could only improve the conditions in province-level union contracts, so employees of those firms are indirectly affected by union contracts set at wider geographical level. Importantly, the variation of wage settlements across provinces and industries allow us to controlling for separate trends across provinces or industries (at the three-digit level).

Finally, we use all employees between 18 and 57 years of age in December 2007 who had been employed at the firm since at least the last quarter of 2007. The latter restriction guarantees that workers are continuously employed in the firm since the day when the oldest agreement in the sample was signed until the last day of 2008, so employees have similar working histories at the very beginning of the recession –i.e., they all have accumulated at least one year of tenure.<sup>3</sup>

## 3.2 Final dataset.

Table 2 compares the estimation sample to the total sample of employees. The total sample of employees contains 301,536 workers. Of those, 99,735 workers are covered by 538 province-sector contracts binding in 2009 and signed between October 2007 and March 2010.<sup>4</sup> The estimating sample overrepresents construction (23% vs 10%) and underrepresents finance (less than 1%, relative to 6% in the total sample) and manufacture and agriculture (12% vs 17% in the total sample).<sup>5</sup> According to the Social Security classification, 14.8% of workers in the estimation sample are high-skilled (i.e., have some college degree or work as managers), 34% have clerical jobs that do not require a college degree while 50% are blue-collar workers. 85% of employees are covered by an open-ended –or high firing cost- individual contract.

Columns (3) and (4) of Table 2 compare the characteristics of collective contracts for 2009 signed before and after September 15th 2008. Contracts signed before the fall

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<sup>3</sup>Including contracts signed earlier like, say, during the first quarter of 2007 would require us to use a sample of employees working in the firm already in 2006. A third of the working force in Spain is hired with fixed-term contracts, so using a sample of job stayers from January 1st 2007 to December 2008 would bias the sample excessively toward workers with high firing costs contracts.

<sup>4</sup>In the data we have 1305 province-industry cells. In many instances, a collective contract covers more than a single three-digit industry.

<sup>5</sup>The evolution of the construction boom and bust has been analyzed elsewhere. As we discuss below, the overrepresentation of construction plays a negligible role in the estimating sample, because wage settlements in that sector actually occur at the national level. Indeed, we use that sector to conduct placebo experiments.

of Lehman Brothers are more likely to cover more than one year (100% prior to 2008 vs 75% afterwards) and to specify an escalation clause (88% vs 44%). Workers covered by contracts signed after the fall of Lehman Brothers are 17 percentage points more likely to be "blue collar".<sup>6</sup> Similarly, workers covered by contracts signed before the regression are 14 pp more likely to be male or have an open-ended contract.

Column 5 recomputes the difference across worker characteristics after controlling for province and three-digit industry dummies. That is, we show the coefficients of a regression of each covariate on the dummy "contract signed before September 15th, 2008" and a full set of 50 province dummies and three-digit industries. After controlling for province and industry dummies, the magnitude of the differences become negligible. For example, the 17 percentage points in the fraction of blue collars falls to 0.1 percentage points. However, there is still a difference in wage growth across contracts signed before and after September 15th (1.24% unadjusted difference in wage growth for 2009 vs .85 pp after adjusting for industry and province)

### 3.3 Subsample with information on collective agreement wage levels

Unions and employers filling the form stopped recording minimum wages in the forms they submitted to the Register of Collective Agreements as of 2001. For the period spanning 1994-2001, the base wage by skill level was available for about 40% of contracts -excluding returns to seniority.<sup>7</sup> We have estimated the base wage level in December 2007 by updating 2001 levels using the revised wage growth settled in the full set of union contracts between 2001 and 2007. Namely, we inflated 2001 wage levels by the initially agreed wage increase in the contract plus any adjustment due to inflation escalation clauses. That procedure allowed us to compute minimum wages in 551 out of the 1305 province-industry cells.<sup>8</sup> The characteristics of the sample that includes

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<sup>6</sup> Waiters, cleaners and cooks are classified as blue collar workers in the Social Security definition.

<sup>7</sup>The form that unions and employers submit to the Ministry contains 10 possible minimum wage level, one for each skill group included in the Spanish Social Security System. It must be kept in mind that collective contracts typically set a much higher degree of detail than 10 skill groups.

<sup>8</sup>Our procedure assumes that the wage structure across skill levels is preserved whenever a contract is renewed (that is, each minimum wage increases by the same percent). We have coded minimum wages in some 200 collective contracts by hand and found that wages structure are indeed preserved across contracts (the R-squared of a regression of minimum wages set in collective contracts between 2007 and 2014 on a set of collective contract dummies is 97%).

contract-specific wage minima are presented in Tables 2 and A.2.

We note that construction is overrepresented in the sample with collective agreements, as it accounts for 30% of workers potentially covered by a sectorial agreement, while it is 20% in the matched sample of Social Security records and Collective Agreements. Furthermore, construction (Business Services, Health and Education) is even more (less) represented in the sample of contracts signed before the fall of Lehman Brothers. We discuss how we deal with that issue below, but it is important to keep in mind that construction cannot drive our results because since 2002 wage growth has been exactly the same across all provincial agreements in that industry -regardless of the date of signature of the provincial contract.

### **3.3.1 Summary statistics of the sample with minimum wage levels**

Table 2 compares workers' earnings in December 2007 to the minimum in their agreement for their province-industry-skill group. We find some slippage, as about 10% of workers have wages below the collective agreement minimum. Some of those workers may be upon special contracts –like those targeted for unskilled youth- that allow employers to pay wages below the minimum in the agreement.

In December 2007, 8.4% of workers had gross earnings between 1 and 1.1 times the minimum. 11% of workers in the sample with wage minima have earnings between 1.1 and 1.2 times the minimum. Half of the workers in the sample had monthly earnings 1.4 times the minimum or higher. Table A.2 provides information about that distribution by industry and skill level.

Figure 2 casts additional light on the set of workers who are most affected by collective agreements. In particular, we show the distribution of nominal wages for the construction sector in Madrid and Barcelona for laborers (group 10, according to the Social Security classification) and foremen (group 8). Each graph includes a vertical line indicating the base wage in the collective agreement for the specific skill level -computed as explained above. The level of the wages set in collective contracts are binding for laborers -low skilled workers- but it becomes less binding for foremen. We did not include the histogram for the highest skill level (architects) for whom collective agreement wages are not binding at all.

Two points are worth noting. Firstly, the graphs show the distribution of wages after a long expansion. Typically, minimum wages in collective agreements become especially

binding during a recession. Lacuesta et al (2012) document similar histograms in 1995 (i.e, soon after the 1993 recession) showing that minimum wages were binding in that period. Secondly, Table A.2 documents that collective agreements are specially binding in Construction or Food and Accommodation, but less so in Manufacturing or Business Services. That pattern is similar to the evidence about the bite of minimum wages in the United States provided by Dube et al (2011).

## 4 Empirical strategy

We estimate models of the transition from employment to unemployment as a function of the exact date when the corresponding collective contract was signed. As suggested in the descriptive statistics of Table 1, contracts signed in different periods settled different wage changes. Therefore, firms in the same industry (province) in 2009 were subject to a different degree of “nominal wage push” depending on whether their collective contract was signed early in 2008 (when the full extent of employment destruction that followed the fall of Lehman Brothers was hard to predict) or in 2009 -when bargaining parties could have observed accumulated employment losses of about 8%. The parameter of interest can therefore be interpreted as the slope of a province-industry level demand curve: higher wage increases should increase the probability of becoming unemployed in 2009.

### 4.1 Discontinuities in the arrival of aggregate information

The first empirical strategy uses the sharp change in expectations about the fall of Lehman Brothers in September 15th, 2008. We assume that the date of signature of an collective agreement reflects the information available to unions and employer federations, who update their information about industry- and province.level and aggregate economy shocks continuously. The second assumption that no comparable sharp change of information occurred before September 15th, 2008. Under those two assumptions, one can identify the wage and employment responses to the arrival of new macroeconomic information by using the following reduced form model

$$Y_{c,t} = \beta_0 + \beta_t 1(\text{signed\_2008})_{s,p,t} + \mu_{p,t} + \mu_{s,t} + \varepsilon_{c,t} \quad (3)$$

$Y_{c,t}$  is settled wage growth in collective contract  $c$  and period  $t$  (in the first stage), and measures of job destruction and job creation among workers covered by the contract (all those in intent-to-treat models).<sup>9</sup> The coefficient of the binary variable  $1(\textit{signed\_2008})_{s,p,t}$  captures any discontinuous change in wage settlements or employment losses around September 15th, 2008.

We introduce additional covariates to that basic model. The first is that wage settlements in union contracts and employment may have varied between 2009 and 2012 across industries and locations in very heterogeneous forms. Hence, we introduce as many geographical and industry controls as the data allows us to. We include dummies for each province (49, excluding Madrid) and up to 112 3-digit industry dummies (the excluded industry is cleaning services, an industry with at least one provincial agreement bargained in each of the quarters we consider).

Our identifying assumption identifies the employment response to the date of signature as the result of an amplification mechanism of a macroeconomic shock caused by imperfect wage adjustment. However, there can be other interpretations of our results. Wages and employment could be reacting to aggregate perturbations prior to September 15th, in which case we would not be really identifying an amplification mechanism to a well-defined macro perturbation. Alternatively, the interpretation would be less straightforward if employer federations and unions anticipating a downturn in economic activity were able to postpone the signature of a new contract. We deal with each of those concerns.

First, we test the assumption that the quarter of the fall of Lehman Brothers was indeed the moment when wage settlements changed abruptly by estimating [Equation 3](#) for an increasingly narrow window of dates of contract signature dates around the third quarter of 2008. Namely, we examine wage and employment outcomes of workers subject to contracts signed between four and one quarters away from that date.<sup>10</sup> By relying on contracts increasingly close to the fall of Lehman Brothers, we can arguably

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<sup>9</sup>Collective contracts signed between September 1st and September 15th 2008 are coded as if signed in the third quarter of 2008. We do not include an indicator for contracts signed in 2008q4, the reference period. The regression uses the matched Social Security Sample-Register of Collective Contracts sample, which is equivalent to running the regression using contract-level data, but weighting each contract by the share in total employment of each industry-province cell.

<sup>10</sup>We group dates of signature by dates, as on average we have some 20 collective contracts being signed every month. Grouping contracts in quarter of signature permits controlling for industry and province dummies, while finer disaggregation would not (say, months)

isolate the impact of the arrival of completely new information around that date. Note also that we run different versions of [Equation 3](#) for each window, thus allowing  $\mu_p$  and  $\mu_s$  to vary with distance to the date of the discontinuity.

Secondly, to address possible problems related to some party already bargaining before the fall of Lehman Brothers anticipating the extent of large losses in September 2008 and successfully postponing bargaining after that date, we use the date of expiry of the last contract to provide a sense of the magnitude of the problem.

## 4.2 Differences-in-differences estimates

The second empirical strategy builds on the fact that collective contracts set different minimum wages across skill groups and assumes that wage settlements in collective contracts do not affect the labor costs and employment chances of employees whose wages were well above the minimum in 2007, prior to the recession. The assumption would not hold if workers with different wage levels were perfect complements in producing output, as in that case, layoff decisions would not depend on the relative wage of each worker. We examine if worker labor costs increase job destruction by using variation in the date when the contract was signed, interacted with the distance to the minimum wage floor. We run the following Linear Probability Model:

$$\begin{aligned}
 Y_{i,s,p} = & \alpha_0 + \sum_{k=k_1}^{k=k_3} \alpha_k 1(\text{signed\_2008})_{s,p} * 1(W_{i,s,p}^{2007} \leq k\bar{W}_{i,s,p}) \\
 & + \sum_{k=k_1}^{k=k_3} \gamma_k 1(W_{i,s,p}^{2007} \leq k\bar{W}_{i,s,p}) + \theta_{s,p} + u_{i,s,p}
 \end{aligned} \tag{4}$$

$Y_{i,s,p}$  is the outcome of interest (wage growth in 2009, employment losses and job creation in that year, described below).  $1(\text{signed\_2008})_{s,p}$  is an indicator that takes value 1 if the collective contract was signed before September 15th, 2008 and 0 otherwise. The function indicates whether the wage increase settled for 2009 depends on the change in the information set of bargaining units after the fall of Lehman Brothers. We documented that wage growth was higher than average among collective contracts signed before that date.

$1(W_{i,s,p}^{2007} \leq k\bar{W}_{i,s,p})$  indicates the distance between the earnings of the worker in December 2007 and the collective agreement minimum. We experiment with three step

functions: wages that are 1.1 times the minimum wage for the skill group (i.e., 10% above that minimum), between 1.1 and 1.2 times, and between 1.2 and 1.4 times the minimum wage. The omitted group are workers whose earnings in December 2007 were at least 1.4 times the minimum wage in the agreement. As in the baseline model, we control for industry and province specific determinants of wage growth and employment losses by including 49 province dummies and three-digit industry indicators. As mentioned above, we include a collective contract-specific fixed effect  $\theta_{s,p}$ , an interaction of province and three-digit industry dummies that absorbs any trend in wage growth or employment affecting all workers covered by the agreement. We also control for nine dummies denoting the skill level of the worker -because minimum wages in collective contracts are specific for each skill group, as Table A.1 shows. Finally, we experiment introducing worker-level characteristics such as five age dummies in ten year bands and an indicator of whether the workers is covered by an open-ended or a fixed-term contract – a measure of the severance payment and the firing costs that a firm must incur to lay-off a worker.

In the first stage, the dependent variable is yearly wage growth between December 2009 and December 2008. The main aim of this specification is to test if changes in wage settlements were indeed most binding for workers whose wages in December 2007 were closest to the minimum. That sample used only contains full-time employees who stay in the firm in 2008 and whose earnings are not below the maximum contribution to the Social Security system.

The second specification uses as the dependent variable an indicator of whether the employee transited from employment into unemployment at some point in time between 2009 and 2010. Namely, we use indicators of monthly transitions into unemployment, of whether the workers experienced at least 3 months of unemployment between 2009 and 2010 as well as an indicator of the fraction of days not worked during the 2009-2010 period. Models with flows from employment into unemployment as a dependent variable estimate Intention-To-Treat parameters that measure whether workers whose wage was closest to the statutory minimum and were covered by contracts signed before September 15th 2008 had a higher chance of transiting into non-employment –relative to workers similarly close to their agreement floor but whose contract was signed in 2009.

The coefficient of interest is  $\alpha_1$ . Given the previous discussion, a positive value of  $\alpha_1$

in the first-stage indicates that bargained wages signed after the fall of Lehman Brothers adjusted to the business cycle by settling higher wage increases than contracts settled in the last quarter of 2008 or the early months of 2009. In addition, if wages settled in collective contracts act as minimum wages, pay increases must bind for workers whose wage was already close to the statutory minimum in their province-industry-skill group cell. Similarly, under the assumption of a downward sloping labor demand, workers covered by agreements settled in 2008 and close to the statutory minimum must have had a higher chance of losing their job, because their employers would have experienced larger wage costs than the rest.

## 5 Results using the full sample

We start by examining the wage growth for 2009 settled in collective agreements signed around September 15th, 2008. We examine wage growth as settled in all province-industry contracts, as a function of the quarter when the contract was signed. We consider 5 quarters before Lehman Brothers and 6 quarters afterwards. Figure 1 plots the estimated wage growth as settled in collective contracts signed each quarter between 2007q4 and 2010q2, along with the estimated standard errors -corrected for heteroscedasticity and arbitrary correlation across workers covered by the same collective agreement. The estimate presented is the quarter-specific coefficient  $\pi_q$  in the regression:

$$\log(w_{s,2009}) - \log(w_{s,2008}) = \pi_0 + \sum_{q=-4}^{q=6} \pi_q 1(\text{sign\_}q = q) + \varepsilon_q$$

Wage settlements effective in 2009 were very similar across contracts signed in 2007q4 through 2008q3 and close to the pre-LB average wage growth of 3%, not shown in the graph. However, contracts signed in 2008q4 settled a wage increase 50bp lower than those settled a quarter before, and the difference is statistically different from zero at the 95th percent confidence level. Contracts signed in 2009 and early 2010 settled progressively smaller wage increases. For example, wage settlements for 2009 signed retrospectively in 2010q2 included wage growth 150bp lower than those signed in 2008q3. The similarity in wage growth settlements between 2007q4 and 2008q3 lends support of an absence of meaningful macroeconomic shocks before the fall of Lehman

Brothers.

We make two notes. Firstly, we model the cross-sectional dispersion in nominal wage growth in 2009 as a function of the date of signature –i.e., there is no time variation involved, so we do not adjust for inflation. Secondly, the lower wage growth in agreements signed later in 2010 does not necessarily imply that the adjustment of wages was sluggish. As mentioned above, the date of signature after a large shock may be endogenous. For example, shortly after the fall of Lehman Brothers, unions and employers may have faced a considerably degree of uncertainty about the magnitude and duration of the recession. In such circumstances, some bargaining units may have chosen not to renegotiate the agreement. In the context of the practice of bargaining in Spain, a short delay in contract renegotiation freezes nominal wages at the levels specified in the expired agreement, an interim solution while uncertainty is resolved. Hence, after September 15th 2008, the profile of wage settlements by quarter of signature may just reflect heterogeneity in the duration of waiting periods, not the response of wage growth to the arrival of new information –see Danziger and Neuman (2005).

To get a grasp of the magnitude of wage increases, Table 1 compares wage growth of each of the 568 collective contracts in our sample, collapsing observations in two periods: contracts signed before September 15th 2008 and contracts signed afterwards. Each contract is weighted by the number of workers covered by the contract in the Social Security sample. According to the new measure, contracts signed before the fall of Lehman Brothers settled for a wage increase that was 90 basis points (0.9 percent) higher than that settled in contracts signed after that date. The standard error (adjusted for heteroscedasticity) is 0.12 percent, leading to an F-statistic of 49. A second element that adds to the persistence heterogeneity in wage growth is the fact that many contracts signed before the fall of Lehman Brothers expired in 2010 or 2011, so wage growth could not be adjusted until 2012. The second column uses as the dependent variable the combined wage growth in 2009 and 2010. The estimated coefficient is about 1 percent when one combines wage growth in 2009 and 2010, very similar to that in the first column. Finally, the third column documents the extent of heterogeneity across contracts by comparing wage growth in settlements for the 2009-2011 period signed before September 15th, 2008 to all contracts signed after that date. The difference in wage growth over the three-year periods increases to 2.37 percentage points (standard error: .36 percent).

In sum, the staggering of labor contracts caused variation in wage growth settlements across industries and provinces between 2009-2011 between 1 and 2.37 percent, forcing some employers to apply wage increases that reflected the macroeconomic situation before the recession while employers covered by contracts signed in 2008q4 or afterwards could apply lower wage increases. Furthermore, as we document shortly, the wage growth of job stayers suggests that those settlements were binding.

## 5.1 Employment outcomes, full sample.

We use several methods to determine if the cross-sectional dispersion in contractual wage growth caused by staggered labor contracts results in different employment dynamics across province-industry cells. We start by examining the response of job destruction.

### 5.1.1 Job destruction

The first method compares the monthly probability of job loss and the incidence of non-employment between in 2008 and 2012 of workers covered by contracts signed between the last quarter of 2007 and the second quarter of 2008 (i.e., signed prior to the fall of Lehman Brothers) and those signed afterwards. This is done by running [Equation 3](#) for each month spanning January 2009 and December 2012. For convenience, [Figure 2](#) shows monthly estimates of the monthly probability of job loss between January 2009 and December 2012 along with the 95% point confidence interval. [Figure 2](#) shows that the monthly probability of job loss in 2009 does not depend much on whether contracts were signed before or after 2008q3. However, there is a higher probability of job loss in 2010 among workers covered by contracts signed before September 2008, peaking at 0.005 percent (standard error: .002, not shown) in December 2010. Similarly, workers covered by collective contracts exposed to wage rigidity had a higher probability of job loss in 2011 and 2012, albeit point estimates are somewhat imprecise. Consistent with the findings in the left Panel of [Figure 2](#), the right panel documents that the probability of being non-employed among workers originally in contracts exposed to wage rigidity increased steadily after 2010, staying around 1% between the end of 2010 until the end of 2012, four years after the macroeconomic shock had happened. [Figure 2](#) suggests two main results. The first is that wage rigidity leads to job destruction with a lag

of about one year and a half (the shock happened around the last quarter of 2008 and job destruction is observed in the data in mid-2010). The second result is that the employment losses of workers exposed to wage rigidity are much more persistent than job destruction, and are visible four years after the macroeconomic shock. Both findings suggest that wage rigidity amplifies the impact of aggregate fluctuations in a rather persistent manner. The reduced form estimates in Figure 2 do not fully explain the reasons for the delay in employment losses. Adjustment costs may have led firms to postpone lay-offs or, alternatively, several macroeconomic shocks may have accumulated between 2009 and 2012, contributing to higher job destruction. We come back to this point later.

Thus far we have interpreted the estimates in Figure 4 as if the developments after September 15th, 2008 reflected an unexpected aggregate shock. To lend support to that interpretation, we re-run [Equation 3](#) for an increasingly narrow window of contract signature dates around the third quarter of 2008. If pre-existing macroeconomic changes were operating, the estimate of the impact of committing to a high wage increase pre-September 15th would vary across specifications. Table 5 regresses the probability of job destruction of each year between 2009 and 2012 -conditional on remaining in employment at the beginning of the calendar year- on the indicator "contract signed before September 15th, 2008" for various time windows around the third quarter of 2008. In keeping with the results in Figure 2, the estimates in the first row of Panel A in Table 4 suggest that the extent of job destruction in 2009 was similar across workers covered by contracts exposed to wage rigidity and those that were not. However, the probability of job loss during 2010 is positive and about 1% higher between among workers covered by contracts signed before the downturn in September 2008 for basically all time windows. The estimates become more imprecise when we compare contracts signed in 2008q2 to those signed in 2008q4 (the point estimate being 2.1 percent, with a standard error of 1.37 percent). Finally, employees whose firms signed a collective contract before September 15th, 2008 and those whose firms signed afterwards experienced similar probabilities of job destruction in 2011 and 2012 does not depend on wage rigidity. Figure 3A and 3B illustrate graphically the results. The average probability of job destruction during 2009 is similar regardless of whether the contract was signed before or after September 15th, 2008. However, contracts signed before the fall of Lehman Brothers experience a higher rate of job destruction.

A second possibility is that some employer federations or unions had anticipated the extent of job losses occurring during the last quarter of 2008. For example, either union or employer federations anticipating a downturn during the latter quarter of 2008 could have delayed negotiations until 2009. The delays between previous contract expiration and new contract signature shown in Table 1 suggest that contracts are typically signed after the expiration of the pre-existing collective contract. That is, among collective contracts binding in 2009 and having expired by the end of 2006, those representing 80% of the labor force were signed during 2007, and only 16% were signed in 2008 -see the first row of Table 1. Among collective contracts binding in 2009 and having expired by the end of 2007, those covering 83% of the labor force were signed in 2008 and only 16% were signed in 2009. In both cases, very few contracts were signed before the expiration of the previous agreement. Unsurprisingly, basically all collective contracts signed during 2009 had expired by the end of 2008. Thus, a strong determinant of the date of signature is when the previous collective contract had already expired -in turn, determined by perceptions of the bargaining parties at the time of the expired contract's signature. According to Table 1, delays in bargaining could have happened among those having expired in 2007 and that were signed only in 2009. Those cover 14% of workers covered by contracts expiring in 2007. While we have not still examined the robustness of the results after excluding those, we note that they look like a limited fraction of the sample.

### **5.1.2 Job creation**

The exogenous variation in the degree of wage rigidity across province-skill cells allows us to examine if wage rigidity alters the incentives to create new jobs. Wage rigidity could affect employment levels operate through two main channels. The first is a labor demand effect that induces firms facing lower increases in labor costs to expand hiring. A second channel is job creation among workers who were already non-employed as of the last quarter of 2008. The final impact is a volume effect related to workers expelled from shrinking industries moving to find those with lower labor costs. We examine the second channel by running models equivalent to [Equation 3](#) where the dependent variable is the number of workers who find their first job or unemployed as of the last quarter of 2008) as a fraction of employment in the province-industry.in 2007q4. The regression is conducted at the province-industry level and controls for industry

and province dummies. The results, shown in Figure 6, suggest that job creation was rather similar across province-industry cells regardless of whether or not firms could adjust wages after the downturn of activity in the last quarter of 2008. In that sense, Figure 2 suggest that the inability to renegotiate wages mainly affected job destruction. We have not still examined the reallocation component mentioned previously (whether previously displaced workers make their way into less wage-rigid industries).

### 5.1.3 Magnitude of the impact

To put magnitudes in perspective, wage rigidity in province-industry cells -measured by whether firms could renegotiate wages after a large aggregate shock- resulted in 1.1% higher wage growth between 2009 and 2011 (Table 4, column 2). Secondly, workers in those province-cell industries experienced about 0.85% higher probability of job loss during 2010 (Table 5, row 2, column 1). Those estimates are consistent with an elasticity of job destruction to settled wages of  $0.77 = (0.85/1.1)$ , a large estimate. However, that estimate does not take into account the persistence of job losses through extended unemployment chances over longer periods.

## 5.2 Heterogeneity of the impact: the role of contract duration

One key dimension through which wage rigidity affects employment dynamics is through contract length. As documented in Tables 2 and 4, collective contracts are typically set for more than one year, implying that, as renegotiating wages during the life of the agreement is very costly for a given firm, a collective contract settling high wage growth for two or more years increases the cost of labor much more than another contract with shorter duration that can be renegotiated shortly after the macroeconomic shock -for example, already by the end of 2009.

To test the hypothesis that longer contract duration leads to more intense job destruction, we assume that the duration of contracts signed *prior to* the fall of Lehman Brothers is exogenous, while the duration of contracts signed after September 15th is not. Hence, we generate two “treatment” groups within collective contracts signed before September 15th, 2008. The first group are contracts set to expire at the end of 2009 while the second group contains contracts settled wage growth for 2009 and 2010 (at least). All the contracts signed after September 15th are all “controls”, in the

sense that the bargaining parties could all adjust wages and contract duration to the aggregate shock, so we do not split those contracts by duration.

The left Panel of Figure 5 shows the impact of the dummy "signing a collective contract before Lehman Brothers" on the monthly probability on non-employment among workers covered by "short" collective contracts (those who could be renegotiated by the end of 2009). The point estimates vary between -0.5 and 0.5 percent and are never statistically different from zero at conventional significance levels. Instead, the impact of "signing a collective contract before Lehman Brothers" on the probability of being non-employed is consistently about 1% among workers covered by contracts expiring in 2010 or later. Table 6 redoes the analysis for various windows of dates of signature around September 15th, 2008, confirming that the probability of job loss among contracts that could be renegotiated for 2010 is not statistically significant for any specification considered (see Table 6, Panel A, row 2), while it lies between 1% and 1.8% percent when we examine contracts ending in 2010 or later (Table 6, Panel B, row 2).

Those results partially explain why Figure 2 detects negligible rates of job destruction the 2009, the year after the macroeconomic shock: among workers covered by collective contracts signed before the fall of Lehman Brothers, a fraction of them could renegotiate wages one year after the shock, thus experiencing negligible rates of job destruction. On the contrary, job destruction was confined to province-industry cells exposed to at least 2 years of wage rigidity.

### 5.3 Heterogeneity in responses: adjustment costs

The findings in Figure 2 suggest that job destruction rates were highest about one year and a half after the large aggregate shock. Such delayed response is consistent with the presence of adjustment costs, of which firing costs are a prominent example. As it is the case in many European economics, employees under a fixed-term contract can be dismissed cheaply after contract expiration, while employers must pay rather large redundancy costs to dismiss workers on an open-ended contract (in the period we analyze, up to 45 wage days per year worked with a limit of two full year wages). We use that variation in firing costs across Spanish employees to examine the pattern of employment responses to wage rigidity.

Figure 6 shows the monthly rates of job destruction (left panel) and of non-

employment (right panel). We note that, within contracts signed before the fall of Lehman Brothers, the rates of job destruction among fixed-term workers are larger than for the full sample, peaking at 1.5% in 2010. That finding is consistent with previous evidence pointing at much larger churning among employees in fixed-term contracts than among other workers. Turning to the probability of non-employment, the large response of employment destruction to wage rigidity among employees in fixed-term contracts result in substantially higher responses of non-employment immediately upon job loss. For example, employees originally in fixed-term contracts in 2007 and exposed to wage rigidity are 2% more likely to be non-employed in 2010 than comparable individuals who were not exposed to wage rigidity (see the right-hand panel of Figure 6). The estimate was 1.5% for workers initially covered by open-ended contracts.

However, large rates of job destruction do not imply larger chances of non-employment over longer horizons. The response of non-employment to wage rigidity among workers who were easy to dismiss is less than 1% four years after the aggregate shock, very similar to the full sample estimate, shown in Figure 2. Table 7 compares the differential monthly probability of job destruction and of being in unemployment among workers whose collective contract was signed before Lehman Brothers, depending on the level of severance payments. During 2010 and 2011, the probability of being in unemployment is *higher* among workers covered by high-severance payment contracts.

One possible explanation of the findings in Table 7 is the fact that firing costs in Spain take the form of large severance payments, that may sustain longer periods of job search. Alternatively, Koenig et al (2017) provide evidence on the role of reference wages in forming reservation wages. Workers under open-ended contracts have typically accumulated higher wages through longer seniority and may be choosier in selecting new jobs.

## 6 Wage and Employment responses close to the minimum

### 6.1 Wage responses

We determine how binding collective contracts were during 2009 and 2010. Table 3 shows the estimates of [Equation 4](#) when the dependent variable is the individual's wage

growth computed between December 2008 and December 2009. The sample is restricted to workers who stay in their job in 2009 (and 2008) and who worked full time in both months. We also excluded workers whose earnings were censored at the Social Security maximum contribution.

Table 8 examines wage growth for workers whose monthly earnings were at most 10% higher than the statutory minimum in the province-industry-skill group cell, between 10% and 20% or between 20% and 40%. The omitted group are workers whose monthly earnings in December 2007 were 40% higher than the minimum in the collective agreement. Note that the main impact of date of signature is a contract-level characteristics and it is not identified in models that include agreement-level fixed effects. However, the interaction of the date of signature and the distance to the minimum in the collective agreement is identified.

The estimates in the fourth row, first column of Table 8 suggest that wage growth among job stayers whose earnings in December 2007 were very close to the collective contract minimum wage was 1.86 percentage -points higher than wage growth in the omitted group. Such pattern can be partly attributed to mean reversion –wages originally close to the agreement minimum cannot fall much. However, wage growth was especially high among those workers close to the minimum in December 2007 and whose collective contract was signed before September 15th, 2008. The coefficient, shown in Table 3, column 1 row 1, is .010 (standard error: .0042). That is, wage growth was 2.86 percent ( $=.01+.0186$ ) among job stayers with earnings close to the minimum in contracts signed before Lehman Brothers. That pattern is consistent with the notion that newly signed contracts signed after a large aggregate shock settle low wage increases, and those are binding. The estimate is remarkably similar in magnitude when we control for collective agreement fixed effects, suggesting that sample selection or idiosyncratic shocks affecting province-industry cells do not play a large role in determining wage growth.

The validity period of most contracts signed before September 15th 2008 spanned both 2009 and 2010, so the wage growth among those settlements is also likely to be higher in 2010 than that among contracts signed afterwards. The estimates in the third column, first row of Table 8 confirm that hypothesis: wage growth between 2008 and 2010 was 1.76 percent higher among contracts signed before September 15th 2008. Crucially workers whose monthly earnings in December 2007 were at least 20%

higher than the collective contract minimum experienced rather similar wage growth -regardless of the date of signature. Such pattern confirms that collective agreements mainly affect the evolution of minimum wages in the skill-province-industry group. One could argue that fairness considerations could lead employers to set similar wage increases for all workers despite of their “distance to the minimum”. The results in our sample do not support that hypothesis.

## 6.2 Employment responses

Next, we compare the employment outcomes of workers covered by collective agreements signed before the fall of Lehman Brothers to those of the rest of workers. Namely, we run models similar to [Equation 4](#) where the dependent variable is an indicator of non-employment in each month between January 2009 and December 2010. Guided by the results in [Table 8](#), we select the group of workers whose wage grew most: those whose monthly earnings in December 2007 were at most 1.2 times the minimum wage in the agreement. [Figure 7](#) shows the increase in the chances of not working starting in March 2009 among workers whose collective contract for 2009 was signed before the fall of Lehman Brothers (and pre-recession earnings below 1.2 times the minimum wage). The differential impact of date on signature on the probability of being non-employed fluctuates between 3% and 4%, positive and statistically different from zero still in late 2010. Note also that the shape of the profile is similar to that shown in [Figure 2](#), which uses all workers but does not control for proximity to the minimum wage.

[Table 9](#) presents OLS regressions that link the probability of transiting from employment to unemployment to the date of signature -interacted with distance to the statutory minimum. Each specification presents two measures of transitions from employment to unemployment. The first is an indicator of job loss during 2009, defined as the event "having three months or more of unemployment during 2009 and 2010". In addition, we also use as an alternative dependent variable the fraction of days not worked during 2009 and 2010.

Columns 1 and 2 in [Table 9](#) present estimates of [Equation 4](#) using as a dependent variable an indicator of having spent in unemployment at least three months during the period 2009-2010. The standard errors are corrected for heteroscedasticity and arbitrary correlation between workers who are covered by the same collective contract.

The pattern of the point estimates in the first and third row, first column of Table 4 implies that within the set of workers whose wage was less than 1.1 times the statutory minimum, those ruled by contracts signed before the fall of Lehman Brothers had a 5.7 higher percentage chance of transiting into unemployment (row 1, column 1 of Table 9). The estimate falls slightly to .053 when we include collective contract fixed effects. Among workers covered by agreements signed pre- Lehman Brothers fall but whose earnings were a bit higher -between 1.1 and 1.2 times the minimum wage- the estimate is .047. The lower magnitude (.047 vs .053) is consistent with the hypothesis that the impact of date of signature should fade away as we look at workers progressively further away from the minimum wage. Nevertheless, the estimates are not statistically different from each other.

We find little evidence of a differential date of signature effect among workers whose monthly earnings in December 2007 were at least 20% higher than the minimum wage for their skill group. For example, for workers whose pre-recession earnings were between 20 and 40% above the minimum and whose collective contract was settled before the fall of Lehman Brothers the estimate is .0116 (standard error .011) –see Table 9, second column, third row.

In the fifth column of Table 9 we use as the dependent variable the fraction of days spent in non-employment. The results are qualitatively similar to those in Columns 1 and 2. Within the set of workers whose monthly earnings in December 2007 were only 10% higher than the minimum wage in the agreement, those covered by a collective contract signed before the fall of Lehman Brothers spent 3.3 percent more time in unemployment than the rest. The standard error is 1 percentage point. Among workers whose earnings in December 2007 were between 10% and 20% higher than the relevant minimum wage, the impact of the date of signature on the percent of days not worked in the 2009-2010 period is 2.1 percent. The estimate is lower than the corresponding one for workers very close to the minimum (.021 vs .033), and it is statistically significant at the 7% confidence level.

### **6.2.1 Pre-existing trends.**

One concern about the evidence in Table 9 refers to the possibility that employment destruction was already falling in the province - industry cells that reached an agreement between the last quarter of 2007 and the third quarter of 2008. To test for such

possibility, we examine rates of job destruction between 2005 and 2008, a period that includes the net employment losses in the last quarter of 2007 and early 2008. For job losses in 2004-2005, we replicate our previous sample selection criteria: we select a sample of workers who stayed in the firm through 2004 and compute the minimum wage in the collective agreement as of December 2002. We then run a specification very similar to [Equation 4](#) but where the worker-specific earnings levels are those of December 2002. We do the same for job losses in 2006-2007 and 2007-2008

The results are shown in Figure 8 and suggest no differential probability of job loss in either of the three periods reconsidered. Industry-province cells that signed collective contracts for 2009 between October 2007 and September 2008 experienced no differential employment losses in none of the three periods considered, regardless of whether workers were close or far from the collective contract minimum.

### **6.3 Heterogeneity of the impact**

The results in Table 9 suggest that the impact of wage rigidity on employment losses is driven by “long” collective contracts signed before the fall of Lehman Brothers. To achieve more precision in the sample split, we lump all workers whose monthly earnings were at most 1.2 times the minimum in their collective contract. Firstly, the yearly wage growth was relatively similar in 2009 between “long” and “short” contracts signed before the fall of Lehman Brothers. Wage growth in “short” contracts signed for 2009 only was .8 percent while the corresponding increase was 1.1 percent in the “long” contracts for that same year. However, workers covered by “long” contracts signed before Lehman Brothers –and whose earnings in 2007 were close to the minimum–experienced on average 2.9 percent more days in unemployment than similar workers covered by agreements signed afterwards. On the contrary, workers covered by “short” collective contracts experienced only .9 percent more days in unemployment. In other words, the higher the predictable growth in labor costs -due to long contracts with high wage growth- the higher the rate of employment destruction. Those results were present in the full sample, and we do not comment them in detail.

Finally, we split the sample by whether or not worker has a “high firing cost contract”. It must be noted that our selection criteria of requiring workers to be working at the firm for at least 2008 results in a sample with a rather large share of employees with an

open-ended contract. The results in Table 9 suggest similar employment losses among both sets of workers, a result that, as discussed in Table 7, mixes the higher rate of job destruction experienced by workers with low severance payments with the higher probability of leaving non-employment

#### **6.4 Impacts by industry: a placebo with construction.**

Table 11 examines differential effects by industry. The specification serves two purposes. Firstly, there are sectors where collective contracts are bargained at the provincial term but that do not set province-specific wage growth because they merely adopt the wage growth set in a nationwide agreement. Construction is one such sector. That is, even though there is within province dispersion in the date of signature of construction agreements, wage growth does not vary across provinces.

We use that feature of provincial bargaining in the construction sector to provide evidence in support of our identifying assumption. Our main hypothesis is that differences in wage growth across provinces and industries caused by the information available to the parties at the time of contract signature was associated to job losses. However, collective contracts specify working conditions –like working hours, or other conditions that may affect firms’ labor costs in ways that are not immediately visible in wage growth. Agreements with a dispersion in signature dates but not in wage growth permit identifying the impact of changes in other restrictions in collective contract on employment destruction. Alternatively, that variation in signature dates permits identifying the role of confounding factors that correlate both with signature date and with employment destruction.

The first column of Table 11 runs [Equation 4](#) for workers in construction. The interaction between date of signature and distance to the wage floor is .0015, implying that workers close to the minimum statutory wage level as of December 2007 and whose collective contract was signed prior to September 15th, 2008 lost 0.15 % working days more than workers in the same province whose contract was signed after that date. Such lack of results counters the hypothesis that signature dates correlate with employment destruction for reasons unrelated to wage growth.

## 6.5 Estimates in different industries.

The impact of contract staggering on the transmission of the aggregate shock could vary across industries for various reasons. Firstly, the employment impact depends on the degree of competition for workers, because minimum wage increases may not necessarily affect employment levels if firms have monopsony power. Secondly, the employment impact may depend on the employers' surplus in matches that set wages close to the minimum in the collective agreement. We document a relatively important set of workers whose wages are close to the minimum wage. If such concentration reflected a high bargaining power of unions, those matches would be especially "fragile" in the presence of a negative aggregate shock. That would not be the case if a lack of competition among employers results in wages compressed around minimum in collective contracts.

We document the separate impact of signature dates in three different industries: The first is manufacturing. The second is a composite of non-tradable service sectors, including Retail, Wholesale, Food and Accommodation and Transportation. Finally, we lump the rest of services as a different industry –such sector includes, among others, services to businesses, health and education.

The estimates in the second column of Table 11 show that date of collective contract signature had very little impact, if any, among manufacturing workers. The estimate is  $-.096$  (standard error:  $.055$ ) negative and imprecise. The lack of an effect of contract staggering in this case may be partly attributable to the comparatively small sample size, but also to the fact that the distribution of wages in manufacturing is not that compressed around minimum wages –see Table A.2. Firm level bargaining is relatively more common than in manufacturing than in other industries.

Finally, the last four columns of Table 11 provide estimates for the services industry. Column 3 shows the estimate of the interaction of signature date and distance to the agreement minimum on employment losses in Trade (including retail and wholesale), Food and Accommodation as well as Transportation. Column 4 shows employment impacts in the remaining industries, which include Business Services and Health and Education. We focus on the estimate of the coefficient of the interaction between signature date and the distance from the collective contract minimum in a regression that explains transitions into non-employment that lasted at least three months. The results suggest that the effect of signature date are confined to services. Employment losses among workers close to the minimum and whose contract was signed before the fall

of Lehman Brothers were 4.6 percentage points higher than among the rest (standard error: 2.4). The estimates in personal or business services, health and education is even larger. A possible interpretation is that a substantial amount of job matches in services carry a relatively small surplus for employers –at least small enough such that those matches are discontinued when wage growth increases by about 1-2%.

## 7 Conclusions

A large literature has estimated the extent and relevance of wage rigidity in different economies as well as the degree of cyclicity of wages. However, it is difficult to assess empirically to what extent those possible wage rigidities have real effects -that is, they translate into lower employment levels after a negative demand shock. Our study exploits the automatic extension of collective contracts in Spain to identify a particularly salient source of wage rigidities: minimum wages that apply to all employees of firms in the same industry-province cell. In addition, we use the large number of industrial and provincial agreements to identify the impact on employment of the cross-sectional dispersion in wage growth caused by wage rigidity –i.e. contracts signed at different moments in time could react differently to a large aggregate shock. Combining information on the exact dates of collective agreements bargaining periods and longitudinal Social Security records we find that, during the last recession, one percentage points higher wage growth caused by the inability to renegotiate contracts led to one percent higher rates of job destruction -consistent with an elasticity of labor demand of -1. We find tentatively low impacts on job creation. We also document that the impacts of wage rigidity may have medium- and long- run consequences. We find that job destruction in 2009-2011 resulted in a 1 percent higher probability of unemployment still in 2012, four years after the macroeconomic shock had been observed.

We illustrate the channels of job destruction by using a subsample with information on the collective agreement minimum wage level as of 2007 (prior to the recession), we find that both wage growth and employment losses were confined among workers whose wages are close to the minimum in the collective agreement. The results imply that job stayers with pre-recession earnings 10% higher than the collective contract minimum and covered by a contract signed before the fall of Lehman Brothers experienced wage growth 2 percent higher than the rest. On the other hand, those workers

with pre-recession earnings at most 10% higher than the minimum and whose contract was signed before the fall of Lehman Brothers spent about 3 percent more days in non-employment. Altogether, the evidence is consistent with the notion that contract staggering constitutes a source of employment fluctuations, consistent with many macroeconomic models.

The evidence is relevant for the policy debate. Firstly, our estimates suggest that the magnitude of wage increases among job stayers is similar to the percent time lost among job losers between 2009 and 2010. An interpretation is that, once one takes flows into non-employment into account, minimum wages in collective contracts have not helped in maintaining employee's earnings constant during a recession. Secondly, the particular form of wage rigidity created for the automatic extension of provincial agreements and multi-period bargaining had a role on the employment destruction during the 2008-2009 recession in Spain. Further research must assess how important this source of wage rigidity was during other recessions. In addition, the role of downward wage rigidity on job creation is a key to assess the future implications of latter labor reforms in Spain and, more generally, to understand how the labor market reacts to economic shocks. These topics are left for further research.

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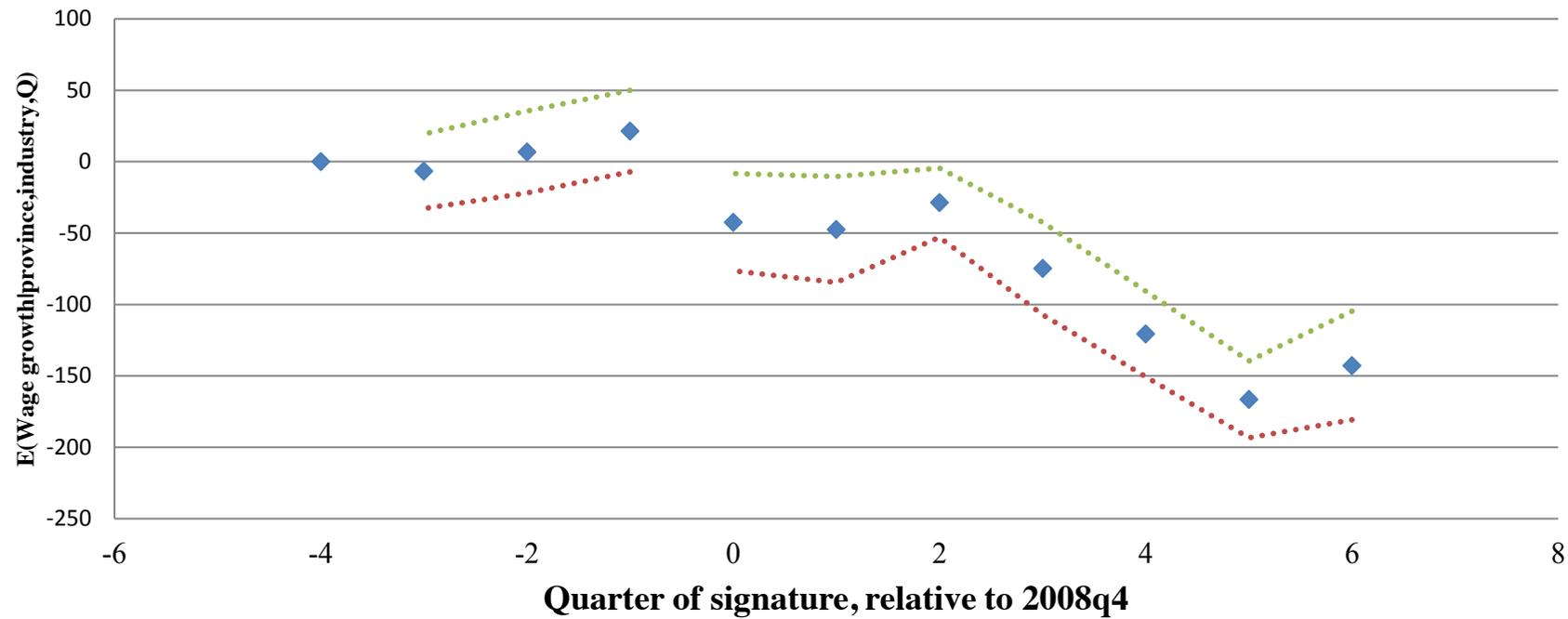
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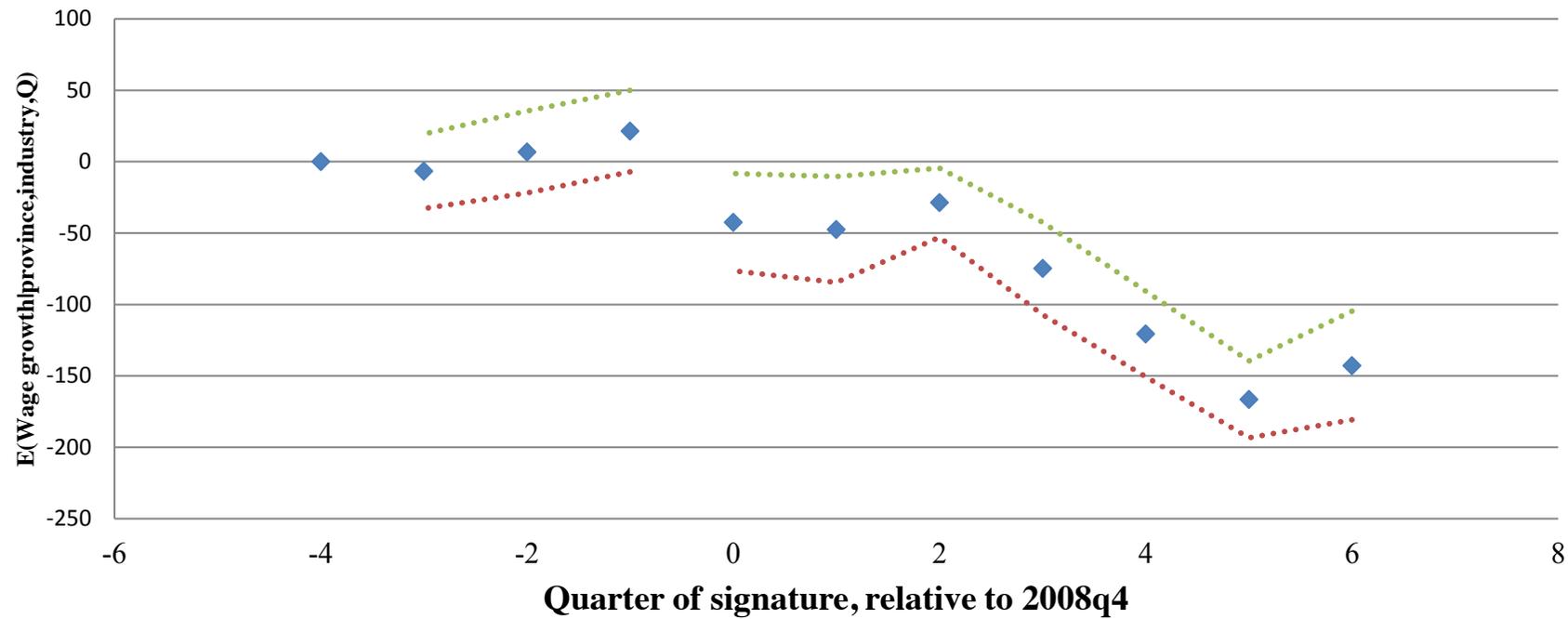
**Figure 1: Agreed wage growth for 2009, by quarter of signature**



The graph shows quarter dummies of a regression where:

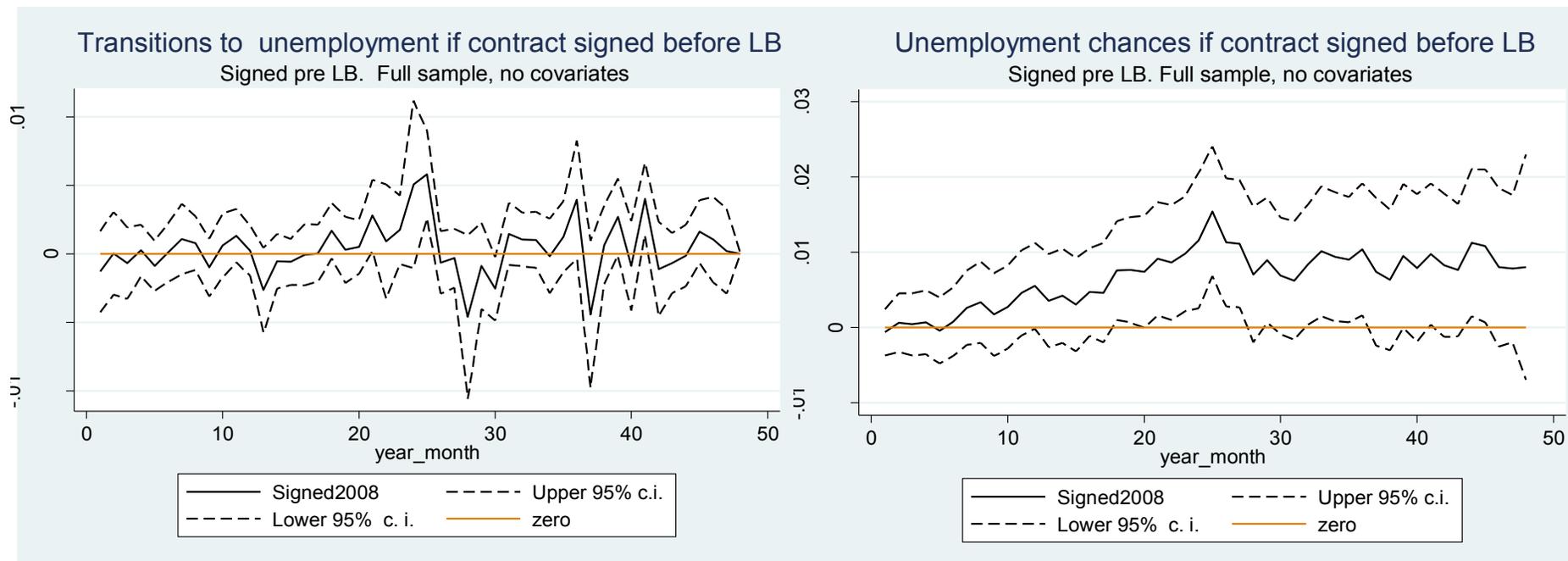
- (a) The dependent variable is agreed wage growth for 2009 (in basis points)
  - (b) Quarter of contract signature dummies between 2008q1 and 2010q2, 2007q4 omitted.
  - (c) The covariates are 49 province dummies and 141 industry dummies
- Standard errors permit arbitrary correlation at the province-industry cell.

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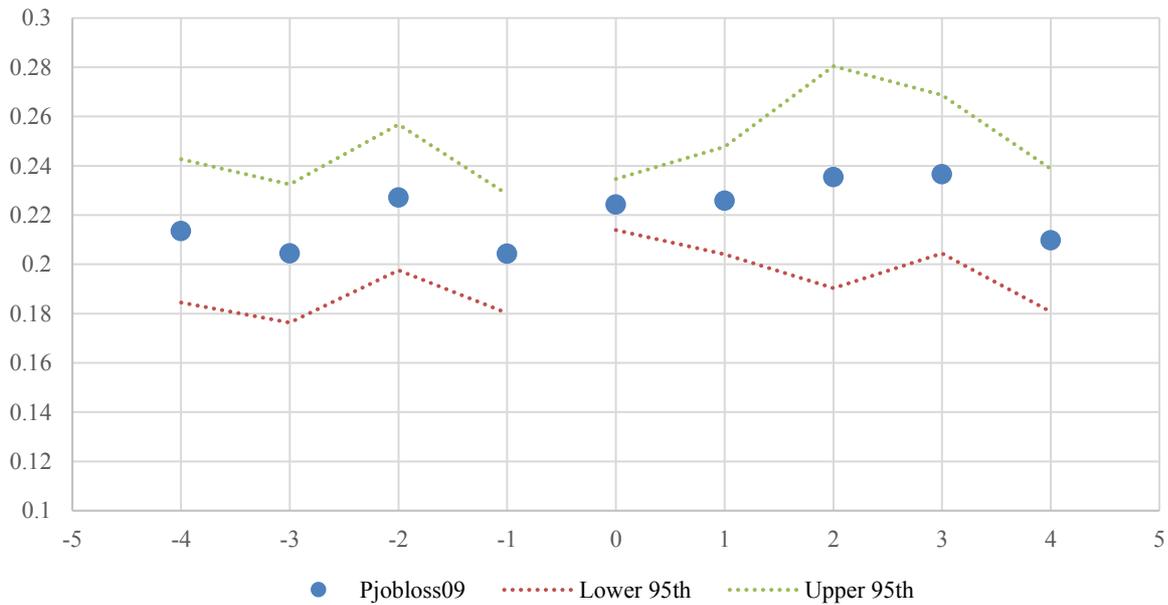


**Figure 2: Job destruction and subsequent employment outcomes among workers covered by province-industry agreements signed between October 1st 2007 and September 15th 2008**

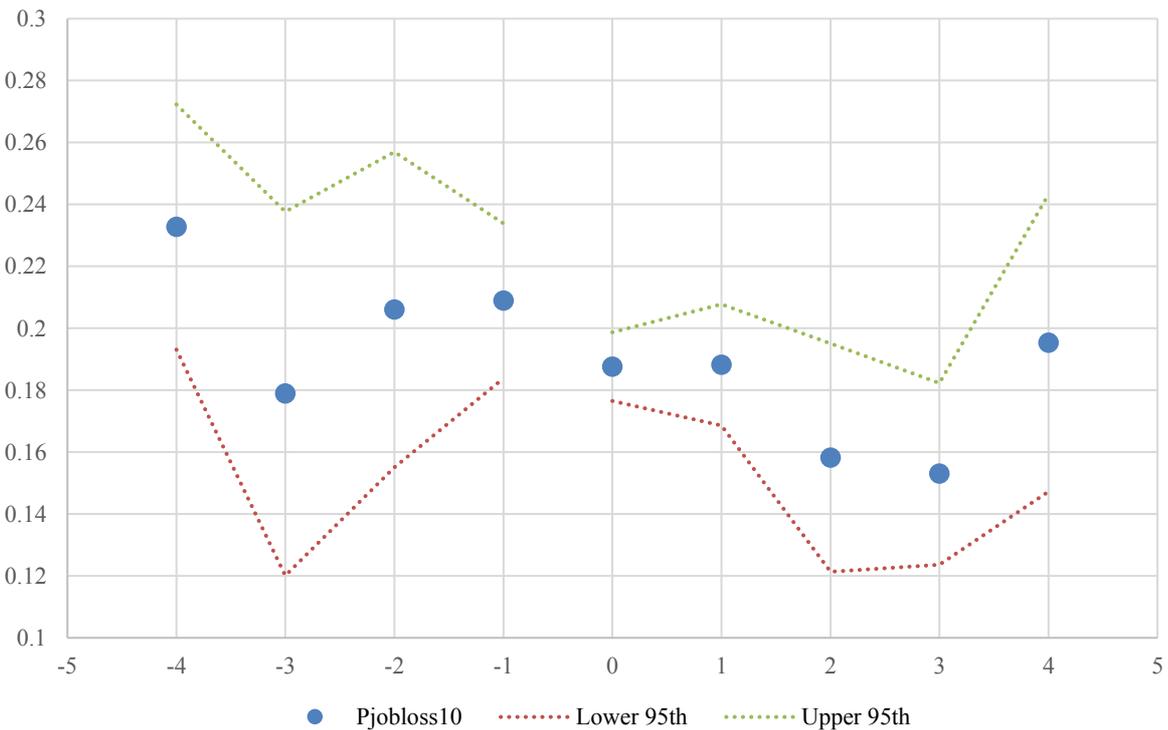
The Figure in the left plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before September 15th, 2008" in 48 separate regressions with an indicator of job loss among in each month between January 2009 and December 2012.

The Figure on the right plots the same regressions but using as a dependent variable the fraction of individuals who were working in the firm as of December 2008 who are not employed in each month. All regressions control for 111 month-specific three-digit industry and 49 month-specific province dummies

**Figure 3A: Probability of job loss in 2009, by quarter of signature**



**Figure 3B: Probability of job loss in 2010, by quarter of signature**

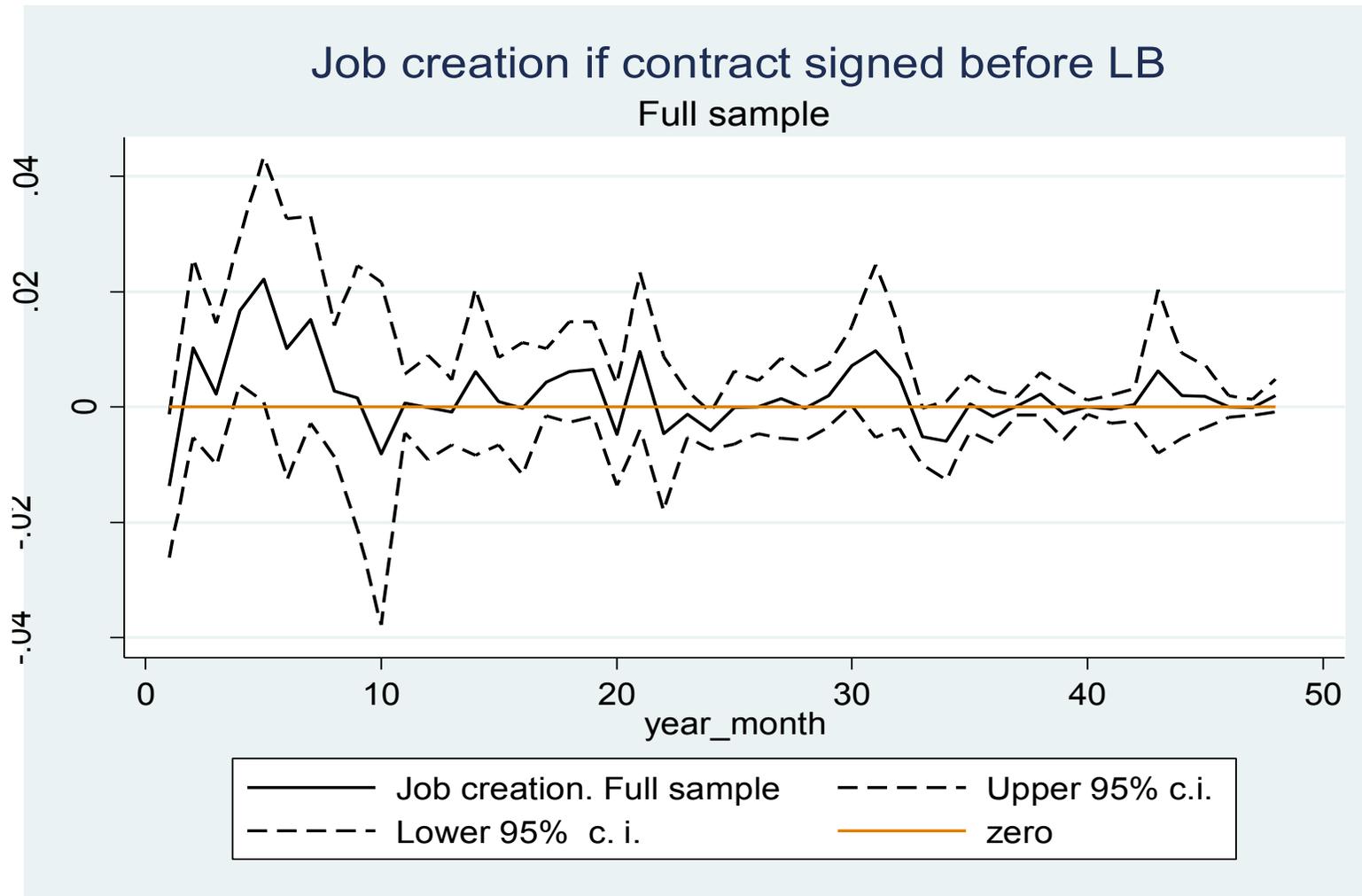


**Figure 3: Job destruction among workers covered by province-industry agreements signed between October 2007 and September 15th 2008**

The graph shows the predicted probability of job loss among collective contracts signed in each quarter, obtained through regressions where

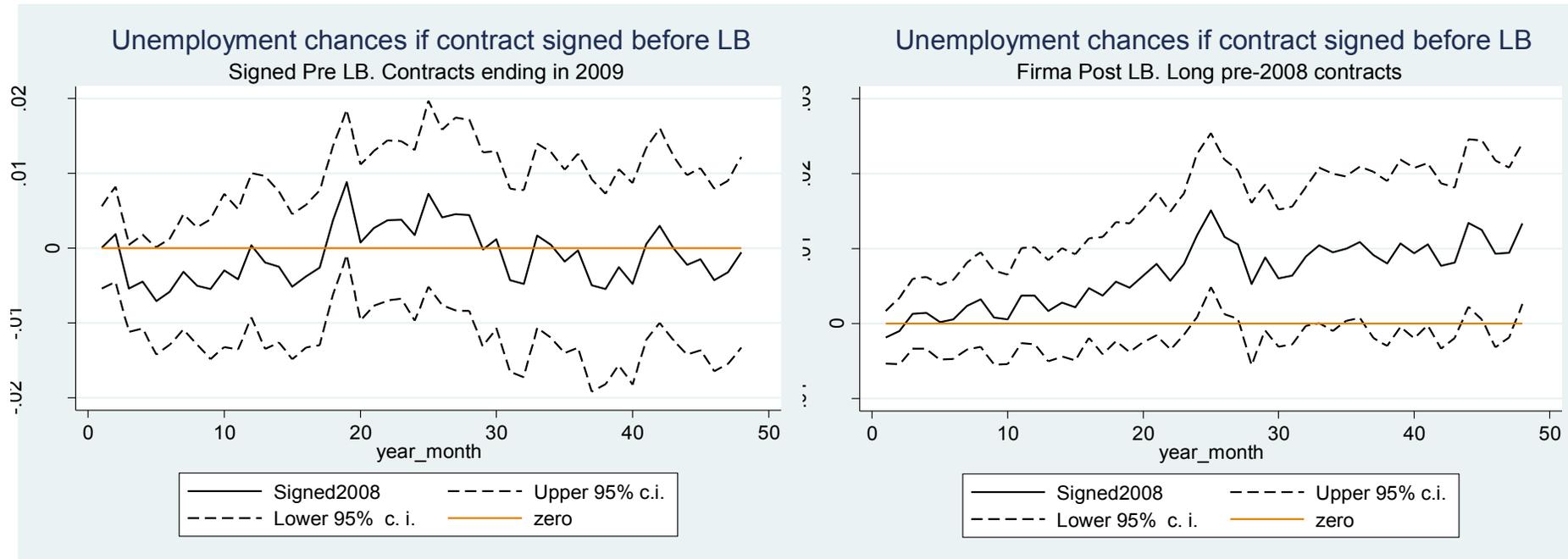
- (a) The dependent variable is the probability of job loss in 2009 (Figure 3A) and 2010 (Fig. 3B)
- (b) Additional covariates are 49 province and 141 industry dummies

Standard errors permit arbitrary correlation at the province-industry cell.



**Figure 4: Job creation in province-industry cells covered by contracts signed between October 2007 and September 15th 2008**

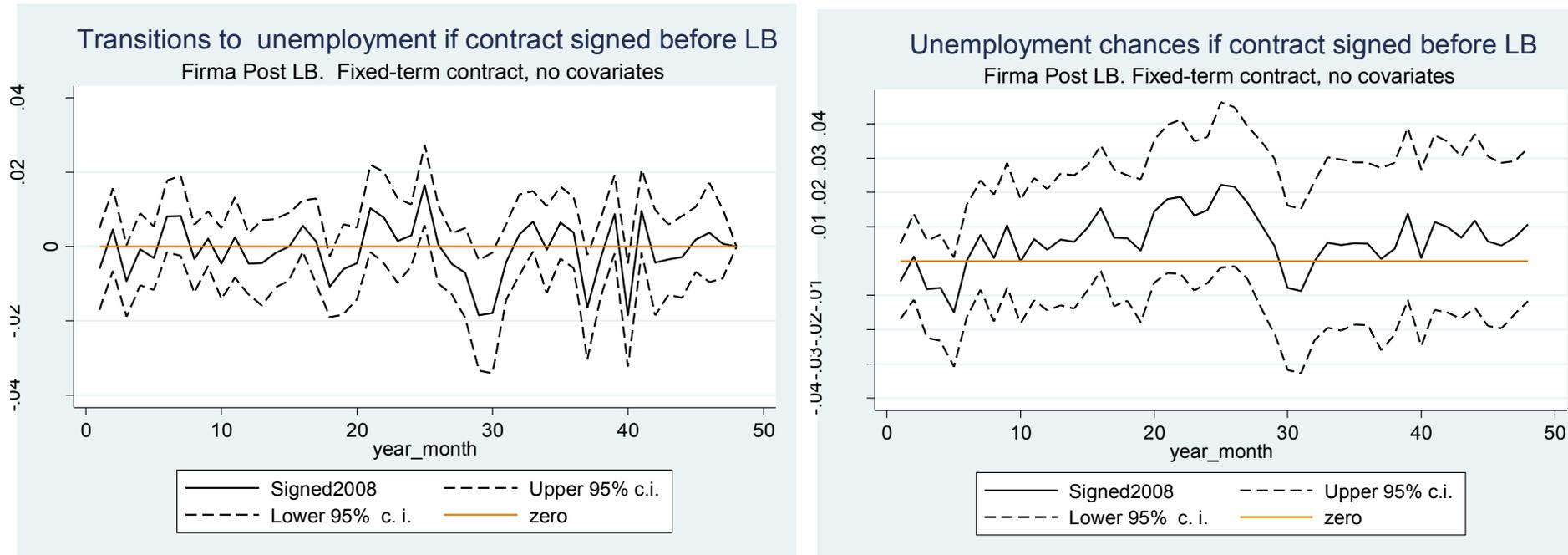
The Figure plots OLS estimates and confidence intervals of the variable "Collective Contract signed before September 15th, 2008" in 48 separate regressions. The dependent variable is the number of new entrants and unemployed as of 2008 who find a job as a fraction of total workers. Sample restricted to contracts signed between the last quarter of 2007 and the first of 2010



**Figure 5: Employment outcomes among workers covered by a province-industry agreements signed between October 2007 and September 15th 2008, by collective contract length**

The Figure in the left plots the OLS estimates and confidence intervals of the variable "Collective Contract signed before September 15th, 2008" in 48 separate regressions with an indicator of non-employment between January 2009 and December 2012. Sample restricted to contracts signed after LB or before, but that expired in 2009

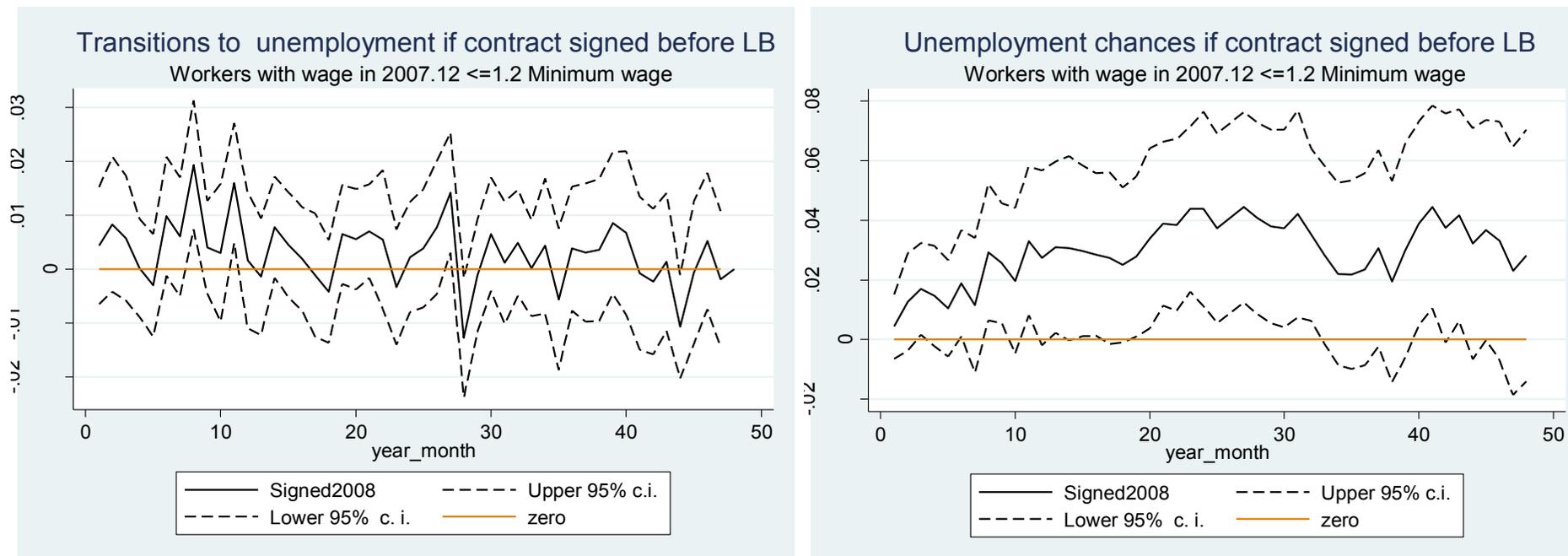
The Figure on the right plots the same regressions but using as a dependent variable the fraction of individuals who were working in the firm as of December 2008 who are not employed in each month. All regressions control for 111 month-specific three-digit industry and 49 month-specific province dummies



**Figure 6: Employment outcomes among workers covered by a province-industry agreements signed between October 2007 and September 15th 2008, by type of individual contract**

The Figure in the left plots the OLS estimates and confidence intervals of the variable "Collective Contract signed before September 15th, 2008" in 48 separate regressions with an indicator of non-employment between January 2009 and December 2012. Sample restricted to workers with a fixed-term contract

The Figure on the right plots the same regressions but using as a dependent variable the fraction of individuals with a Fixed-term contract and who were working in the firm as of December 2008 who are not employed in each month. All regressions control for 111 month-specific three-digit industry and 49 month-specific province dummies

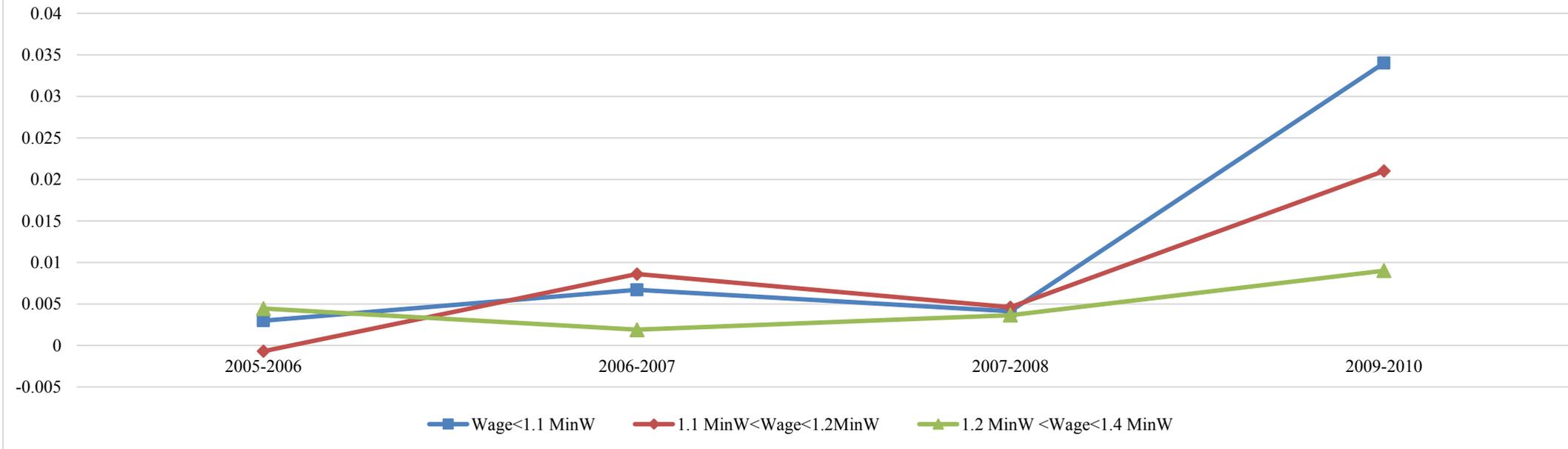


**Figure 7: Employment outcomes among workers covered by a collective contract signed before LB and whose earnings are at most 20% above the collective contract minimum**

The Figure in the left plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before September 15th, 2008" in 48 separate regressions with an indicator of job loss among in each month between January 2009 and December 2010.

The Figure on the right plots the same regressions but using as a dependent variable the fraction of individuals who were working in the firm as of December 2008 who are not employed in each month. All regressions control for 111 month-specific three-digit industry and 49 month-specific province dummies

**Figure 8: Pre-2009 incidence of unemployment in industry-province bargaining units signing before LB fall**



The Figure presents estimates of the interaction between "signed before LB" and the percent difference between the wage of a worker and the collective agreement minimum on the fraction of days in unemployment between  $t+1$  and  $t+2$ . Four regressions are shown: 2005-6, 2006-7, 2007-8 and 2009-10. In each year, we show estimates separately for workers whose wage is below 1.1 the minimum wage, between 1.1 and 1.2 times the minimum wage and between 1.2 and 1.4 times the minimum wage.

The sample in each regression includes workers who stayed in their firm for at least one year and the dependent variable in each regression is the fraction of days not worked during the subsequent two years.

**Table 1: Staggered collective contracts: Year of signature of 2009 agreement, by expiration of previous one**

Last contract expired in (December)	Collective contract signed for 2009 signed in:				Total row
	2007	2008	2009	2010	
2006	<b>80</b> <i>100</i>	16 <i>21</i>	2 <i>2</i>	--	100 <i>(40)</i>
2007	--	<b>83</b> <i>77</i>	14 <i>16</i>	3 <i>9</i>	100 <i>(30)</i>
2008	--	3 <i>2</i>	<b>69</b> <i>81</i>	28 <i>90</i>	100 <i>(30)</i>
Total column	(32) 100	(32) 100	(25) 100	(9) 100	(100) (100)

Numbers in normal font are the number of provincial collective agreements binding in 2009 that are signed in the year of the column as a fraction of the total number of contracts expiring in the year shown in the row

Each cell in italics shows the number of provincial collective agreements binding in 2009 whose previous contract expired in the year of the row as a fraction of the total number of contracts signed in the year shown in the column

All statistics weighted by the number of workers covered by the agreement

**Table 2: Summary statistics, by year of signature**

	Social Security Sample	Covered by provincial collective agreements	Contract signed		Difference	
			2007m9 - 2008m9	After 2008m9	Raw	Net of province- three digit industry
	(1)	(2)	(3)	(4)	(5)	(6)
Agriculture, manufacturing and utilities (%)	17.5	12.6	14.7	11.6		
Construction (%)	10.5	23.0	38.1	10.9		--
Trade, Food & Accomodation (%)	27.6	32.7	30.3	34.8		--
Transportation (%)	6.0	4.6	4.6	4.7		--
Finance and real estate (%)	5.1	0.18	0.3	0.0		--
Services to businesses, health and education (%)	32.2	26.3	11.4	37.9		--
Multi-year (%)	--	88.2	100	75.1		
Escalation clause (%)	--	64.3	82.2	44.2		
Mean settled wage growth 2009 (%)	--	2.37	3.07	1.80	1.24**	0.846**
[median]		2.9	3.5	1.5	2.00	
(Standard deviation)		(1.20)	(.96)	(1.15)		
Age	39.4	37.3	37.3	37.2	-.34	-.11
		(10.7)	(10.7)	(10.4)		
Female	.43	41.3	33.7	47.4	13.7**	0.15*
College, managers (%)	21.1	14.8	11.7	17.2	-5.5**	1.00
White collar workers (%)	35.7	35.0	28.5	40.1	-11.6**	-1.32**
Blue collar workers(%)	43.2	50.1	59.6	42.6	17**	0.12
Fixed-term contract (%)	13.28	14.0	16.7	12.2	4.5**	.84
Workers (province-industry cells)	301536	99,735 (1,305)	43,441	56,094		

The sample in the first column is drawn from employees in Social Security records between 18 and 57 years of age in December 2007.

The sample in the second column contains employees staying in the firm since October 1st 2007 until December 31st 2008 and for whom we identified a provincial collective contract signed between September 2007 and June 2010. All worker characteristics refer to December 2007.

The characteristics of collective agreements are those settled for 2009.

Escalation clause takes value one if the contract stipulates an adjustment for realized inflation if it exceeds a threshold level.

**Table 3: Summary statistics of sample with information about wages, by year of signature**

	Full sample N=48,985	Signed before Sept 15th, 2008 N=16,168	Signed after Sept 15th, 2008 N=32,817
Agriculture, manufacturing and utilities (%)	10.7	10.0	10.4
Construction (%)	30.5	65.8	13.3
Trade, Food & Accomodation (%)	27.4	14.3	33.4
Transportation (%)	5.0	4.0	5.2
Services to businesses, health and education (%)	26.6	3.6	33.2
Mean settled wage growth (%)	2.24 (1.27)	3.44 (.83)	1.65 (1.00)
Age	37.33	37.13	37.4
Female (%)	38	20	47
High skill (%)	17	10.0	20.1
Mid skill (%)	35	21.6	41.5
Low skill (%)	47.7	68.5	37.5
Earnings in December 2007:			
Below min_wage (%)	9.7	11.0	9.0
Between 1 and 1.1 times min_wage agreement (%)	8.4	12.4	6.5
Between 1.1 and 1.2 times min_wage (%)	11.0	18.0	7.7
Between 1.2 and 1.4 times min_wage (%)	17.8	19.9	16.8
Above 1.4 times min_wage (%)	52.8	38.6	60.0

The sample contains employees staying in the firm since October 1st 2007 until December 31st 2008 and for whom we identified a provincial collective contract with information about province x industry x skill minimum wages.

Min\_wage denotes the minimum wage in the agreement for the province x 3-digit industry and skill level of the worker.

All worker characteristics refer to December 2007. The characteristics of collective agreements are those settled for 2009.

**Table 4: OLS impacts of the date of signature of collective contract on wage growth**

<i>Last contract expired in (December)</i>	Agreed wage growth for years:		
	2009	2009 and 2010	2009 - 2011
Collective contract signed before LB	.0092 (.0013)	.0115 (.0017)	.0237 (.0036)
R-squared	.69	.49	.57
Province dummies	yes	yes	yes
3-digit industry dummies	yes	yes	yes
Observations	569	569	404

The unit of observation is the collective agreement.

All observations weighted by the number of workers in the industry-province.

The final column includes all contracts signed after September 15th, 2008 as well as those contracts signed before for the period 2009-2011 (45% of all workers signing before LB)

C:\trabajo\wage\_rigidity\_lb\1 signature\logfiles\First\_stage\_with\_collective\_contracts\_stayers\_zeroes\_coll.log

**Table 5: Regression discontinuity estimates of the impact of collective contract signature date on job losses**

	Full sample	+/- "5 quarters"	+/- 4 quarters	+/-3 quarters	+/- 2 quarters	+/- 1 quarter
Number of contracts	[568]	[506]	[452]	[326]	[240]	[133]
<i>Panel A: Probability that the worker is no longer working at the firm in years 2009-2012</i>						
1. Probability job separation in 2009 (standard error)	.0099 (.0055)*	.0042 (.0044)	.0030 (.0052)	.0010 (.0063)	-.0077 (.0078)	-.0199 (.0120)
2. Probability job separation in 2010	.0089 (.0055)*	.0113 (.0049)**	.0135 (.0052)***	.0116 (.0059)**	.0088 (.011)	.0212 (.0137)
3. Probability job separation in 2011	.0040 (.0061)	.003 (.0069)	.0041 (.0073)	.0030 (.0082)	-.0177 (.01399)	0 (.0106)
4. Probability job separation in 2012	.0029 (.0064)	.0078 (.0078)	.0089 (.0091)	-.0061 (.0083)	-.0061 (.013)	-.0037 (.0111)
<i>Panel B: Probability that the worker is unemployed in the last month of each year 2009-2012</i>						
Probability unemployed in 2009m12	.0017 (.0031)	.0066 (.0034)*	.0059 (.0037)*	.0014 (.0039)	.0070 (.00488)	.0067 (.0057)
Probability unemployed in 2010m12	.0114 (.0048)**	.0120 (.0047)**	.013 (.0055)**	.0117 (.0051)***	.0194 (.0053)***	.0057 (.0063)
Probability unemployed in 2011m12	.0081 (.0040)**	.0081 (.0049)**	.0085 (.0052)*	.0079 (.0057)	.0140 (.0063)***	.0039 (.0079)
Probability unemployed in 2012m12	.0098 (.0042)**	.0078 (.0057)**	.0100 (.0060)*	.0090 (.0062)	.0170 (.0062)***	.0018 (.0095)

Each cell shows the OLS estimate of the variable "Collective contract signed before September 15th, 2008" on the probability of separation (Panel A) or the probability that the worker is unemployed

All regressions control for 3-digit industry dummies and province dummies (variable omitted: Madrid)

**Table 6: RDD estimates of the impact of collective contract signature date on job losses**

	Quarters around September 15th				
	+/- 5	+/- 4	+/-3	+/- 2	+/- 1
<i>Panel A: Job destruction if collective contract signed before LB ended in 2009</i>					
1. Probability job separation in 2009 (standard error)	-.0055 (.0084)	.0103 (.0093)	-.0201 (.0116)	.0020 (.0141)	.0004 (.0260)
2. Probability job separation in 2010	.0087 (.0077)	.007 (.009)	-.003 (.0132)	.0156 (.0147)	.0102 (.0303)
3. Probability job separation in 2011	-.0138 (.0085)*	-.0123 (.0087)	-.0249 (.0117)**	-.0265 (.0146)	-.0322 (.0280)
4. Probability job separation in 2012	-.0131 (.0086)	-.0102 (.0094)	-.0071 (.0128)	-.000 (.0137)	-.0116 (.0317)
<i>Panel B: Job destruction if collective contract signed before LB ended after 2009</i>					
1. Probability job separation in 2009 (standard error)	.0018 (.0054)	.0015 (.0059)	.0022 (.0074)	-.0138 (.009)	-.0228 (.0191)
2. Probability job separation in 2010	.0109 (.0057)*	.0146 (.0058)**	.0184 (.0062)***	.0199 (.0126)	.0261 (.0224)
3. Probability job separation in 2011	.0046 (.0081)	.0047 (.0088)	.0064 (.0104)	-.033 (.0186)*	.0142 (.0113)
4. Probability job separation in 2012	.0048 (.0083)	.0106 (.0086)	.0098 (.0093)	-.0100 (.0108)	.0047 (.0193)
<i>Panel C: Probability of unemployment if collective contract signed before LB ended after 2009</i>					
Unemployed in 2009m12	.0034 (.0037)	.0034 (.0040)	.0049 (.0040)	.0111 (.00465)**	.0027 (.0070)
Unemployed in 2010m12	.0139 (.0049)***	.0140 (.005)***	.0179 (.0057)***	.0270 (.0057)***	.0100 (.0075)
Unemployed in 2011m12	.0084 (.0055)*	.0092 (.0055)*	.0115 (.0062)*	.0186 (.0067)***	-.0067 (.0090)
Unemployed in 2012m12	.0102 (.0059)*	.0108 (.0059)*	.0132 (.0067)*	.0215 (.0069)***	-.0053 (.0113)

Each cell shows the OLS estimate of the variable "Collective contract signed before September 15th, 2008" on the probability of separation (Panel A) or the probability that the worker is unemployed  
All regressions control for 3-digit industry dummies and province dummies (variable omitted: Madrid)

**Table 7: The impact of signature on the probability of job loss and unemployment, fixed-term vs open-ended contracts**

Open ended contracts					Fixed term contracts				
2010									
	Pr(job loss remain in firm)		Pr(unemployment)			Pr(job loss remain in firm)		Pr(unemployment)	
January	-0.0016	(.0017)	0.0036	(.0030)	January	-0.0045	(.0058)	0.0035	(.0096)
February	-0.0001	(.0011)	0.0040	(.0030)	February	-0.0017	(.0047)	0.0029	(.0096)
March	-0.0007	(.0010)	0.0018	(.0029)	March	-0.0001	(.0047)	0.0065	(.0090)
April	0.0001	(.0015)	0.0033	(.0028)	April	0.0055	(.0035)	0.0120	(.0092)
May	0.0005	(.0010)	0.0035	(.0029)	May	0.0015	(.0058)	0.0032	(.0097)
June	<b>0.0028</b>	<b>(.0010)</b>	<b>0.0065</b>	<b>(.0029)</b>	June	-0.0101	(.0041)	0.0036	(.0090)
July	0.0015	(.0011)	<b>0.0086</b>	<b>(.0030)</b>	July	-0.0059	(.0063)	-0.0004	(.0104)
August	0.0014	(.0011)	0.0063	(.0032)	August	-0.0044	(.0049)	0.0112	(.0104)
September	0.0022	(.0013)	<b>0.0074</b>	<b>(.0032)</b>	September	<b>0.0107</b>	<b>(.0060)</b>	0.0147	(.0108)
October	0.0000	(.0019)	<b>0.0068</b>	<b>(.0034)</b>	October	0.0079	(.0064)	0.0151	(.0114)
November	<b>0.0024</b>	<b>(.0013)</b>	<b>0.0093</b>	<b>(.0032)</b>	November	0.0013	(.0059)	0.0100	(.0109)
December	0.0030	(.0031)	<b>0.0111</b>	<b>(.0040)</b>	December	0.0031	(.0042)	0.0117	(.0106)
2011									
	Pr(job loss remain in firm)		Pr(unemployment)			Pr(job loss remain in firm)		Pr(unemployment)	
January	0.0055	(.0017)	<b>0.0140</b>	(.0035)	January	<b>0.0161</b>	<b>(.0055)</b>	0.0188	(.0120)
February	-0.0012	(.0013)	<b>0.0095</b>	(.0035)	February	0.0008	(.0053)	0.0181	(.0116)
March	0.0000	(.0012)	<b>0.0098</b>	(.0034)	March	-0.0046	(.0042)	0.0131	(.0110)
April	-0.0046	(.0037)	0.0063	(.0043)	April	-0.0072	(.0062)	0.0068	(.0122)
<b>May</b>	0.0011	(.0015)	<b>0.0092</b>	(.0035)	<b>May</b>	<b>-0.0187</b>	<b>(.0076)</b>	0.0008	(.0128)
June	-0.0014	(.0011)	<b>0.0087</b>	(.0032)	June	-0.0169	(.0081)	-0.0107	(.0120)
July	0.0021	(.0012)	<b>0.0084</b>	(.0035)	July	-0.0038	(.0052)	-0.0118	(.0119)
August	0.0014	(.0011)	<b>0.0096</b>	(.0035)	August	0.0030	(.0055)	-0.0034	(.0115)
September	0.0009	(.0010)	<b>0.0107</b>	(.0037)	September	0.0067	(.0041)	0.0017	(.0124)
October	0.0006	(.0015)	<b>0.0098</b>	(.0036)	October	-0.0007	(.0059)	0.0009	(.0125)
November	0.0011	(.0013)	<b>0.0086</b>	(.0036)	November	0.0067	(.0051)	0.0012	(.0118)
December	<b>0.0022</b>	(.0011)	<b>0.0097</b>	(.0036)	December	0.0040	(.0049)	0.0015	(.0117)

**Table 7 (cted.): The impact of signature on the probability of job loss and unemployment, fixed-term vs open-ended contracts**

2012									
Pr(job loss remain in firm)		Pr(unemployment)		Pr(job loss remain in firm)		Pr(unemployment)			
January	-0.0035	(.0028)	0.0072	(.0041)	January	-0.0171	(.0071)	-0.0034	(.0132)
February	0.0016	(.0016)	0.0064	(.0037)	February	-0.0030	(.0054)	-0.0005	(.0124)
March	0.0020	(.0014)	<b>0.0087</b>	(.0039)	March	0.0086	(.0055)	0.0095	(.0124)
April	-0.0003	(.0016)	0.0079	(.0040)	April	-0.0190	(.0071)	-0.0035	(.0128)
May	0.0039	(.0013)	<b>0.0093</b>	(.0038)	<b>May</b>	0.0096	(.0057)	0.0073	(.0126)
June	-0.0001	(.0016)	<b>0.0083</b>	(.0041)	June	-0.0045	(.0073)	0.0060	(.0122)
July	-0.0007	(.0012)	<b>0.0080</b>	(.0037)	July	-0.0035	(.0048)	0.0031	(.0117)
August	0.0003	(.0011)	<b>0.0110</b>	(.0041)	August	-0.0030	(.0056)	0.0080	(.0125)
September	0.0018	(.0013)	<b>0.0110</b>	(.0042)	September	0.0026	(.0045)	0.0021	(.0124)
October	0.0009	(.0017)	0.0082	(.0045)	October	0.0034	(.0068)	0.0004	(.0120)
November	0.0003	(.0017)	0.0073	(.0041)	November	0.0009	(.0048)	0.0020	(.0110)
December	--	--	<b>0.0102</b>	(.0042)	December	--	--	0.0062	(.0110)

**Table 8: Diff-in-Diff. estimates of the impact of date of signature on actual wage growth -by distance to the contract-specific minimum wage**

	Wage growth 2008-2009		Wage growth 2008-2010
	(1)	(2)	(3)
1. Contract signed before September 15th 2008 * (min_wage<Earnings 2007.12 <1.10*min_wage)	.0102 (.0042)**	.0134 (.0046)***	.0176 (.0057)***
2. Contract signed before September 15th 2008 * (1.1 min_wage<Earnings 2007.12 <1.20*min_wage)	.0057 (.0041)*	.0087 (.0046)*	.004 (.006)
3. Contract signed before September 15th 2008 * (1.2*min_wage<Earnings 2007.12 <1.40*min_wage)	.0019 (.0028)	.0032 (.0033)	.010 (.0049)**
4. Earnings in 2007.12 between min_wage and 1.1 times minimum wage	.0186 (.0032)	.0189 (.0034)	.0304 (.0043)
5. Earnings in 2007.12 between 1.1 times min_wage and 1.2 times minimum wage	.0149 (.0028)	.0155 (.0030)	.0302 (.0033)
6. Earnings in 2007.12 between 1.2 times min_wage and 1.4 times minimum wage	.0096 (.0019)	.0103 (.0025)	.0193 (.003)
7. Collective contract signed before September 15th 2008	.0008 (.003)	--	--
Constant	.0046 (.0016)	.0083 (.0006)	-.0043 (.0008)
Collective contract fixed-effect	no	yes	yes
Number of observations (contracts):	31675 (558)		25981 (556)
R-squared	.0137	.0281	.0463

The sample used in columns 1 and 2 contains workers who stayed in the same firm all through 2008 and 2009 and who are full time employees in December 2008 and 2009. We use employees whose earnings in those two months were below (above) the censored maximum (minimum). The dependent variable is the difference between the logarithm of monthly earnings in December 2009 and that in December 2008 and the covariates, aside from those shown, are provincial dummies and three-digit industry dummies (in column 1) and collective agreement fixed effects in columns 2 and 3. The model is estimated by OLS, and the standard errors permit arbitrary correlation between observations in the same collective agreement. The sample in column 3 includes job stayers in the firm between 2008 and 2010.

**Table 9: Diff-in-Diff estimates of employment losses by date of signature and distance to contract-specific minimum wage**

Dependent variable: Set of covariates:	1(90+ days in U 2009-2010)			Days in U 2009-2010/730		
	Basic (1)	Basic (2)	Extended (3)	Basic (4)	Basic (5)	Extended (6)
1. Contract signed before September 15th 2008 * (min_wage<Earnings 2007.12 <1.10*min_wage)	.057 (.021)**	.053 (.022)**	.053 (.022)**	.034 (.010)***	.0334 (.0118)**	.0329 (.0118)**
2. Contract signed before September 15th 2008 * (1.1 min_wage<Earnings 2007.12 <1.20*min_wage)	.057 (.018)**	.0469 (.019)**	.0475 (.029)**	.024 (.010)***	.021 (.012)*	.0212 (.012)*
3. Contract signed before September 15th 2008 * (1.2*min_wage<Earnings 2007.12 <1.40*min_wage)	.013 (.012)	.0116 (.0112)	.0121 (.0128)	.010 (.007)	.009 (.0075)	.0093 (.0075)
Earnings in 2007.12 between min_wage and 1.1 times minimum wage	.075 (.011)	.0837 (.012)	.077 (.011)	.038 (.0076)	.041 (.0076)	.0396 (.0076)
Earnings in 2007.12 between 1.1 times min_wage and 1.2 times minimum wage	.066 (.013)	.074 (.012)	.067 (.013)	.035 (.0084)	.040 (.0084)	.0372 (.0083)
Earnings in 2007.12 between 1.2 times min_wage and 1.4 times minimum wage	.0489 (.007)	.052 (.007)	.047 (.007)	.023 (.0041)	.025 (.0041)	.0235 (.0041)
Contract signed before September 15th, 2008	.0216 (.010)	--	--	.009 (.006)	--	--
Constant	.159 (.008)	.15 (.006)	.176 (.0147)	.084 (.004)	.082 (.003)	.089 (.008)
Collective agreement fixed effect	no	yes	yes	no	yes	yes
Number of observations (collective contracts):				44017 (561)		
R-squared	.093	.095	.097	.0819	.0872	.0881

(a) Coefficients of OLS regressions of the dependent variable in the second row on dummies indicating the distance between the distance between the monthly wage of the worker in December 2007 (2007.12) and the minimum statutory wage in the province - industry - skill group. The omitted group in all specifications are workers whose earnings in December 2007 was at least 1.4 times the statutory minimum wage.

(b) The sample contains all workers in the Social Security records who were employed in the same firm since October 1st 2007 until December 2008

(c) The coefficient of interest is the interaction of the distance to the statutory wage and the date of signature of the contract. All models but (1) and (3) include 561 collective contract fixed effects. The basic list of covariates include 9 dummies with the skill group of the worker and a dummy for a fixed-term contract. The extended set of covariates adds 5 age dummies, and an indicator of female worker. Standard errors -in parentheses- clustered by province-industry

**Table 10: The probability of not working in 2009-2010 , sample splits**

<i>Panel A: Differential effect by the duration of contracts -if signed before 2008m9</i>				
Dependent variable:	Contracts signed in 2008 expiring in 2009		Contracts signed in 2008 expiring after 2010	
	Actual wage growth	% Days in U 2009-2010	Wage growth	% Days in U in 2009-2010
	(1)	(2)	(3)	(4)
Signed before 2008m9 *	.0079	.009	.011	.0290
(min_wage <Earnings 2007.12 < 1.20min_wage)	(.0066)	(.0114)	(.0042)***	(.0110)**
Signed before 2008m9 *	-.0058	.001	.0055	.011
(1.2 min_wage <Earnings 2007.12 < 1.40min_wage)	(.0057)	(.0095)	(.0036)	(.0085)
Min_wage<Earnings 2007.12 <1.10 min_wage	.0169	.0428	.017	.041
	(.0024)	(.007)	(.0024)	(.007)
(1.20 Min_wage<Earnings 2007.12 <1.40 min_wage)	.103	.0272	.103	.0255
	(.0024)	(.0042)	(.002)	(.0042)
Constant	.0105	.0711	.0076	.082
	(.00068)	(.002)	(.0007)	(.003)
Number of observations (collective contracts):		32264 (379)	29704 (534)	41479 (512)
<i>Panel B: Differential effects by severance payments in individual contract</i>				
Dependent variable:	Fixed term contract		Open ended contract	
	Actual wage growth	% Days in U 2009-2010	Wage growth	% Days in U in 2009-2010
	(1)	(2)	(3)	(4)
Signed before 2008m9 *	.0026	.021	.012	.0257
(min_wage <Earnings 2007.12 < 1.20min_wage)	(.014)	(.024)	(.0041)***	(.009)**
Signed before 2008m9 *	.0043	.024	.0036	.008
(1.2 min_wage <Earnings 2007.12 < 1.40min_wage)	(.044)	(.020)	(.0038)	(.008)
Min_wage<Earnings 2007.12 <1.10 min_wage	.0160	.0280	.017	.036
	(.011)	(.0156)	(.0024)	(.0065)
(1.20 Min_wage<Earnings 2007.12 <1.40 min_wage)	.0054	-.010	.110	.027
	(.011)	(.0114)	(.002)	(.0042)
Constant	.015 (.0029)	.188 (.018)	.0076 (.0007)	.0766 (.077)
Observations:	4136 (335)	6018 (324)	27539 (547)	37999 (555)

**Table 11: The response of days not worked, by industry 2009-2010**

Dependent variable:	Construction		Manufacturing		Food and Accomodation (1)		Business services (2)	
	U >=90 days	% Days in U	U >=90 days	% Days	U >=90 days	% Days	U >=90 days	% Days
	(1)	(2)		(2)	(3)	(4)	(5)	(6)
Signed before 2008q4 * (20pp above minimum)	.010 (.022)	.001 (.010)	-.096 (.055)	-.026 (.0489)	<b>.046</b> <b>(.028)*</b>	.015 (.017)	<b>.074</b> <b>(.043)*</b>	<b>.0444</b> <b>(.0196)**</b>
Signed before 2008q4 * (20pp <wage< 40pp)	-.016 (.022)	.001 (.014)	.024 (.030)	-.012 (.019)	-.006 (.018)	-.008 (.010)	.028 (.044)	.021 (.023)
Wage in 2008 20 pp above minimum	.10 (.018)	.060 (.007)	.084 (.037)	.055 (.023)	.070 (.013)	.031 (.006)	.061 (.011)	.028 (.007)
Wage in 2008 20-40pp above minimum	.068 (.0188)	.028 (.013)	.059 (.016)	.038 (.011)	.055 (.011)	.029 (.006)	.047 (.012)	.018 (.006)
Constant	.19 (.018)	.11 (.008)	.17 (.018)	.088 (.012)	.133 (.007)	.067 (.003)	.054 (.011)	.070 (.004)
Number of observations:	13406 (84)		4693 (235)		15481 (178)		11899 (65)	
R-squared	.123	.08	.134	.133	.042	.041	.073	.0294

Covariates: 5 age dummies, type of labor contract and an indicator of female worker, collective contract fixed effects and indicators of skill level

(1) Retail, Wholesale, Food and Accomodation and Transportation (2) Personal and business services, health, education

(a) Industry-specific coefficients of OLS regressions of the dependent variable in the second row on dummies indicating the distance between the distance between the monthly wage of the worker in December 2007 (2007.12) and the minimum statutory wage in the province - industry - skill group. The omitted group in all specifications are workers whose earnings in December 2007 was at least 1.4 times the statutory minimum wage.

(b) The sample contains all workers in the Social Security records who were employed in the same firm since October 1st 2007 until December 2008

(c) The coefficient of interest is the interaction of the distance to the statutory wage and the date of signature of the contract. All models but (1) and (3) include 561 collective contract fixed effects. The basic list of covariates include 9 dummies with the skill group of the worker and a dummy for a fixed-term labor contract. The extended set of covariates adds 5 age dummies, and an indicator of female worker. Standard errors -in parentheses- account for heteroscedasticity and arbitrary correlation across observations within the same 3-digit industry x province cell.

TABLA DE RETRIBUCIONES BRUTAS DEL CONVENIO DE LA CONSTRUCCIÓN Y OBRAS PÚBLICAS DE NAVARRA

*Revisión salarial. Efectos: 1-1-2010 a 31-12-2010. Incremento: 4,5%*

NIVELES	SALARIO BASE	COMPL. CONVENIO	PLUS EXTRASAL.	TOTAL MES	JULIO	NAVIDAD	VACACIONES	TOTAL ANUAL
II Titulado Superior	2.102,35	555,97	65,54	2.723,86	3.467,27	3.467,27	3.467,27	40.364,27
III Titulado Medio	1.725,24	467,14	65,54	2.257,92	2.873,88	2.873,88	2.873,88	33.458,76
IV Encargado General	1.670,51	453,53	65,54	2.189,58	2.795,09	2.795,09	2.795,09	32.470,65
V Jefe Admon. 2. <sup>a</sup>	1.498,57	410,43	65,54	1.974,54	2.519,81	2.519,81	2.519,81	29.279,37
VI Delineante 1. <sup>a</sup>	1.285,55	357,97	65,54	1.709,06	2.179,21	2.179,21	2.179,21	25.337,29
VII Capataz	1.262,40	355,72	65,54	1.683,66	2.144,91	2.144,91	2.144,91	24.954,99
VIII Oficial 1. <sup>a</sup>	1.234,36	358,33	65,54	1.658,23	2.110,66	2.110,66	2.110,66	24.572,51
IX Oficial 2. <sup>a</sup>	1.085,89	326,52	65,54	1.477,95	1.878,53	1.878,53	1.878,53	21.893,04
X Especialista	1.022,26	315,92	65,54	1.403,72	1.782,02	1.782,02	1.782,02	20.786,98
XI Peón Especializado	1.004,83	317,78	65,54	1.388,15	1.760,69	1.760,69	1.760,69	20.551,72
XII Peón Ordinario	969,67	313,00	65,54	1.348,21	1.708,38	1.708,38	1.708,38	19.955,45
XIII Aspirante Admon.	689,31	222,80	65,54	977,65	1.239,23	1.239,23	1.239,23	14.471,84
XIV Aprendices:								
De 16 y 17 años: 1. <sup>er</sup> año	597,24	179,58	65,54	842,36	1.033,19	1.033,19	1.033,19	12.365,53
De 16 y 17 años: 2. <sup>o</sup> año	651,53	195,91	65,54	912,98	1.127,12	1.127,12	1.127,12	13.424,14
De 18 a 21 años: 1. <sup>er</sup> año	705,83	212,24	65,54	983,61	1.221,05	1.221,05	1.221,05	14.482,86
De 18 a 21 años: 2. <sup>o</sup> año	760,12	228,56	65,54	1.054,22	1.314,97	1.314,97	1.314,97	15.541,33

**Table A.1 An example of the wage minima system of collective agreement (construction in Navarre, 2010)**

**Table A.2. Key summary statistics and wage distribution relative to the minimum, by industry and skill**

	Industry				Skill level		
	Manufacturing	Construction	Trade, F&A	Other services	High	Medium	Low
1. Age	38.5	37.2	35.92	37.86	37.2	36.3	38.11
2. Female	.218	.12	.57	.61	.40	.646	.2
3. High skill	.10	.13	.11	.33	--	--	--
4. Low skill	.72	.70	.29	.43			
5. Earnings in December 2007:							
Fraction workers below min_wage	.042	.09	.13	.08	.046	.11	.10
Fraction between 1 and 1.1 times min_wage	.024	.16	.08	.04	.023	.07	.11
Fraction between 1.1 and 1.2 times min_wage	.047	.21	.09	.05	.03	.09	.15
Fraction between 1.2 and 1.4 times min_wage	.25	.18	.20	.12	.07	.16	.22
Above 1.4 times min_wage	.64	.35	.49	.70	.83	.56	.40
Sample size	4396	14934	13218	13011	8451	17145	23389

All statistics in the Table are sample averages of the matched sample of Social Security records and collective contracts with information about wage levels.

**Table A.3 Wage distribution, relative to the minimum in the collective contract**

	2007	2010	2012
Wage below minimum in agreement	.054	.04	.036
Wage between minimum and 1.1 times minimum	.078	.06	.058
Wage between 1.1 and and 1.2 times the minimum	.106	.091	.087
Wage between 1.2 and and 1.4 times the minimum	.213	.226	.206
Wage above 1.4 times the minimum	.549	.592	.612
Sample size	32526	32536	32536

Sample of job stayers. Estimates differ from those in Table A.2. because the sample contains job stayers between waves only