

Staggered Contracts and Unemployment during Recessions*

Effrosyni Adamopoulou[†] Luis Díez-Catalán[‡] Ernesto Villanueva[§]

February 8, 2023

Abstract

This paper studies the impact of downward wage rigidity on wage dynamics and employment flows after the outbreak of major recessions over the last 30 years in Spain. Downward wage rigidity stems from collective agreements, which set province-industry-skill specific minimum wage floors for all workers. We show that agreements signed before the onset of the 1993 and 2009 recessions settled on average for a 1.0-1.5 pp higher nominal wage growth than the agreements signed after. By exploiting variation in the renewal of collective contracts and leveraging Social Security data and the distribution of the worker-level bite of minimum wage floors, we find that in both recessions actual wage growth was indeed higher among workers covered by collective contracts signed during expansions and with wages close to the floors. However, employment responses vary across recessions. In the low-inflation recession of 2009, job losses are highly persistent and entirely driven by workers with pre-recession wages close to the minimum wage floors while in the high inflation recession of 1993, job losses were limited and short-lived. Using Labour Force Survey data in a similar setting we find that downward wage rigidity during the first year of the COVID-19 pandemic triggered adjustments at the intensive margin of labor (short time work). Our findings highlight the interplay between rigidity at different parts of the wage distribution, macroeconomic environment and labor market institutions and identify conditions under which collective contract staggering and the inability to renegotiate may amplify aggregate shocks.

JEL Codes: J23, J31, J50

Keywords: Labor Demand, Collective Bargaining, Wage Rigidity, Staggering, Minimum Wages, Social Security Data, Inflation

*Previously circulated as "Contract Staggering and Unemployment during the Great Recession: Evidence from Spain". We thank Ainhoa Aparicio Fenoll, Andreas Gulyás, Cristina Bellés Obrero, Samuel Bentolila, Pierre Cahuc, Antonio Ciccone, Christian Dustmann, Mike Elsby, Dan Hamermesh, Simon Heiler, Boris Hirsch, Marcel Jansen, Juan Francisco Jimeno, Keith Kuester, Fernando Martins, Christian Moser, Silvana Tenreyro, Harald Uhlig, Andrea Weber and seminar participants at the University of Mannheim, the Banco de España, the Collegio Carlo Alberto, the European Winter Meeting of the Econometric Society in Berlin, the IAB and the NBER Summer Institute for useful comments and suggestions. We also thank Cristina Guillamón and Sergio Puente for help with the social security data, Mario Izquierdo for help with the information on collective contracts and Cristina Somcutean for excellent research assistance. Adamopoulou gratefully acknowledges financial support by the German Research Foundation (through the CRC-TR-224 project A3 and a Gottfried Wilhelm Leibniz-Prize). All errors and opinions are our own.

[†]University of Mannheim and IZA. E-mail: adamopoulou@uni-mannheim.de

[‡]BBVA Research. E-mail: luismiguel.diez@bbva.com

[§]Banco de España. E-mail: ernesto.villanueva@bde.es

1 Introduction

The role of wage rigidity in generating employment fluctuations has been subject to considerable debate both in academic and policy circles, especially during downturns. Several macroeconomic studies stress that binding collective contracts signed in different moments of the business cycle are a source of wage inflexibility that explains aggregate fluctuations in output and account for international differences in the transmission of monetary policy –see Taylor (1980) and, more recently, Broer et al. (2022), Gertler and Trigari (2009), Gertler et al. (2020) and Olivei and Tenreyro (2007, 2010). However, basic evidence on how collective contracts shape the wage distribution is still scarce and there is no consensus on the degree of wage cyclicality and its impact on the allocation of labor–see Pischke (2018).¹ In the policy arena, 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 induced by binding collective contracts –see OECD (2013 and 2019).

In this paper we investigate empirically how downward nominal wage rigidity affected labor market adjustments after the onset of three major recessions over the last 30 years in Spain. The source of wage rigidity stems from collective agreements bargained at the province-industry level that automatically apply to all firms and their workforce in the bargaining unit, i.e., that are automatically extended. Those contracts specify minimum wage floors for various skill groups of workers and are renegotiated infrequently.²

Our empirical strategy builds on a basic insight from the macroeconomic literature (e.g., Card 1990; Olivei and Tenreyro, 2007 and 2010): 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 after the onset of the shock. Thus, whenever an aggregate shock hits, workers and employers covered by contracts already signed cannot readily adjust to the unfavorable macroeconomic conditions and are subject to wage rigidity, presumably leading firms to lay-off workers or make use of short-time work schemes. On the contrary, firms and workers covered by contracts signed after the deterioration of the macroeconomic conditions can potentially adjust to a recessionary period by settling on lower nominal wage growth, thus forming a control group. In sum, the automatic extension of sector-province-level contracts, together with the difficulty of opting out from existing ones, generates substantial cross-sectional variation in the degree of nominal wage rigidity in a given year. That cross-sectional variation in wage growth provides a unique way to estimate the role of downward nominal wage rigidity on employment destruction after a large fall in aggregate demand.

We identify three arguably unanticipated aggregate shocks that resulted in a large fall in employment: the onset of the 1993 recession in the first quarter of 1993, the fall of Lehman Brothers (LB) in the third quarter of 2008, and the outbreak of the COVID-19 pandemic in the first quarter of 2020. As Figure A1 in Appendix A shows, the unemployment rate rose sharply in 1993, 2009-2012 and to a lower extent in 2020. The three recessions had different

¹Adamopoulou and Villanueva (2022a), Bhuller et al. (2022), Cardoso and Portugal (2005), Card and Cardoso (2022), Dolado et al. (1997) and Gautier et al. (2022) are few exceptions.

²Collective contract length varies but usually exceeds two years during expansions.

characteristics. The first recession was less severe than the other two and it took place in a period of high inflation that led to real wage drops despite wage indexation practices. The second and the initial stage of the third recession occurred instead during years of very low inflation (Figure A2 in Appendix A). Furthermore, the third recession was accompanied by lockdown measures and a sharp increase in the number of workers in short-time work schemes (Figure A3 in Appendix A), also due to the policy response.

We use a register that contains all collective agreements signed in Spain. The dataset contains information about the date of signature and expiration of the collective contracts and the nominal wage growth that they settle. Therefore, we can infer the macroeconomic conditions that employers and unions could possibly incorporate during negotiations. We then match the information on all *sectoral* collective contracts with longitudinal data from a four percent random sample of Spanish Social Security records. The resulting matched sample allows us to estimate the effect of downward nominal wage rigidity on wages and employment outcomes, using variation from more than 500 collective contracts in each recession.

Our analysis uncovers four main findings. First, contracts signed before the onset of the 1993 recession, before the fall of Lehman Brothers in 2008, and before the outbreak of the COVID-19 pandemic in 2020, settled for a nominal negotiated wage growth between 0.2 and 1.5 pp higher than contracts signed afterwards. An analysis with a representative subsample of workers with available information about their corresponding minimum wage floors shows that during both the 1993 and the 2009 recessions wage rigidity is concentrated among workers whose pre-recession wage levels were close (i.e., up to 20% above) to the minimum wage floors and were covered by "rigid" contracts.³

Second, we find that nominal wage rigidity has severe employment consequences only in recessionary periods of low inflation. Job losses in the high-inflation recession (1993) are homogeneous across workers due to wage indexation but are rather limited and short-lived possibly because real wages effectively declined. This is true not only if we compare the 1993 and the 2009 recessions (which may differ also in other dimensions beyond inflation) but also within recessions. By exploiting province level heterogeneity in inflation rates during the 1993 recession we find employment losses only in provinces with inflation rates below the national one. Our results are consistent with the hypothesis of "inflation greasing the wheels of the labor market" (See Tobin, 1972; Card and Hyslop, 1997; Fehr and Goette, 2005; and, more recently, Blanco et al. 2022). In the low-inflation recession (2009) instead, job losses are entirely driven by workers with wages close to the minimum wage floors and are highly persistent.⁴ According to our estimates, workers whose collective agreements were not able to adjust remained non-employed even four years after the fall of Lehman Brothers. This is in line with Yagan (2019) who documents considerable employment hysteresis from the Great Recession in the US. In terms of magnitudes, our estimates imply an elasticity of employment status to negotiated wages of up to 0.6 in September 1993 (and 0 afterwards) and of 1.0 by the end of 2010 (and till the end of 2012).

³Wage floors by skill level are available for around 50% of the workers that we analyze. See [Section 4.3](#) for details.

⁴For workers further away from the minima, wage rigidity was less consequential in terms of employment outcomes as there existed margins of wage adjustment.

Third, we identify channels that exacerbate job losses associated to wage rigidity. Similarly to Sorkin (2015), who shows theoretically that an increase in the real value of minimum wages has a large long run impact on employment only when it is perceived as permanent, we find that job destruction in 2009-2012 is mainly driven by collective contracts, whose duration extended wage rigidity at least two years into the recession. Instead, employment protection legislation (EPL) does not seem to play an important role as the estimated employment outcomes of workers with fixed- and open-ended contracts are similar in the long-run.

Fourth, we show that in the presence of specific policies, like short-time work schemes during the COVID-19 pandemic, wage rigidity can have implications on the intensive rather than the extensive margin of labor. Moreover, we find that escalation clauses in collective contracts –a common practice in Spain– become discernible only if coupled with high inflation.⁵ Indeed, during the high-inflation recession of 1993, the excess wage increase in contracts that were signed pre-recession and indexed wage *growth* to past inflation was reflected homogeneously on workers' wages, i.e., it did not vary much with the distance between workers' wages and their corresponding minimum wage floors.

Overall, our results are consistent with the hypothesis that the degree of nominal wage rigidity generated by the automatic extension of provincial agreements and contract staggering can amplify employment destruction during recessionary periods of low inflation. We conduct several robustness checks to control for other factors that may correlate with the date of signature and wage growth and employment outcomes. Firstly, we use contract-level fixed effects that absorb any factor that affects all workers covered by the same collective contract. These models identify the differential wage growth and employment outcomes of workers with pre-recession earnings close to the minimum wage floors relative to workers whose pre-recession earnings were further away from the floors. By comparing those differential outcomes across contracts signed before and after the onset of the recessions we can infer the impact of wage rigidity on employment outcomes. Secondly, we verify that the parallel trends assumption is satisfied in our setting, which is crucial for our identification strategy. Thirdly, we examine the particular case of construction, an industry where all province-level agreements have set the same (nationwide) 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 lead to differential employment losses. Overall, our findings suggest that in the 2009 low-inflation recession, contract staggering contributed to propagating the aggregate shock by increasing flows into non-employment. Finally, we address any possible concerns about the endogeneity of the signature date by using the expiration date as an alternative identification strategy. In this way, we are able to take into account also a small fraction of collective contracts that had expired but did not get renewed to guarantee the exogeneity of the "treatment".

Our study adds 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) and Gertler et al. (2020) for

⁵Escalation clauses are a form of wage indexation and entail ex-post inflation adjustments of negotiated wage growth in case of high inflation

the US, Elsby et al. (2016) for the UK, Stüber (2017) for Germany, Martins et al. (2012) for Portugal or De la Roca (2014) for Spain. Those papers test whether the cyclical nature of wages differs between new matches and incumbents and adopt various strategies to control for the cyclical adjustment of match quality. Other studies infer wage rigidity from the distribution of longitudinal wage changes in survey or administrative data –see Altonji and Devereux (2000) or Dickens et al. (2007). Our results complement those studies by establishing the impact of wage rigidity on employment outcomes. Among others, Devicienti 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 main results on wage cyclical nature rely on the bite of wage floors, that are free of the composition biases that are often discussed in this literature. Also, we can identify a precise source of rigidity, i.e., the proximity to the wage floors, and identify the role of wage cushions in different settings. 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 only. Furthermore, 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 the absence of nominal wage cuts, 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, 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.

We contribute to the literature that studies the role of *explicit* forms of wage rigidity in shaping employment outcomes –see, among others, Gertler and Trigari, 2009 and 2020). 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. More recently, Martins (2021) 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 within the scope of the agreement. Guimaraes et al. (2017), also using Portuguese data, compute the firm-specific increase in payroll following the extension of collective contracts and show that it leads to a decrease in the number of employees. Using a setup similar to ours, Fanfani (2020) exploits the staggered nature of collective bargaining in Italy and estimates large disemployment effects. A study for the Netherlands (Caloia et al., 2021) finds instead very small wage and employment responses, possibly due to the limited pass-through of minimum wage floors in this country. Finally, Björklund et al. (2019) and Faia and Pezone (2020) show that wage rigidity amplifies the effects of monetary policy on firms’ outcomes in Sweden and Italy respectively. We complement those studies in several dimensions. We are able to quantify the persistence of individual-level job losses, a key determinant of the impact of job losses on aggregate demand. This is possible because our data allow us to track workers over time and provide estimates of the likelihood of affected workers eventually finding a job⁶. Thus, by identifying labor market dynamics

⁶Changes in firm’s employment levels include job-to-job changes that leave aggregate employment constant.

during recessions our results cast light on how job losses associated to wage rigidity can lead to an accumulation of human capital losses (Jacobson et al., 1993). Moreover, the relatively large number of collective contracts covering a large share of workers allows us to uncover the role of contract length (and of the resulting wage rigidity) –by comparing contracts with different duration, and the role of adjustment costs –by examining the labor market histories of workers with different degrees of employment protection. In addition, the information on the corresponding minima for each worker and their cushion, i.e., the distance of their actual wage from their minimum wage floor, permits us to analyze workers, who are a priori subject to different degrees of wage rigidity as well as the role of institutional factors such as escalation clauses. Lastly, by analyzing in a unified framework both low- and high- inflation recessions we uncover that the labor market effects crucially depend on the macroeconomic environment. All in all, differences in the contract length, cushions and inflation rates can explain the wide range of estimated elasticities documented in the literature (e.g., Card and Cardoso, 2022; Fanfani, 2020; Martins, 2021).

2 Institutional Background

Similarly to other European countries (e.g., France, Italy, Portugal, Belgium and Nordic countries), collective bargaining is a key feature of the Spanish labor market (see Visser, 2013). Moreover, in Spain there is a nationwide minimum wage, which all collectively negotiated wage floors must respect. Below we describe the main characteristics of collective contracts and other institutional features of the Spanish labor market that may interact with wage rigidity.

2.1 Collective Contracts

2.1.1 Extensions

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 within an industry. In other words, upon publication in the Official State Gazette (*Boletín Oficial del Estado*), the terms and conditions in a sectoral contract become binding for all employers within the scope of the agreement regardless of each worker’s unionization status. 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 in Spain take place at various geographical levels. There are sectoral agreements covering employees in the whole country, while the most disaggregated

geographical level is the municipality. However, the most common geographical level of sectoral bargaining is the province.⁷ 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.

2.1.2 Content and duration

Sectoral collective contracts establish minimum wage floors 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. Table B.1 in Appendix B provides an example of minimum wage floors 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. Note that the monthly wage floor for the lowest skill group is 977 euros (fourth column, thirteenth row), well above the statutory minimum wage for that year (633 euros).

Collective contracts establish not only minimum 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.

Collective contracts set minimum wages and working conditions for a pre-specified period. Collective contract duration varies over the business cycle, but 60% of contracts signed in the period around the 1993 recession and 88% of contracts signed in the period around the fall of Lehman Brothers had a validity period that exceeded one year (see Tables 1a and 1b). Average duration was shorter in 1993 (a period of high inflation) than in the 2009 recession. During the latter, the average contract duration was above three years. 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 an 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.

2.1.3 Escalation, opting out clauses and firm-level contracts

More than 60% of collective contacts signed in the period surrounding the 1993 and 2009 recessions contain escalation clauses that may increase negotiated wage growth further following high inflation episodes. Escalation clauses were particularly relevant during the 1993 recession when inflation rates were above 4%. Figure B1 in Appendix B shows an example of an escalation clause in the 1993 collective contract of the meat industry. First, it establishes a minimum guaranteed wage growth for all workers for 1992 (1993) equal to the realized inflation by the

⁷There are 52 provinces in Spain, the average size of which is about one million inhabitants.

end of 1991 (1992) minus 1 pp. Second, it states that wage floors for 1992 are revised (ex post) to account for the deviation between the inflation that was expected for 1992 –when the collective contract was signed– and the realized inflation. Third, it states that a similar revision of the 1993 wage floors may take place in the future in case there is a deviation between the expected and realized inflation. Therefore, escalation clauses during the 1993 recession may have extended wage rigidity to workers well above the minimum wage floors. The presence of escalation clauses was less consequential during the 2009 recession as inflation rates reached very low levels.

Regarding opting out, the Worker’s Act mentions some conditions that permit a 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*). There is no 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.⁸

We focus on sectoral 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. This is because 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). Thirdly, two labor reforms in 2010 and 2012 have tried to weaken the automatic extension of sectoral agreements on the presumption that this contracting level prevents aggregate wage adjustments.

2.2 Other Institutions

The Spanish labor market is characterized by duality (Bentolila and Dolado, 1994). According to Eurostat, more than 27% of workers were temporary in 2008, for whom no firing costs applied (Barceló and Villanueva, 2016). There are also short-time work schemes (Boeri and Bruecker, 2011) that firms can resort to during periods of hardship. These schemes allow workers to maintain their job by working fewer hours and with the State covering 50-70% of their regular wage. While present, they were barely used before 2020. During the COVID-19 pandemic, access became easier and their generosity increased (Konle-Seidl, 2020). Temporary contracts and short-time work schemes are thus likely to influence the response of the extensive and intensive margin of labor to wage rigidity.

⁸On 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 agreement are covered by firm-level agreements (according to the union’s reports). Previous research has documented that firm-level contracting is most common among large firms and generally sets wages above the minima in sectoral collective contracts (Card and De la Rica 2006).

3 Conceptual framework

Under collective bargaining, employer federations and unions bargain over sectoral contracts that fix a set of minimum wage floors. Those wage floors are mandatory within an industry and/or province. As collective contracts are renegotiated infrequently (every 1.5 to 3.5 years on average, depending on the recession), they cannot readily adjust to changes in the business cycle. Various models in the literature analyze the consequences of this lack of renegotiation.

A first set of models predicts that negotiated wages are the outcome of Nash bargaining between firms and workers and that the resulting wage depends on the state of the economy, which determines in turn firms' and workers' outside options. The infrequent renegotiation and staggered nature of these collective contracts implies that at each point in time wages negotiated under different economic conditions coexist. Following an aggregate unexpected perturbation, all firms experience a fall in the joint surplus. However, in those industries/provinces whose minimum wage floors are already settled, there is a set of low-productivity matches that are no longer viable at the prevailing wage. On the contrary, firms that are renegotiating their contracts at the time of the perturbation can adjust wages and mitigate employment losses (see Gertler and Trigari, 2008; Olivei and Tenreyro, 2009; Faia and Pezone, 2020). The lengthier the validity period of collective contracts already signed, the larger the number of matches that are likely to be affected by the lack of renegotiation.

The transmission mechanism that goes from the lack of renegotiation of wage floors into higher wage growth and job losses depends on many factors, notably firm-specific wage setting policies, inflation and labor market policies. Regarding firm wage-setting policies, the above models implicitly assume that collective contracts fix the wages of *all* workers in an industry. The plausibility of that assumption depends on the particular context. For example, Card and Cardoso (2022) discuss that in the US system of labor relations, unions fix a wage for each position. Nevertheless, in other contexts, unions and employers fix *minimum* wages, so the lack of renegotiation of wage floors does not necessarily translate into pervasive wage rigidity. In particular, several studies in Italy, France, the Netherlands, Portugal and Spain document that the sensitivity of wages to aggregate perturbations crucially depends on wages' distance from the minimum floor, and estimate average elasticities of wages to changes in the minimum floors well below 1.⁹ In that context, the degree of concentration of wages around minimum floors is a key determinant of the real impacts of the inability to renegotiate collective contracts.

As for macroeconomic factors, collective agreements typically set nominal wage floors and the level of inflation shapes the possible bite of wage rigidity. Indeed, wage rigidity has been found to be particularly prevalent in low-inflation scenarios.¹⁰ However, during periods of high

⁹Card and Cardoso (2022) and Cardoso and Portugal (2005) document elasticities of wages to increases in minimum wage floors of about 0.5 in Portugal. Caloia et al. (2020) find elasticities of about 0.2 in the Netherlands, similar to those in France (Gautier et al., 2022). Adamopoulou and Villanueva (2022a) document very different responses close and far from the wage floors in the metalworking industry in Spain and Italy. See Adamopoulou and Villanueva (2022b) for a review.

¹⁰Fehr and Goette (2005) examine the incidence of wage cuts in Switzerland in periods of low and high inflation and document that the incidence of wage cuts is similar in both. Their results imply that the absence of wage cuts in low inflation environments causes job losses. Faia and Pezone (2020) examine collective contracts in a period including the low-inflation recession of 2008 and find that monetary policy shocks lower the valuation of the stock market value of firms unable to renegotiate wages.

inflation, the dispersion of nominal wage growth caused by staggered collective contracts can result in small differences in real costs to the employer. While some collective contracts feature wage indexation clauses through which high inflation rates end up triggering wage growth adjustments after some time, it is important to examine low- and high-inflation periods to obtain a full picture of the relevance of wage rigidity.

A crucial issue when assessing the cost of possible forms of wage rigidity is the persistence of non-employment. The above models abstract from inputs other than labor. However, the literature on the economic consequences of minimum wage increases does discuss the differences between short- and long- run employment responses. A relevant case is models that feature putty-clay technology. This form of technology implies that firms can freely substitute between capital and labor when they pay the entry cost of building a machine. But as soon as capital is installed, firms cannot easily change their labor demand-see Sorkin (2015). In these models, short-run employment responses to increases in minimum wages are limited, as they mainly pick scale effects (for example, the reduction in output when minimum wages are passed-through to prices). Nevertheless, the full extent of the substitution between capital and labor can only be observed in the long run, once firms are able to adjust their mix of capital and labor. Those considerations are potentially more important when minimum wage increases are perceived as permanent, for example, when contracts that cannot be renegotiated are settled for a long period.

Finally, labor market institutions may shape the labor market adjustment. The above models do not consider alternatives to job loss like short-term reduction in hours, which is a possible way to weather a recession. Similarly, the existence of job separation costs may induce higher separation rates among workers with fixed-term contracts.

In sum, we can derive four testable hypotheses:

1. In any recession, a sudden deterioration of the state of the economy reduces the outside option of workers resulting in lower growth in negotiated wages only among contracts that could be renegotiated. This is derived from the bargaining model of Olivei and Tenreyro (2009).

2. The response of overall wages to contract renegotiation depends on the "wage cushion", i.e., the distance between actual wages and the worker-specific wage floor. This is an implication of the empirical observation that not all wages move with the wage floors (Adamopoulou and Villanueva, 2022b; Card and Cardoso, 2022; Gautier et al., 2022).

3. The set of workers covered by contracts that can be renegotiated experience fewer job losses than those covered by contracts already bargained at the time of the shock. The size and persistence of those real impacts depend on inflation levels. The former is an implication of Olivei and Tenreyro (2009) and the latter of studies on "inflation greasing the wheels of the labor market" (Card and Hyslop, 1997; Tobin, 1972; Blanco et al., 2022).

4. The real impacts of wage rigidity (especially in the long run) increase with contract duration, either due to the cumulative magnitude of the shock or because of capital adjustments (Sorkin, 2015). Those impacts can be detected only if the period of analysis is long enough.

4 Data

We use two main datasets for our analysis. The first one is the Census of Collective Agreements signed in Spain between 1990 and 2020 – *Registro de Convenios y Acuerdos Colectivos*. The second is the Continuous Sample of Working Histories, a 4% random sample of Social Security records - *Muestra Continua de Vidas Laborales*.¹¹ We describe each source in detail below.

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 negotiated wage growth for that period. As Tables 1a and 1b show, about 63% of signed contracts in the years around the 1993 and 2009 recessions specify an ex-post adjustment (escalation clause) if realized inflation exceeds a threshold specified in the contract. In that 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). Importantly for the purpose of our study, the Census of Collective Agreements includes information on the validity period and the date in which the agreement was signed. Other entries are not compulsory and unions do not always fill those. Still, about 70% of forms included the skill-specific minimum wage floors in the agreements signed between 1994 and 2001. We updated those wage floors backwards to 1991 and forwards to 2007 using the annual negotiated wage growth reported in each agreement. This allows us to obtain worker-specific wage floors for a 60% and 40% subsample of workers in the years right before the onset of the 1993 and 2009 recessions.¹²

The second sample is drawn from the Continuous Sample of Working Histories (CSWH), a 4% sample of any worker with an active record with the Social Security System at some point in 2004. The information is recorded electronically and includes each worker’s retrospective labor market history –potentially, dating back to 1988. The CSWH has a longitudinal design. In particular, the sample tracks any individual who is present in one of the subsequent waves and 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, 2017). The register collects monthly information on the employment status, the earnings, and the skill level of each worker. Earnings refer to base wages and thus exclude bonuses, overtime or other complements.

For the analysis of the employment dynamics during the 2009 recession, we use information on workers that were present in the 2010 CSWH, also recording the labor market history of any individual who has ever been present in the sample between 2005 and 2009, to avoid possible sample selection biases caused by workers who left the labor market between the last quarter of 2008 and the first quarter of 2009. For the 1992 recession, we rely on retrospective information of workers present in the 2005 CSWH. Retrospective information before 2004 would not necessarily

¹¹Since Social Security data are not available yet, we use the labor force survey to study the effects of wage rigidity during the COVID-19 pandemic. An additional advantage of the labor force survey is that it also contains information on short-time work.

¹²The subsample is smaller for the 2009 recession as updating the wage floors forward for 6 years (from 2001 to 2007) is more demanding than updating the information backwards for 3 years (from 1994 to 1991).

be representative of the population of workers prior to that year if those individuals left the labor force at some point before and kept no links with the Social Security System. Bonhomme and Hospido (2017) show that, at least for males, this lack of representativeness is not a first-order concern.

All employers must assign one of ten possible skill levels to each employee above 18 years old as contributions to Social Security differ across levels. The skill levels correspond to those set in collective contracts, a feature that allows us to assign to each worker the corresponding minimum wage.¹³

4.1 Linking datasets

The Census 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 match the Census of Collective Agreements to the Social Security records using the three-digit industry of economic activity and the province of the establishment where employees work.

Sample coverage

Our sample is composed of all workers employed by establishments that belong to industries covered by a province-level agreement. The resulting sample covers around 50% of all employees. This is partly because 20-25% of Spanish employees are not covered by any collective contract whatsoever according to the OECD. In addition, we exclude workers in industries regulated by nation- or region-level contracts.¹⁴

The reason we focus on provincial collective agreements is threefold. 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, 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 (less than 15%) could be covered by firm-level contracts. Such contracts are not 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 collective contracts, so employees of those firms were effectively affected by collective contracts set at wider geographical level. Importantly, the variation of wage settlements across provinces and three-digit industries allow us to control for separate trends across geographic

¹³The 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 (7) entry-level clerks. Finally, blue-collar workers are also split into three levels according to the level of qualification. This classification is comparable to that settled in collective contracts, as we discuss below.

¹⁴For example, 5% of the employees in Social Security records work in Financial Services or in Real Estate, which are covered by a nationwide contract. This type of collective contracts would provide little identification, as we control for province dummies throughout the analysis.

areas and sectors.

Lastly, we restrict the sample to employees between 18 and 57 years of age as of December 1991 or December 2007, who had been employed at the firm since at least the last quarter of 1991 or 2007. The latter restriction guarantees that workers have been continuously employed at the firm since the time the oldest agreement in each recession was signed. In this way, employees have similar working histories at the onset of each recession, i.e., they all have accumulated at least one year of tenure.¹⁵

4.2 Final dataset

Tables 1a and 1b report the descriptive statistics for the samples we use to analyze the effects of wage rigidity during the 1993 and 2009 recessions. All worker characteristics refer to December 1991 or 2007, respectively. As column (1) shows, around 15% of workers in the estimation sample are high-skilled (have a college degree or work as managers), 30-35% have clerical jobs that do not require a college degree, while 55-50% are blue-collar workers. Around 85% of workers are covered by an open-ended individual contract.¹⁶ The share of female employees is higher in the 2009 than in the 1993 sample, in line with the rise in female labor force participation observed over time (Source: [ILO](#)).

Columns (2) and (3) of Tables 1a and 1b compare the workers' and collective contracts' characteristics of the treated and control groups, i.e., across contracts signed before and after the onset of each recession. Column (4) of Tables 1a and 1b reports the differences across these characteristics after controlling for province and three-digit industry dummies. That is, column (4) reports the coefficients of a regression of each covariate on the dummy "contract signed before 1992m12 or 2008m9" and a full set of 50 province dummies and three-digit industry dummies. Although there were originally some differences in workers' characteristics (compare columns 2 and 3), their magnitude becomes negligible after controlling for province and industry dummies (see column 4). This implies that the treated and control groups of workers are comparable in terms of observable characteristics.

By contrast, a statistically significant difference remains in terms of negotiated wage growth across contracts signed before and after December 1992 (1.8 pp higher among those signed prior to the 1993 recession) or before and after September 2008 (1.3 pp higher among those signed prior to the 2009 recession). Likewise, there is a statistically significant difference in the duration of contracts signed before and after the onset of each recession, as those signed pre-recession tend to be lengthier.¹⁷ These patterns confirm i) the presence of wage rigidity among collective contracts signed pre-recession, which may accumulate over multiple years depending

¹⁵Including contracts signed earlier, e.g., during the first quarter of 2007 would require us to use a sample of employees working at the firm already in 2006. Almost one third of the working force in Spain is hired with fixed-term contracts, so using a sample of job stayers from December 2006 to December 2008 would bias the sample excessively towards workers with open-ended contracts.

¹⁶Due to the employment protection legislation, open-ended contracts entail higher firing costs than fixed-term contracts. See [Section 2](#) for details.

¹⁷During the 2009 recession, there is also a statistically significant difference in the presence of escalation clauses across contracts signed pre/post September 2008. However, this is most likely due to the low inflation rates in the period 2009-2010 that made the inclusion of escalation clauses in collective contracts less imperative.

on contract duration, ii) that social partners take into account the prevailing macroeconomic conditions at the time of the negotiations when setting the wage growth and contract duration.

4.3 Subsample with information on collective agreement wage levels

Unions and employers stopped recording minimum wage floors in the forms they submitted to the Register of Collective Agreements as of 2001. However, for the period spanning 1994-2001, the wage floor by skill level (excluding returns to seniority) was available for around 70% of all province level contracts.¹⁸ We estimated the minimum wage floors in December 2007 (prior to the 2009 recession) by updating the 2001 levels using the negotiated wage growth settled in those union contracts between 2001 and 2007.¹⁹ Namely, we inflated 2001 wage floors by the initially agreed wage growth in the contract plus any adjustment due to inflation escalation clauses. This procedure allowed us to obtain the minimum wage floors in 551 out of the 1305 province-industry cells (42% of all province level contracts) in December 2007, i.e., about one year prior the 2009 recession. We used a similar procedure to estimate the minimum wage floors in December 1991, i.e., about one year prior to the 1993 recession, by updating the 1994 wage floors backwards.²⁰ To our knowledge, this is the first paper that merges negotiated minimum wage floors to the universe of social security records for a period spanning 1990 and late 2010. In later work, Card and Cardoso (2022) did the same for Portugal for the period 2010-2016 but also there the sample of workers that could be assigned a minimum wage floor was around 50% of the original sample.

Reassuringly, in our subsample with available information on workers' minimum wage floors, collective contracts' and workers' characteristics resemble those of the full sample (see Tables B2a and B2b in Appendix B). Moreover, as Figure B2 in Appendix B shows, the distribution of negotiated nominal wage growth settled in the sample of collective contracts for which we observe minimum wage floors (panels b) is very similar to that of the full sample (panels a). Both in the subsample and the full sample, the distribution of negotiated nominal wage growth settled by contracts signed after the onset of each recession lies to the left of that settled by contracts signed before each recession, thus confirming a higher level of wage rigidity among the latter.

¹⁸The form that unions and employers submit to the Ministry of Labor contains 10 minimum wage floors, one for each skill group included in the Spanish Social Security System. Thus, although collective contracts typically set a higher degree of detail than 10 skill groups, the assignment of wage floors to workers in this sample is 1-1.

¹⁹The dataset contains both the current wage structure in the agreement as well as that in the previous agreement, thus allowing to test if the structure of wage floors varies across agreements. For the vast majority of cases, all wage floors are updated by the same wage growth, thus preserving the wage floor structure across skill levels.

²⁰Our procedure assumes that the wage structure across skill levels is preserved whenever a contract is renewed (that is, each minimum wage floor increases by the same percent). We have coded minimum wage floors in some 200 collective contracts by hand and found that the wage floors' structure is indeed preserved across contracts (the R-squared of a regression of minimum wage floors set in collective contracts between 2007 and 2014 on a set of collective contract dummies is 97%).

5 Methods

To study the effects of wage rigidity, we estimate models of workers' transition from employment to non-employment as a function of whether their corresponding collective contract was signed before or after the onset of each recession. We focus solely on collective contracts that were effective in the first year of each recession and settled wage growth for 1993 and 2009, respectively. As shown in the descriptive statistics (Tables 1a and 1b), contracts signed before the onset of the 1993 or 2009 recession settled higher wage increases for 1993 or 2009 than contracts signed after. Therefore, firms in the same industry and province could be subject to a different degree of "nominal wage push" during the 1993 or 2009 recession depending on whether their collective contract was signed before the deterioration of the macroeconomic conditions (when the full extent of employment destruction was hard to predict) or after (when bargaining parties could have observed contractions in demand). The parameter of interest can therefore be interpreted as the slope of a province-industry level demand curve: higher wage increases should increase workers' probability of becoming unemployed during the recession.

5.1 Discontinuities in the arrival of aggregate information

Our baseline empirical strategy uses the sharp change in expectations about the outbreak of each recession. We assume that the date of signature of a collective agreement reflects the information available to unions and employer federations, who update continuously their information about industry-, province- and aggregate-economy-level shocks. The second assumption is that no comparable sharp change of information occurred before the outbreak. Under those two assumptions, one can identify the wage and employment responses to the arrival of new macroeconomic information by using intention-to-treat (ITT) models.

We start by estimating the effects on negotiated wage growth settled for 1993 or 2009 (a sort of a "first stage") using the following reduced-form model:

$$\Delta NW_{sp} = \alpha_0 + \sum_{l=-5}^{+6} \alpha_{1l} Q_l \text{SIGNED}_{sp} + ind_s + prov_p + \epsilon_{sp}. \quad (1)$$

The dependent variable, ΔNW_{sp} , is the negotiated wage growth set in industry s and province p for 1993 or 2009, $Q_l \text{SIGNED}_{sp}$ is the quarter of signature of the collective contract in industry s and province p , and $l=0$ is 1992q4 (quarter before the onset of the 1993 recession) or 2008q3 (quarter before the onset of the 2009 recession). The coefficients of the leads and lags, $\sum_{l=-5}^{+6} \alpha_{1l}$, capture any discontinuous change in negotiated wage settlements after 1992q4 or 2008q3, as well as any possible pre-trends.

We verify that negotiated wage growth is reflected into workers' actual wages by adopting a difference-in-differences strategy in [Section 5.2](#). By allowing differential responses over the wage distribution, we can test whether the cushion plays a role in the transmission of macroeconomic shocks to wages.

We then examine the effects of wage rigidity on employment outcomes by estimating monthly

regressions of the following linear probability model (LPM):

$$Y_{jspt} = \alpha_{0t} + \alpha_{1t}SIGNEDpre_{s,p} + \gamma X_{jt} + ind_s + prov_p + \epsilon_{jspt}. \quad (2)$$

We first run a set of monthly regressions to study *transitions* into non-employment, where the dependent variable, Y_{jspt} , takes the value 1 if worker j , who was employed in December 1992 or December 2008 in industry s and province p , loses her job in month t , and 0 otherwise. We then run another set of monthly regressions to study the *persistence* of non-employment. In this case, the dependent variable, Y_{jspt} , takes the value 1 if worker j , who was employed in December 1992 or December 2008 in industry s and province p , is non-employed (works less than 15 days) in month t , and 0 otherwise. We consider a 2-year and a 4-year horizon to study the employment effects during the 1993 and 2009 recession, respectively. As we trace the same workers over time, we can examine whether, on average, displaced workers eventually find new jobs, which is informative about the total costs of wage rigidity. We also control for worker characteristics X_{jt} , namely, age (five age dummies in ten year bands), gender, and occupation. The coefficient of the binary variable $SIGNEDpre_{s,p}$ captures the effect of wage rigidity. To define the variable $SIGNEDpre_{s,p}$ we focus on all collective contracts that settled wage growth for the first recessionary year and got signed in a window surrounding the onset of each recession. More specifically, when we analyze the 1993 recession, we focus on collective contracts that settled wage growth for 1993 and got signed between 1991m10 and 1994m6. Then, the variable $SIGNEDpre_{s,p}$ takes the value 1 for presumably "rigid" contracts that got signed in the pre-recessionary period 1991m10-1992m12 and settled nominal wage growth for 1993, and the value 0 for presumably "flexible" contracts that also settled nominal wage growth for 1993 but got signed in the recessionary period 1993m1-1994m6. Similarly, when we analyze the 2009 recession, we focus on collective contracts that settled wage growth for 2009 and got signed between 2007m10 and 2010m6. Then, the variable $SIGNEDpre_{s,p}$ takes the value 1 for presumably "rigid" contracts that got signed in the pre-recessionary period 2007m10-2008m9 and settled nominal wage growth for 2009, and the value 0 for presumably "flexible" contracts that also settled nominal wage growth for 2009 but got signed in the recessionary period 2008m10-2010m6.^{21,22}

As there may be unobserved factors that correlate both with the date of signature and with the demand of labor, such as local demand shocks, we control for unrestricted province and three-digit industry dummies ($prov_p$ and ind_s). More specifically, we include dummies for each province (49, excluding Madrid) and up to 112 3-digit industry dummies (the excluded industry

²¹Unlike negotiated wage growth, we do not conduct an event study when we study employment outcomes. This is because the signature dates of collective contracts that get renewed *after* the onset of recessions are likely to reflect both arrival of information and endogenous reactions of firms and employers. Therefore, we pool all post periods together with the dummy $SIGNEDpre_{s,p}=0$, thus avoiding weighing more contracts that could potentially be subject to non random delays.

²²In this part of the analysis, we do not take into consideration the remaining duration of the collective contract (beyond 1993 or 2009), which could prolong wage rigidity especially amidst the 2009 recession, when the duration of collective contracts was typically above three years. In [Section 7.3](#), we distinguish between collective contracts of shorter and longer duration and show that the latter are a possible explanation behind the "delayed" job losses that occurred after Lehman brothers, following almost two years of accumulated wage rigidity.

is cleaning services, an industry with at least one provincial agreement bargained in each of the quarters we consider). These dummies also account for differences in the duration of collective contacts or in the timing/delay of the negotiations that may be sector/province specific.

Our empirical specification 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 could be other interpretations of our results. Wages and employment could be reacting to aggregate perturbations prior to the recession, in which case we would not really identify an amplification mechanism to a well-defined macro perturbation. Alternatively, the interpretation of the results 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. Moreover, contracts may be applied in a more lax manner during a recession.

We address these concerns as follows. First, we test the assumption that 1993q1 and 2008q4 was indeed the moment when wage settlements changed abruptly by estimating [Equation 1](#). Namely, we examine the negotiated wage growth set in collective contracts signed for several quarters before and after the onset of each recession.²³ Second, we deal with the possible 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 address this issue by using the expiration date rather than the signature date of collective contracts to define our measure of wage rigidity. The idea is that social partners that signed collective contracts in the pre-recession period set the end of the validity without being able to foresee the deterioration of the macroeconomic conditions. Subsequently, some of these contracts happened to expire (and potentially got renewed) before or after the onset of the recessions. This alternative identification strategy ensures the exogeneity of the treatment.²⁴ Third, we propose a way to tackle the lack of exact information on the number of firms that opted out from collective agreements and for the degree of enforcement of collective contracts during the period of our analysis. To do so, we test for the prevalence of “informal” opt-outs by examining actual wage growth among job stayers. 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 their pre-recession earnings from their corresponding minimum wage floor.

5.2 Difference-in-differences estimates

Our second empirical strategy is a difference-in-differences (DD) framework, which builds on the fact that collective contracts set different minimum wages across skill groups. This strategy assumes that wage settlements in collective contracts do not affect the labor costs and employment chances of employees with pre-recession wages well above the minima (control group).²⁵

²³We group dates of signature by quarters, as on average we have some 20 collective contracts being signed every month. Grouping contracts by quarter of signature allows us to control for industry and province dummies, while finer disaggregation (say, months) would not.

²⁴While collective contracts get renewed in all months, the vast majority of collective contracts tend to expire on January 1, resulting in lower variation (see Figure B3 in Appendix B). This is why we use the date of signature as our main identification strategy and the expiration date in a robustness exercise.

²⁵This assumption would not hold if workers with different wage levels were perfect complements in producing output as in this case layoff decisions would not depend on the relative wage of each worker. This assumption

For this part of the analysis, we use the subsample of contracts with information on minimum wage floors 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 wages were closest to the minima and thus at highest risk of displacement following already settled negotiated wage increases.

Another advantage of the DD setting is that it allows us to use contract-specific fixed effects, i.e., 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. Yet, we are able to identify the impact of wage rigidity on employment outcomes by comparing workers whose pre-recession wages were close to the minima to workers covered by the same collective contract but whose wages were further away from the minima. If the dispersion of wages induced by collective contract staggering matters, job losses will be more prevalent in relative terms among workers close to the minima, who are covered by contracts signed before the onset of recessions.

We thus examine whether workers' wages and employment outcomes are affected in a differential way by using variation in the date when the contract was signed, interacted with the distance to the minimum wage floor. We run the following regression:

$$Y_{jspt} = \delta_0 + \sum_{k=k_1}^{k=k_3} \delta_{1t} \text{SIGNED}pre_{sp} * 1(W_{j\text{sp}}^{pre} \leq k\bar{W}_{j\text{sp}}) + \sum_{k=k_1}^{k=k_3} \delta_{2t} 1(W_{j\text{sp}}^{pre} \leq k\bar{W}_{j\text{sp}}) + \theta_{sp} + \epsilon_{jspt}. \quad (3)$$

Y_{jspt} is the outcome of interest (workers' actual wage growth, job loss or non-employment) and $\text{SIGNED}pre_{sp}$ is an indicator that takes value 1 if the collective contract was signed before the onset of each recession and 0 otherwise. The binary variable $1(W_{j\text{sp}}^{pre} \leq k\bar{W}_{j\text{sp}})$ denotes the distance between the wage of worker j and the minimum wage floor that corresponded to her skill, industry and province as of December 1991 or 2007, i.e., one year prior to the onset of each recession. We use three step functions k to define three distinct groups of workers by distance from the minima: those with wages that are at most 1.1 times the minimum wage floors, between 1.1 and 1.2 times, and between 1.2 and 1.4 times the minima. The omitted group are workers whose wages in December 1991 or 2007 were at least 1.4 times the minimum wage floors. As in the baseline model, we control for industry and province specific determinants of wage growth or job losses by including 49 province dummies and three-digit industry indicators. Additionally, in this DD setting, we are able to include a collective contract-specific fixed effect θ_{sp} , i.e., an interaction of province and three-digit industry dummies that absorbs any trend in wage growth or employment outcomes affecting all workers covered by the agreement. Moreover, we control for nine dummies denoting the skill level of the worker as minimum wages in collective contracts are specific for each skill group (see Table B.1 in the Appendix B). Finally, we experiment including worker-level characteristics (age, gender and occupation dummies).

We first run a regression where the dependent variable is workers' actual wage growth in 1992-1993 or 2008-2009. The main aim of this specification is to test whether changes in

may also not hold in periods of high inflation among workers covered by contracts with escalation clauses.

negotiated wage settlements were indeed most binding for workers whose pre-recession wages were closest to the minima –ignoring possible employment impacts, that we analyze later. For this regression we only consider full-time employees who stay in the firm for at least one year after the outbreak of the recession and whose earnings are not below the maximum contribution to the Social Security system. The coefficient of interest is δ_1 . A positive value of δ_1 implies that collective contracts signed before the onset of the recession could not adjust to the business cycle, hence resulting in higher wage increases than contracts signed after, and that those wage increases were more binding among workers with pre-recession wages close to the minima.

Similarly, under the assumption of a downward-sloping labor demand, workers covered by agreements settled before each recession and with wages close to the minimum wage floors are expected to face a higher risk of job loss, as they entail larger wage cost increases for their employers than the rest of workers. As in the specification with the full sample, we then examine monthly transitions into and persistence of non-employment by comparing workers subject to different levels of wage rigidity but also of different distance from the minimum wage floors.²⁶

6 Results

6.1 Negotiated wage growth set in collective contracts

We start by examining the negotiated nominal wage growth, settled in all province-industry contracts for the first year of each recession, as a function of the quarter when the contract was signed. In the regressions, we consider 4-5 quarters before and up to 7 quarters after the onset of each recession and include province \times three-digit industry fixed effects. Figure 1 plots the estimated coefficients of Equation 1, along with the estimated robust standard errors clustered at the province and 3-digit industry level level. We observe a decrease in negotiated nominal wage growth for 1993 and 2009 of around 1.0-1.5 pp on average, which materializes only after the first quarter of each recession -without any noticeable pre-trend beforehand.²⁷ Negotiated nominal wage growth adjusts downwards steadily as more information on the severity of the recession arrives to social partners.²⁸ This implies a different degree of wage rigidity between employers covered by contracts signed before and after the onset of each recession; the former were subject to wage rigidity and thus forced to apply wage increases that reflected the pre-recession macroeconomic conditions, while the latter could bargain wage increases that would reflect recessionary aggregate conditions. We argue that whether an employer was subject to wage rigidity or not was exogenous as both recessions were unlikely to be foreseen (also

²⁶By pooling all post periods together with the dummy $SIGNED_{pre_{sp}}=0$ and by estimating separate monthly regressions, we avoid many of the problems raised in the recent econometrics literature on staggered diff-in-diff –see Callaway and Sant’Anna, 2021 and Baker et al., 2022.

²⁷Contracts signed in 1994 for the 1993 recession (panel a) and in 2010 for the Lehman Brothers recession (panel b) were signed with a delay and result in wage increases ex-post which were not observed as of 1993 and 2009.

²⁸This pattern may also be due to some harder hit industries taking longer to reach an agreement. Therefore, in what follows, we only consider whether collective contacts were signed pre/post-recession (rather than the quarter of signature) and use collective contract expirations as an alternative identification strategy to study the effects on employment.

supported by the lack of pre-trends). Furthermore, as we document shortly, the actual wage growth of job stayers suggests that those settlements were binding.

6.2 Actual wage growth by workers' distance from the minima

We examine how binding collective contracts are by analyzing the actual nominal wage growth in the first year of each recession among workers with pre-recession earnings close or far from the negotiated minima. To do so, we compute the distance between workers' wages and their corresponding minimum wage floor (statutory minimum in their province-industry-skill group cell). Figure B4 in Appendix B presents the distribution of the wage cushion in December 2007. Around 9% of workers have wages between 1 and 1.1 times their minimum wage floor, 12% between 1.1 and 1.2 times the minimum wage floors, 19% between 1.2 and 1.4 times the minimum wage floors and more than half of the workers in the sample have wages 1.4 times their minimum wage floor or higher.²⁹ Similarly, we compute workers' wage cushion as of December 1991. Concentration around the minima used to be higher in the 90's as 22% of workers have wages between 1 and 1.1 times their minimum wage floor, 12% between 1.1 and 1.2 times the minimum wage floors, 17% between 1.2 and 1.4 times the minimum wage floors and 35% of workers have wages 1.4 times their minimum wage floor or higher.

Table 2 presents the estimates of Equation 3, where the dependent variable is the individual's nominal base wage growth between December 1992 and December 1993 (panel a) or December 2008 and December 2009 (panel b). The sample is restricted to full time employees with available information on their corresponding minimum wage floors, who stayed in the same firm all through 1993 (panel a) or 2009 (panel b).³⁰ We report the results for workers whose monthly earnings were at most 10% higher than the corresponding minimum wage floor, between 10% and 20% higher or between 20% and 40% higher. The omitted group (reference category) are workers whose monthly earnings in December 1991 or 2007 were 40% higher than the corresponding minimum. Standard errors are corrected for heteroscedasticity and arbitrary correlation across workers in the same three-digit industry \times province cell. The regressions control for three-digit industry fixed effects in column (1) and for collective contract fixed effects in column (2).³¹

The estimates in Table 2, row 1, column (1), panels a and b suggest that nominal wage growth among job stayers subject to "rigid" collective contracts and with pre-recession wages very close to the minima is 1.4 pp (1993 recession) and 1.9 pp (2009 recession) higher than wage growth in the omitted group. The estimates are very similar when we control for collective contract fixed effects (row 1, column 2, panels a and b), suggesting that sample selection or idiosyncratic shocks affecting province-industry cells do not play an important role in determining wage growth. Given that the negotiated wage growth was 1.8 pp and 1.3 pp higher among

²⁹We find some slippage, as about 8% of workers have wages below their corresponding minimum wage floor. 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 collective agreement.

³⁰We also exclude workers whose earnings were censored at the Social Security maximum contribution.

³¹While the main impact of date of signature is a contract-level characteristic and it is not identified in models that include collective contract fixed effects, the interaction of the date of signature and the distance to the minimum in the collective agreement is identified.

contracts signed before the 1993 and 2009 recession (see Tables 1a and 1b, row 11, column 4), our estimates imply a very high pass-through (around 80 to 100%) for workers close to the minimum wage floors. The effects (and the pass-through) gradually fade away as we move further away from the minima, and ultimately vanish among workers whose monthly earnings in December 1991 or 2007 were at least 20% higher than the collective contract minimum (Table 2, column 2, rows 3 and 5 in panels a and b). These results are consistent with the notion that contracts signed after a large aggregate shock settle lower wage increases, and those are binding –as in Olivei and Tenreyro, (2007) and (2009). However, wage cushions acted like a buffer against wage rigidity in both recessions, and the spillovers of collective contracts to overall wage growth were confined to wages close to the floors –as in Adamopoulou and Villanueva (2022a) and Card and Cardoso (2022).

Whereas the distribution of *nominal* wage changes among workers is similar in both recessions, the implications on wage rigidity in *real* terms varied substantially. More specifically, the inflation rate in 1993 reached 4.6%, while average wage growth was about 2% in the sample of 1993 stayers, well below the 4.6%. By contrast, the inflation rate in 2009 was close or below zero. Consequently, the estimated nominal wage growth in 2009 (around 1.6% for workers close to the minima) translated into increases in the real cost of labor. This asymmetry in the real wage effects of the two recessions could have differential implications in terms of the distribution of job losses as we will see in [Section 6.3](#).

6.3 Employment outcomes

According to our analysis so far, staggered collective contracts across province-industry cells resulted in cross-sectional dispersion in negotiated wage growth, which translated into differential actual wage growth among workers. A natural question that arises is whether this differential wage rigidity also led to differential employment dynamics. To answer this, we estimate monthly regressions at the worker level to examine the response of job destruction and then we analyze the persistence of the non-employment status.

6.3.1 Job loss and persistence of non-employment

We first compare the monthly probability of job loss and the incidence of non-employment in the aftermath of each recession for workers covered by contracts signed before or after the outbreak of each negative macroeconomic shock.³² The incidence of non-employment captures not only job losses but also the extent to which displaced workers manage to find new jobs. To this end, we restrict the sample to workers with at least one year of tenure in December 1992 or 2008 and estimate [Equation 2](#) for each month spanning a period of several years after each recession.³³ All regressions are conducted at the monthly level and thus implicitly control for month-specific three-digit industry and month-specific province dummies. Figure 2, left panels plot the estimates of the monthly probability of job loss along with the 95% confidence intervals.

³²Throughout the paper, we abuse the terminology and use interchangeably the terms non-employment and unemployment since they are indistinguishable in the social security data.

³³Month 0 indicates the month prior to the onset of each recession, i.e. December 1992 and December 2008, respectively.

We find an increased probability of job loss among workers covered by contracts signed before each recession. In the case of the 1993 recession, the effect is immediate (job destruction peaks in month 3 –see Table 3, panel a) while in the case of the 2009 recession the effect takes quite a few months to materialize (job destruction increases in month 21 –see Table 3, panel b). These estimates do not fully explain the reasons for the delay in employment losses after the fall of Lehman Brothers. Collective contract duration may have played a role as lengthier contracts could prolong rigid wages. Alternatively, adjustment costs may have induced firms to postpone lay-offs, contributing to higher job destruction. We examine possible mechanisms underlying the employment effects as well as the role of institutions in [Section 7](#).

Consistent with the findings in the left panels of Figure 2, the right panels document an increase in the probability of being non-employed among workers originally covered by contracts exposed to wage rigidity (see also Table 4 panels a and b). In the case of the 1993 recession, wage rigidity results in non-employment by the end of the first year (month 12) but this effect is short-lived (the estimate is around -0.005 by the end of the second year, i.e., even negative). Presumably, high inflation rates during that period resulted in real wage decreases, thus greasing the wheels of the labor market –a hypothesis we explicitly test below. In the case of the 2009 recession instead, wage rigidity leads to a steady increase in the incidence of non-employment and the effects are visible even four years after the outbreak of the macroeconomic shock, with workers covered by contracts signed pre recession experiencing a 1 pp higher probability of remaining non employed. This finding implies that real wage rigidity during a low-inflation recession can amplify the impact of aggregate fluctuations in a very persistent way.

To put magnitudes in perspective, the wage rigidity in province-industry cells (measured by whether firms could renegotiate wages after a large aggregate shock) resulted in 1 pp higher nominal (and real) wage growth in 2009-2010 (see Table 1b, row 13, column 4). Moreover, workers in those province-cell industries experienced up to 1% higher probability of non-employment by the end of 2010 (see Table 4, column 3 in panel b). Those estimates are consistent with an elasticity of employment status to negotiated wages of 1.0 (in absolute value). The elasticity of employment status to nominal negotiated wage growth is lower during the high-inflation recession of 1993, reaching at most 0.6(=1.0/1.8) in September 1993 (see Table 4, column 1 in panel a and Table 1a, row 11, column 4), and becomes zero thereafter. Both elasticities are within the range of estimates in the literature, e.g., those estimated at the worker level by Card and Cardoso (2022) for Portugal (practically zero) or at the firm level by Fanfani (2020) and Martins (2021) for Italy [0.4; 1.2] and Portugal [0.7; 2.0], respectively. As we show below, this wide range of estimates may be explained by differences in the segment of workers under consideration (in terms of distance from the minimum wage floors), macroeconomics factors (inflation rate) as well as institutional factors (collective contract duration).

Given the striking differences in the persistence of employment losses during high- and low-inflation downturns, in what follows we focus on the probability of job loss when we analyze the 1993 recession and on the incidence of non-employment when we analyze the 2009 recession.

6.3.2 Heterogeneity by distance to the minima

Guided by the results in [Section 6.2](#), we examine whether wage rigidity has differential employment effects on workers depending on their distance from the minimum wages. We adopt a difference-in-differences framework and compare employment outcomes of workers with wages in December 1991 or 2007 0-10%, 10-20% and 20-40% above the minima to those of workers far from the minima (>40%), who are covered by collective agreements signed before or after the onset of each recession. We thus estimate [Equation 3](#), where the dependent variable is the monthly probability of job loss during the 1993 recession and the monthly probability of being non-employed in the aftermath of the fall of Lehman Brothers. The sample is restricted to full time employees as of December 1991 (1993 recession) or 2007 (2009 recession). with available information on their corresponding minimum wage floors.

The three panels in [Figure 3a](#) show that in the case of the 1993 recession there was no statistically significant employment effect on any group of workers under consideration. This could be attributed to the fact that high inflation in 1993 counterbalanced nominal wage increases even among workers whose pre-recession wages were close to the minimum wage floors (see [Table 2](#), panel a). By contrast, real wages did increase during the 2009 recession, especially among workers closer to the minima (see [Table 2](#), panel b). As a result, we observe an increased incidence of unemployment mainly among workers very close or close to the minima ([Figure 3b](#), left and central panels) while there is no effect among workers further away from the minima ([Figure 4b](#), right panel). More specifically, we find that among workers covered by contracts signed prior to 2008q3 and with cushions below 10% the probability of being non-employed was 5 pp higher than among comparable workers whose contract was signed afterwards. Conversely, the results are basically 0 for workers with cushions above 20%. Therefore, unemployment following negative macroeconomic shocks during non-inflationary periods is likely to be confined to the subgroup of workers close to the minima.

6.4 Robustness

In this section we conduct a battery of tests regarding the validity of our identification strategy. More specifically, we examine i) whether the cross-sectional dispersion in the degree of wage rigidity is indeed exogenous, ii) whether the province-sector variability that we exploit throughout the analysis is plausible, and iii) whether the parallel trend assumption is satisfied in our difference-in-differences framework.

6.4.1 Identification using the date of expiration

In our empirical strategy we use the date of signature of collective contracts to derive a measure of wage rigidity. A possible concern with this strategy is that employer federations or unions may anticipate the extent of job losses occurring during economic downturns. As a result, either union or employer federations anticipating a downturn could delay negotiations. This would imply that the date of signature of collective contracts and (the associated degree of wage rigidity) is not always exogenous. Available data on the delays between previous contract

expiration and new contract signature suggest that new contracts are typically signed shortly after the expiration of the previous agreement. For example, among collective contracts binding in 2009 and having expired at the end of 2006, those representing 80% of the labor force were signed during 2007, and only 16% were signed in 2008. Among collective contracts binding in 2009 and having expired at the end of 2007, those covering 83% of the labor force were signed in 2008 and only 14% were signed in 2009. In both cases, very few contracts were signed before the expiration of the previous agreement. Basically, all collective contracts signed during 2009 had expired by the end of 2008. A similar pattern is observed in the 1993 recession. Thus, a strong determinant of the date of signature is when the previous collective contract had expired, which is in turn determined by perceptions of the bargaining parties back at the time of the expired contract's signature.

To address any possible endogeneity concern, we adopt an alternative identification strategy and use the date of expiration (rather than the date of signature) of collective contracts as a proxy for wage rigidity. The date of expiration is arguably exogenous since unions and employment federations cannot possibly manipulate it *ex post*.³⁴ Figures 4a and 4b report the results on the probability of job loss and the incidence of non-employment with this alternative identification strategy. The effects are very similar to the benchmark estimates: job loss peaks relatively quickly (month 6) during the 1993 recession but there is no evidence of non-employment on a longer horizon. By contrast, the incidence of non-employment remains persistently high for about two years after the fall of Lehman Brothers. These results support our benchmark identification strategy and demonstrate that the effects on employment are robust to different measures of wage rigidity.

6.4.2 A case study with construction

In our benchmark specification we exploit variation by industry-province in the date of contract signature to achieve identification. Our main hypothesis is that differences in the wage growth across provinces and industries caused by the information available to the parties at the time of contract signature is associated to job losses. However, collective contracts may also specify working conditions such as overtime, working hours, number of holidays, fringe benefits or other elements that may affect firms' labor costs in ways that are not immediately discernible in wage growth. Interestingly, there are few sectors in Spain where collective contracts are negotiated at the province level but do not set province-specific wage growth. Construction is one such sector as since 2012 province-level contracts have merely adopted the wage growth set in a nationwide agreement. That is, even though there is within-province dispersion in the date of signature of its collective contracts, wage growth does not vary across provinces. Agreements with dispersion in signature dates but not in wage growth allow us to identify the impact of changes in other conditions set by collective contracts on job destruction. Moreover, this variation in signature dates permits identifying the role of confounding factors that correlate both with signature date and with job destruction.

We use the distinctive features of collective bargaining in the construction sector to provide

³⁴Almost 90% of collective contracts expire on January 1, thus resulting in lower variation than in the case we use signature dates for identification (see Figure B3 in Appendix B).

evidence in support of our identifying assumptions. To this end, we focus on workers employed in the construction sector with at least one year of tenure as of December 2008 and estimate [Equation 2](#). Figure A4 in Appendix A presents the results of this exercise. While imprecise due to the small sample size, the estimates of the incidence of non-employment due to wage rigidity are either insignificant or even negative during the entire period of analysis. The results of this case study counter the hypothesis that signature dates could correlate with job destruction for reasons unrelated to wage growth.

6.4.3 Pre-existing trends

Another potential concern about our identification strategy is pre-existing trends, i.e., the possibility that employment was already falling in the province-industry cells that reached an agreement before the onset of a recession. To test for pre-trends, we examine the probability of job loss between 2007 and 2008.³⁵ We apply the same sample selection criteria as in the benchmark specification and select workers who stayed in the firm all through 2006. We then run a placebo regression similar to [Equation 2](#) but for job losses in the period 2007-2008, i.e., prior to the 2009 recession.³⁶ The results shown in Table 4, panel c suggest no statistically significant increase in the probability of job loss in all time horizons considered.

We also verify that the parallel trends assumption is satisfied in our difference-in-differences framework, when we compare workers close and far from the minima (whose collective contracts got signed before and after the onset of the recession). To do so, we compute the workers' distance to their corresponding minimum wage -that is, the negotiated wage floor that corresponds to their skill, province, and industry as of December 2005- and run a specification similar to [Equation 3](#) for the probability of being non-employed in the period 2007-2008. Guided by the results in Table 2, we focus on the group of workers, whose wage grew most, i.e., those with pre-recession monthly wages at most 20% above the corresponding minimum. Figure 5 plots the estimated coefficients and 95% confidence intervals for all months in the period 2007-2008. There is no differential effect of wage rigidity on the incidence of non-employment among workers close to the minima prior to the fall of Lehman Brothers. This confirms that the parallel pre-trends assumption is satisfied in our setting.

7 Mechanisms and the role of institutions

The main results of our analysis point to rather small and transitory labor market effects during the 1993 recession and a persistent disemployment effect during the 2009 recession. To this end, it is important to distinguish between real and nominal wage rigidity and also consider its duration. Labor market institutions are also likely to determine the overall effect. For example, institutions may augment the pool of workers subject to wage rigidity, e.g., through escalation clauses or protect a certain segment of workers, e.g., through EPL. Moreover, labor market policies may affect the way firms respond to wage rigidity amid recessions, e.g., through

³⁵Due to data limitations before the 90's, it is not possible to check pre-trends for the 1993 recession.

³⁶The placebo regression also includes the last quarter of 2008, i.e., the months immediately after the fall of Lehman Brothers.

short-time work schemes instead of layoffs. In this section we shed light on the mechanisms underlying our main results and examine how institutions interact with wage rigidity.

7.1 Inflation rate

A plausible explanation behind the minor labor market consequences during the 1993 recession is the high inflation rate (around 4.6%) that countervailed nominal wage increases set by collective contracts. However, the 1993 recession differed from the 2009 recession also in other dimensions beyond inflation. To account for this, we exploit province-level variation in inflation rates *within* the 1993 recession, ranging between 2.7 and 5.5%. More specifically, we classify provinces as high (low)-inflation if the provincial inflation rate in 1993 was above (below) the national one and rerun Equation 2 for workers in these two groups. Figure 6 presents the results on the probability of non-employment for workers in high and low inflation provinces. We see that collective contracts signed prior to 1992 do not result in any statistically significant employment loss in high-inflation provinces (Figure 6, left panel). By contrast, in the low-inflation provinces, the probability of non-employment increases by almost 2 pp eight to nine months after the onset of the recession but also 15 and 20 to 21 months after (Figure 6, right panel). Thus, high inflation rates can effectively grease the wheels of the labor market.

7.2 Escalation clauses

Although high inflation rates can limit the effects of nominal wage rigidity, they may also make it more widespread by triggering escalation clauses. Escalation clauses typically consist of a minimum nominal wage increase guarantee for all workers covered by the collective contract. Figure B1 in Appendix B provides an example of an escalation clause in the collective contract of the food industry signed in 1992, which envisaged a minimum wage increase guarantee for 1993 equal to the realized inflation minus 1 pp.³⁷ Table 5, column 2 presents the results on actual wage growth when we restrict the sample on workers covered by escalation clauses. In the case of the 1993 recession, the estimates become much more homogeneous across groups (compare rows 1 and 2 in column 2 of panel a) while in the case of the 2009 recession the group of workers closest to the minimum wage floors continues to be the most affected one (row 1 in column 2 of panel b).³⁸ On the one hand, the 1993 recession took place during an inflationary period with an inflation rate of 4.6%. This triggered escalation clauses contained in the majority of collective contracts, which translated into higher nominal wage growth also for workers far from the minima (see Figure 7, top right panel). On the other hand, the inflation rate during the Lehman Brothers recession was very low (around 0 in 2009). Therefore, although escalation clauses were widespread even then, they had no effect in absence of inflation. As a result, in the

³⁷Escalation clauses were a common practice not only in the period around the 1993 recession but also in the years around the 2009 recession, with more than 60% of collective contracts containing one (see Tables 1a and 1b).

³⁸A test of equality of coefficients in the subsample of collective contracts with escalation clauses cannot reject the hypothesis that they are the same among workers at most 10%, 10-20% and 20-40% above the minima during the 1993 recession (Table 5, panel a, row 7). By contrast, coefficients are statistically different among workers close and far from the minima during the 2009 recession (Table 5, panel b, row 7).

case of the 2009 recession, high nominal wage growth was confined to workers very close to the minima irrespectively of the presence of escalation clauses (see Figure 7, lower right panel).³⁹

In terms of employment outcomes, as the three panels of Figure A6 in Appendix A show, they are homogeneous among workers subject to escalation clauses. This finding reconciles with the spillover effects of escalation clauses during an inflationary period. Whenever high inflation triggers escalation clauses that in turn increase the nominal wages of most workers, there is no reason to expect differential employment outcomes by distance from the minima.

7.3 Collective contract duration

One key dimension through which wage rigidity may affect employment dynamics is contract length as emphasized in Sorkin (2015). Collective contracts are typically set for more than one year, implying that a contract settling high wage growth for two or more years would increase the cost of labor much more than a contract with shorter duration that could be renegotiated shortly after the macroeconomic shock (i.e., already by the end of 2009).⁴⁰ In a context of high inflation, the average duration of the collective contracts signed before December 1992 was just 1.3 years while the average duration of contracts signed before September 2008 was 3.8 years. The lengthy duration of collective contracts may explain why employment losses during the 2009 recession were rather long-lasting.

To test the hypothesis that longer contract duration is driving the persistent employment losses after the fall of Lehman Brothers, we assume that the duration of contracts signed *prior* to the onset of the recession is exogenous.⁴¹ Hence, we generate two “treatment” groups within collective contracts signed before September 2008. The first group comprises of short contracts set to expire at the end of 2009 while the second group are long contracts that settled wage growth at least up until 2010. All the contracts signed after the onset of the recession are “controls”, in the sense that the bargaining parties could adjust wages and contract duration to the aggregate shock.

The left panel of Figure 8 shows the effects of wage rigidity on the monthly probability on non-employment among workers covered by short collective contracts (those that could be renegotiated by the end of 2009) while the right panel shows the effects among workers covered by long contracts (those that could be renegotiated only in 2010 at the earliest). There is a statistically significant increase in the probability of non-employment mainly among workers covered by long contacts. This result is also economically relevant: it implies an increase in

³⁹Another way to see this is to focus on workers with a large cushion (at least 40% above the minimum wage floors) and check whether the distribution of actual wage growth changes for those covered by collective contracts with escalation clauses. Figure A5 in Appendix A shows that the distribution of actual wage growth does not vary by the presence of escalation clauses in the 2009 recession (compare the left and right lower panels of Figure A5). Instead, during the 1993 recession, we detect more wage cuts among high cushion workers covered by escalation clauses (compare the left and right upper panels of Figure A5). Presumably, this is how firms tried to counterbalance the wage growth envisaged for all workers after the triggering of the escalation clauses.

⁴⁰Renegotiating wages during the validity period of a collective agreement would be very costly for a given firm as opting out was cumbersome at least until 2012.

⁴¹This assumption is likely to be satisfied if social partners did not foresee the recession and this is supported by the lack of pre-trends in all estimates.

the average monthly probability of non-employment of around 1% for workers covered by long collective contracts (it is almost zero for workers under shorter collective contracts).

These results imply that longer contract duration is a plausible mechanism behind the long-lasting unemployment effects of wage rigidity during the 2009 recession. Moreover, they partially explain why, in Figure 2, we find negligible rates of job destruction in 2009, the year right after the macroeconomic shock: among workers covered by collective contracts signed before the fall of Lehman Brothers, there was a fraction whose wages could be renegotiated shortly 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 two years of wage rigidity.

7.4 Adjustment costs

Another possible mechanism behind the delayed unemployment response after the fall of Lehman Brothers is the presence of adjustment costs, and firing costs are a prominent example. As it is the case in many European countries, 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.⁴² We thus examine whether the difference in firing costs across employees on fixed-term and open-ended contracts can explain the pattern of employment responses.

Figure 9 shows the effects on the monthly probability of job destruction (left panel) and non-employment (right panel). We find that, among workers covered by collective contracts signed before the fall of Lehman Brothers, the probability of job destruction is larger if their contract was fixed-term, peaking at 1.5% in 2010 (compared to 1.0% among all). This finding is consistent with previous evidence pointing at much larger churning among employees on fixed-term contracts than among other workers.

However, the increased job destruction rates among fixed-term workers do not imply larger incidence of non-employment over longer horizons. The response of non-employment to wage rigidity among workers who were easier to dismiss is less than 1% four years after the aggregate shock, very similar to the benchmark estimate in Figure 2. Actually, during 2010 and 2011, the probability of being in unemployment was still visible only among workers in open-ended contracts. One possible explanation is that firing costs in Spain take the form of large severance payments, that may sustain longer periods of job search. Alternatively, DellaVigna et al. (2017) and Koenig et al. (2016) 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. All in all, collective contract duration rather than adjustment costs are likely to explain the timing of job destruction after 2009.

⁴²In the period we analyze, severance payments amounted up to 45 wage days per year worked, with a limit of two full year wages.

7.5 Short time work schemes

The employment response to macroeconomic shocks may also depend on other labor market policies and their interplay with wage rigidity. For example, during the COVID-19 pandemic in Spain, there has been an unprecedented use of short-time work schemes (see Figure A3 in the Appendix). We first verify that collective contracts signed after the onset of the COVID-19 pandemic were less rigid than those signed before by estimating [Equation 1](#). Indeed, we find that negotiated wage growth settled for 2020 was around 0.2 pp lower among the former (see Figure 10). This result may be interpreted with caution as collective contracts that got signed during the pandemic may not be random. Interestingly, we find that the negotiated wage growth settled for 2021 was rather unaffected (See Figure A7 in the Appendix, left panel). This is consistent with the idea that unions and employer federations may have initially considered the pandemic to be a rather transitory shock. However, there was a lot of uncertainty, which is reflected in the reduced average duration of the newly signed collective contracts (Figure A7 in the Appendix, right panel).

To study the effect of wage rigidity not only on the probability of non-employment but also on the probability of short time work, we use available data from the Spanish labor force survey and a specification similar to [Equation 2](#) –see Appendix C for more details. Figure 11, left panel shows that workers covered by collective contracts, which got signed after 2020q1 had a lower probability of short time work. Instead, there is no statistically significant effect on the probability of non-employment (Figure 11, right panel). Our findings suggest that wage rigidity during the COVID-19 pandemic led to employment adjustments at the intensive rather than the extensive margin.

8 Conclusions

A large literature has estimated the extent and relevance of wage rigidity in different economies as well as the degree of wage cyclicality. However, it is difficult to assess empirically under which conditions wage rigidities have real effects, that is, whether 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 numerous 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 that got signed at different moments in time and could react differently to large aggregate shocks. By combining information on the exact dates of signature and expiration of collectively bargained agreements we find that agreements signed after the onset of the 1993 and 2009 recessions settled for a 1.0-1.5 pp lower nominal wage growth than the agreements signed before. By exploiting variation in the renewal of collective contracts and leveraging the distribution of the worker-level bite of minimum wage floors in longitudinal Social Security records, we find that nominal wage rigidity can have severe employment consequences only in recessionary periods of low inflation. Job losses in the high-inflation recession of 1993 are rather limited and short-

lived as real wages effectively decline. In the low-inflation recession of 2009 instead, job losses are highly persistent and entirely driven by workers with wages close to the minimum wage floors. All together, the evidence is consistent with the notion that contract staggering during low-inflation recessionary periods can constitute an amplifier of employment fluctuations, consistent with the macroeconomic models of Gertler and Trigari (2009) and Olivei and Tenreyro (2007, 2010), among others.

The evidence is relevant for the policy debate. First, our estimates suggest that the magnitude of wage increases among job stayers is similar to the percent time out of employment among job losers in the aftermath of the 2009 recession. An interpretation is that, once one takes flows into non-employment into account, minimum wages in collective contracts may have not helped in maintaining employees' earnings constant during that recession. Second, the particular form of real wage rigidity resulting from the automatic extension of provincial agreements and multi-period bargaining played an important role on the employment destruction during 2009 recession while high inflation rates counterbalanced nominal wage rigidity during the 1993 recession. In that sense, an assessment of the employment impacts of collective contracts crucially depends on inflation rates, the distribution of wage cushions, and contract duration. These factors may explain the wide range of estimated elasticities reported in the literature. Third, in presence of particular policies (i.e., short time work schemes) wage rigidity during the COVID-19 pandemic was accompanied mostly by adjustments in the intensive rather than the extensive margin of labor. The role of downward wage rigidity on employment is key to assess the future implications of subsequent labor reforms in Spain and, more generally, to understand how the labor market reacts to economic shocks and the type of mitigating policies that should be implemented. These topics are left for future research.

References

Adamopoulou, Effrosyni and Ernesto Villanueva (2022a): "The Bite of Collective Contracts in Italy and Spain," *Labour Economics*, Vol. 76, 102147.

Adamopoulou, Effrosyni and Ernesto Villanueva (2022b): "Employment and Wage Effects of Extending Collective Bargaining Agreements," *IZA World of Labor*, 136.

Altonji, Joseph G. and Paul J. Devereux (1999): "The Extent and Consequences of Downward Nominal Wage Rigidity," NBER Working Paper No. w7236.

Baker, Andrew C., David F. Larcker and Charles C.Y. Wang (2022): "How much Should We Trust Staggered Difference-in-Differences Estimates?," *Journal of Financial Economics*, Vol. 144(2), 370-395.

Barceló, Cristina and Ernesto Villanueva (2016): "The Response of Household Wealth to the Risk of Job Loss: Evidence from Differences in Severance Payments," *Labour Economics*, Vol. 39, 35-54.

Barwell, Richard and Mark Sweitzer (2007): "The Incidence of Nominal and Real Wage Rigidities in Great Britain: 1978–98," *The Economic Journal*, Vol. 117(524), F553–F569.

Bentolila, Samuel and Juan J. Dolado (1994): "Labour Flexibility and Wages: Lessons from Spain," *Economic Policy*, Vol. 18, 54-99.

Bentolila, Samuel, Mario Izquierdo and Juan F. Jimeno (2010): "Negociación colectiva: La gran reforma pendiente," *Papeles de Economía Española* 124.

Björklund, Maria, Mikael Carlsson, and Oskar Nordström Skans (2019): "Fixed-Wage Contracts and Monetary Non-neutrality," *American Economic Journal: Macroeconomics*, Vol. 11, 171-92.

Blanco, Andres, Andres Drenik, Christian Moser and Emilio Zaratiegui (2023): "A Theory of Non-Coasean Labor Markets", CEPR Discussion Paper no 17808.

Bonhomme, Stephane and Laura Hospido (2017): "The Cycle of Earnings Inequality: Evidence from Spanish Social Security Data," *Economic Journal*, Vol. 127, 1244-1278.

Boeri, Tito and Bruecker Herbert (2011): "Short-time Work Benefits Revisited: Some Lessons from the Great Recession", *Economic Policy*, Vol. 26, 697-765.

Broer, Tobias, Karl Harmenberg, Per Krusell, and Erik Öberg (2022): "Macroeconomic Dynamics with Rigid Wage Contracts," *American Economic Review: Insights*, forthcoming.

Bhuller, Manudeep, Karl Ove Moene, Magne Mogstad, and Ola L. Vestad (2022): "Facts and Fantasies about Wage Setting and Collective Bargaining", *Journal of Economic Perspectives*, Vol. 36(4), 29-52.

Cahuc, Pierre and Andre Zylberberg (2004): *Labor Economics*. Cambridge: The MIT Press.

Callaway, Brantly and Pedro H.C. Sant'Anna (2021): "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics*, Vol. 225(2), 200-230.

Calmforms, Lars and John Drifill (1988): "Bargaining Structure, Corporatism and Macroeconomic Performance," *Economic Policy*, Vol. 3, 13-61.

Caloia, Francesco, Jante Parlevliet and Mauro Mastrogiamomo (2021): "Staggered Wages, Unanticipated Shocks and Firm Adjustments", DNB Working Paper no 711.

Card, David (1990): "Unexpected Inflation, Real Wages, and Employment Determination in Union Contracts," *American Economic Review*, Vol. 80(4), 669-688.

Card, David and Ana Rute Cardoso (2022): "Wage Flexibility under Sectoral Bargaining," *Journal of the European Economic Association*, Vol. 20(5), 2062-2097.

Card, David and Dean Hyslop (1997): "Does Inflation Grease the Wheels of the Labor Market?," in Christina D. Romer and David H. Romer, eds., *Reducing Inflation: Motivation and Strategy*, University of Chicago Press, 71-122.

Card, David and Sara de la Rica (2006): "Firm-level Contracting and the Structure of Wages in Spain," *Industrial and Labor Relations Review*, Vol. 59(4), 73-592.

Cardoso, Ana Rute and Pedro Portugal (2005): "Contractual Wages and the Wage Cushion under Different Bargaining Settings," *Journal of Labor Economics*, Vol. 23, 875-902.

Devicienti, Francesco, Ana Maida and Paolo Sestito (2007): "Downward Wage Rigidity in Italy: Micro-Based Measures and Implications," *The Economic Journal* Vol. 117(524), 530-552.

Danziger, Leif and Shoshana Neuman (2005) "Delays in Labor Contracts: Theory and Evidence," *Journal of Labor Economics*, Vol. 23(2), 341-371.

De la Roca, Jorge (2014): "Real Wages and Business Cycles: Evidence from Spain using Social Security Data," *SERIEs*, Vol. 5, 173-195.

DellaVigna, Stefano, Attila Lindner, Balázs Reizer, and Johannes F. Schmieder (2017): "Reference-Dependent Job Search: Evidence from Hungary", *Quarterly Journal of Economics*, Vol. 132(4), 1969-2018.

Dickens, William, Lorenz Goette, Erica Groshen, Steinar Holden, Julian Messina, Mark Schweitzer, Jarkko Turunen and Melanie E. Ward (2007): "How Wages Change: Micro Evidence from the International Wage Flexibility Project," *Journal of Economic Perspectives*, Vol 21(2), 195-214.

Dolado, Juan J. , Florentino Felgueroso, and Juan F. Jimeno (1997), "The Effects of Minimum Bargained Wages on Earnings: Evidence from Spain," *European Economic Review*, Vol. 41, 713-721.

Du Caju, Philip, Erwan Gautier, Daphne Momferatou and Melanie Ward-Warmedinger (2008): "Institutional Features of Collective Bargaining in 23 European Countries, Japan and the US," ECB Working Paper number 974.

Elsby, Michael (2009): "Evaluating the Economic Significance of Downward Nominal Wage Rigidity," *Journal of Monetary Economics*, Vol. 56(2), 154-169.

Elsby, Michael W., Donggyun Shin, and Gary Solon (2016): "Wage Adjustment in the Great Recession and Other Downturns: Evidence from the United States and Great Britain," *Journal of Labor Economics*, Vol. 34(S1), S249-S291.

Faia, Ester and Vincenzo Pezone (2020): "The Heterogeneous Cost of Wage Rigidity: Evidence and Theory", SAFE Working Paper No. 242.

Fanfani, Bernardo (2020): "The Employment Effects of Collective Bargaining", Università Cattolica del Sacro Cuore Working paper No. 95.

Fehr, Ernst and Lorenz Goette (2005): "Robustness and Real Consequences of Nominal Wage Rigidity," *Journal of Monetary Economics*, Vol. 52, 779-804.

Gautier, Erwan, Sebastien Roux and Milena Suarez Castillo (2022): "How Do Wage Setting

Institutions Affect Wage Rigidity? Evidence from French Micro Data," *Labour Economics*, Vol. 78, 102232.

Gertler, Mark and Antonella Trigari (2009): "Unemployment Fluctuations with Staggered Nash Wage Bargaining," *Journal of Political Economy*, Vol. 117(1), 38-86.

Gertler, Mark., Christopher Huckfeldt, and Antonella Trigari (2020): "Unemployment Fluctuations, Match Quality and the Wage Cyclicalilty of New Hires," *Review of Economic Studies*, Vol. 87, 1876-1914.

Guimaraes, Paulo, Fernando Martins and Pedro Portugal (2017): "Upward Nominal Wage Rigidity," Working Papers w201702, Banco de Portugal.

Haefke, Christian, Marcus Sonntag and Thijs van Rens (2013): "Wage Rigidity and Job Creation," *Journal of Monetary Economics*, Vol. 60, 887 - 899.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G Sullivan (1993): "Earnings Losses of Displaced Workers," *American Economic Review*, Vol. 83, 685-709.

Koenig, Felix, Alan Manning and Barbara Petrongolo (2016): "Reservation Wages and the Wage Cyclicalilty Puzzle," IZA Discussion Paper No. 9717.

Konle-Seidl, Regina (2020): "Short-time Work in Europe: Rescue in the Current COVID-19 Crisis?",

Jimeno, Juan F. and Carlos Thomas (2013): "Collective Bargaining, Firm Heterogeneity and Unemployment," *European Economic Review*, Vol. 59, 63-79.

Layard, Richard, Stephen Nickell and Richard Jackman (1991): *Unemployment: Macroeconomic Performance and the Labour Market*. Oxford University Press.

Martins, Pedro (2021): "30.000 minimum wages: The Economic Effects of Collective Bargaining Extensions," *British Journal of Industrial Relations*, 59:2, 335-369.

Martins, Pedro, Gary Solon and Jonathan Thomas (2012): "Measuring What Employers Do about Entry Wages over the Business Cycle: A New Approach", *American Economic Journal: Macroeconomics*, Vol. 4(4), 36-55.

Ministerio de Trabajo y Asuntos Sociales (2008): "Guía de la Negociación Colectiva".

Ministerio de Trabajo y Asuntos Sociales (2012): "Guía de la Negociación Colectiva".

OECD Employment Outlook 2012.

OECD Employment Outlook 2013.

OECD (2019). *Negotiating OurWay Up: Collective Bargaining in a ChangingWorld of Work*, OECD Publishing.

Olivei, Giovanni and Silvana Tenreyro (2007): "The Timing of Monetary Policy Shocks," *American Economic Review*, Vol. 97(3), 636-663.

Olivei, Giovanni and Silvana Tenreyro (2010): "Wage Setting Patterns and Monetary Policy: International Evidence," *Journal of Monetary Economics*, Vol. 57(7), 785-802.

Pischke, Jörn-Steffen (2018): "Wage Flexibility and Employment Fluctuations: Evidence from the Housing Sector," *Economica*, Vol. 85, 407-427.

Sorkin, Isaac (2015): "Are There Long-run Effects of the Minimum Wage?," *Review of Economic Dynamics*, Vol. 18, 306-333.

Stüber, Heiko (2017): "The Real Wage Cyclicalilty of Newly Hired and Incumbent Workers in Germany," *Economic Journal*, Vol. 127, 522-546.

Taylor, John B. (1980): "Aggregate Dynamics and Staggered Contracts," *Journal of Political Economy*, Vol. 88(1), 1–23.

Tobin, James (1972): "Inflation and Unemployment," *American Economic Review*, Vol. 62, 1-18.

Visser, Jelle (2013): "Wage Bargaining Institutions: from Crisis to Crisis," *European Commission Economic Papers* No. 488.

Yagan, Danny (2019): "Employment Hysteresis from the Great Recession," *Journal of Political Economy*, Vol. 127(5), 2505-2558.

Tables

Table 1a. Descriptive statistics: Sample means (standard deviations)-1993 recession

Characteristic	All	Contract signed pre 1992m12	Contract signed post 1992m12	Diff. net of prov., sec. FE
	(1)	(2)	(3)	(4)
Sectoral distribution				
Agriculture, manufacturing & utilities (%)	18.5	18.9	18.2	-
Construction (%)	21.6	22.4	21.0	-
Trade, Food & Accomodation (%)	32.5	40.1	27.8	-
Transportation (%)	6.2	6.6	5.9	-
Finance and real estate (%)	0.20	0.0	0.27	-
Services to businesses, health & educ. (%)	20.0	11.6	25.2	-
Collective contract characteristics				
Multi-year (%)	59.5	98.2	35.3	59.1***
Collective contract duration (in years)	1.43	1.53	1.37	0.17***
	(0.61)	(0.62)	(0.59)	(0.06)
Escalation clause (%)	62.0	64.6	60.5	6.96
Negotiated wage growth for 1993 (%)	5.20	6.35	4.49	1.79***
	(2.23)	(1.25)	(2.40)	(0.25)
Worker characteristics				
Age (in years)	37.4	37.4	37.4	-0.14
	(11.0)	(11.0)	(11.0)	(0.16)
Female (%)	29.2	26.6	30.9	-2.08***
College, managers (%)	15.2	14.2	15.8	-0.19
White collar workers (%)	30.6	29.0	31.6	0.04
Blue collar workers(%)	54.2	56.8	52.6	0.14
Fixed-term contract (%)	n.a.	n.a.	n.a.	n.a.
N	73,183	28,154	45,029	73,183

Notes: Sample of workers' Social Security records linked to their corresponding provincial collective contract. All worker characteristics refer to December 1991. Escalation clause takes value one if the contract stipulates an adjustment for realized inflation whenever it exceeds a threshold level. Column (4) reports the OLS coefficients of a regression of differences between columns (2) and (3), net of province and sector FE.

Table 1b. Descriptive statistics: Sample means (standard deviations)-2009 recession

Characteristic	All	Contract signed pre 2008m9	Contract signed post 2008m9	Diff. net of prov., sec. FE
	(1)	(2)	(3)	(4)
Sectoral distribution				
Agriculture, manufacturing & utilities (%)	12.6	14.7	11.6	-
Construction (%)	23.0	38.1	10.9	-
Trade, Food & Accomodation (%)	32.7	30.3	34.8	-
Transportation (%)	4.6	4.6	4.7	-
Finance and real estate (%)	0.18	0.3	0.0	-
Services to businesses, health & educ. (%)	26.3	11.4	37.9	-
Collective contract characteristics				
Multi-year (%)	88.2	100	75.1	23.1***
Collective contract duration (in years)	3.29	3.74	2.95	0.95***
	(1.22)	(1.02)	(1.26)	(0.20)
Escalation clause (%)	64.3	82.2	44.2	20.3***
Negotiated wage growth for 2009 (%)	2.09	3.09	1.31	1.29***
	(1.51)	(0.97)	(1.39)	(0.19)
Negotiated wage growth for 2009-2010 (%)	3.88	4.67	3.27	0.99***
	(1.85)	(1.74)	(1.70)	(0.23)
Worker characteristics				
Age (in years)	37.3	37.3	37.2	-0.11
	(10.7)	(10.7)	(10.4)	(0.11)
Female (%)	41.3	33.7	47.4	-1.47*
College, managers (%)	14.8	11.7	17.2	0.10
White collar workers (%)	35.0	28.5	40.1	-1.32**
Blue collar workers(%)	50.1	59.6	42.6	1.21
Fixed-term contract (%)	14.0	16.7	12.2	0.84
N	99,735	43,641	56,094	99,735

Notes: Sample of workers' Social Security records linked to their corresponding provincial collective contract. All worker characteristics refer to December 2007. Escalation clause takes value one if the contract stipulates an adjustment for realized inflation whenever it exceeds a threshold level. Column (4) reports the OLS coefficients of a regression of differences between columns (2) and (3), net of province and sector FE.

Table 2. Effects on actual wage growth, heterogeneity by distance to the minima

a. 1993 recession		
	Dep. Var.: $\Delta \log(wage)_{1993}$	
	(1)	(2)
Contract signed before 1992m12*	0.014**	0.016**
($W_{\min} < W_{1991m12} \leq 1.1 * W_{\min}$)	(0.006)	(0.007)
Contract signed before 1992m12*	0.008*	0.011*
($1.1 * W_{\min} < W_{1991m12} \leq 1.2 * W_{\min}$)	(0.005)	(0.006)
Contract signed before 1992m12*	0.003	0.004
($1.2 * W_{\min} < W_{1991m12} \leq 1.4 * W_{\min}$)	(0.005)	(0.006)
Constant	0.015***	0.012***
	(0.004)	(0.003)
FE	Industry	Collective contract
N of observations (contracts)	7,911	7,911 (463)
Adj. R ²	0.030	0.035
b. 2009 recession		
	Dep. Var.: $\Delta \log(wage)_{2009}$	
	(1)	(2)
	All contracts	All contracts
Contract signed before 2008m9*	0.019**	0.023***
($W_{\min} < W_{2007m12} \leq 1.1 * W_{\min}$)	(0.007)	(0.007)
Contract signed before 2008m9*	0.016**	0.017***
($1.1 * W_{\min} < W_{2007m12} \leq 1.2 * W_{\min}$)	(0.006)	(0.007)
Contract signed before 2008m9*	0.001	0.001
($1.2 * W_{\min} < W_{2007m12} \leq 1.4 * W_{\min}$)	(0.005)	(0.005)
Constant	-0.009**	-0.007**
	(0.004)	(0.004)
FE	Industry	Collective contract
N of observations (contracts)	21,514	21,514 (528)
R ²	0.013	0.017

Notes: *** p<0.01, ** p<0.05. * p<0.10. Robust s.e. clustered at the 3-digit industry \times province level. The dependent variable is nominal wage growth in 1993 (panel a) or in 2009 (panel b). Sample restricted to full time employees with available information on their corresponding minimum wage floor, who stayed in the same firm all through 1993 (panel a) or 2009 (panel b). The regressions in column (1) include province and 3-digit industry fixed effects as well as intercepts for $W_{\min} < W_{1991m12} < 1.1 * W_{\min}$, $1.1 * W_{\min} < W_{1991m12} < 1.2 * W_{\min}$ and $1.2 * W_{\min} < W_{1991m12} < 1.4 * W_{\min}$ and in col. (2) collective contract fixed effects. Omitted category: "(Collective Contract signed before 1992m12)*(1.4*Wmin<W1991m12)" in panel a and "(Collective Contract signed before 2008m9)*(1.4*W2007m12<Wmin)" in panel b. Additional controls: gender and age dummies.

Table 3. Benchmark estimates of job loss probability

a. Benchmark 1993 recession			
Dep. var.: Prob(Job loss_9394 1 year tenure in 1992m12)			
	(1)	(2)	(3)
Months since 1993m1	3	9	21
Contract signed before 1992m12	0.005***	0.003	0.000
	(0.002)	(0.005)	(0.002)
N	65,805	54,912	41,419
Adj. R ²	0.010	0.013	0.006
b. Benchmark 2009 recession			
Dep. var.: Prob(Job loss_0910 1 year tenure in 2008m12)			
	(1)	(2)	(3)
Months since 2009m1	3	9	21
Contract signed before 2008m9	-0.000	-0.001	0.003**
	(0.001)	(0.001)	(0.001)
N	96,150	85,357	70,528
Adj. R ²	0.011	0.005	0.005

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust s.e. clustered at the 3-digit industry × province level.

Separate monthly regressions of the probability of non-employment 3, 9 and 21 months after 1993m1 (panel a) or 2009m1 (panel b). Sample restricted to workers with at least one year of tenure as of 1992m12 (panel a) or 2008m12 (panel b). All regressions control for month-specific three-digit industry and month-specific province dummies.

Table 4. Benchmark estimates of unemployment chances and parallel trends

a. Benchmark 1993 recession				
Dep. var.: Prob(Unemp_9394 1 year tenure in 1992m12)				
	(1)	(2)	(3)	(4)
Months since 1993m1	9	12	24	48
Contract signed before 1992m12	0.010 (0.006)	-0.001 (0.005)	-0.002 (0.005)	-
N	72,688	72,688	72,457	-
Adj. R ²	0.047	0.042	0.048	-
b. Benchmark 2009 recession				
Dep. var.: Prob(Unemp_0910 1 year tenure in 2008m12)				
	(1)	(2)	(3)	(4)
Months since 2009m1	9	12	24	48
Contract signed before 2008m9	0.000 (0.003)	0.004 (0.003)	0.010** (0.004)	0.011** (0.005)
N	101,828	101,818	101,784	98,336
Adj. R ²	0.052	0.057	0.061	0.073
c. Placebo				
Dep. var.: Prob(Unemp_0708 1 year tenure in 2006m12)				
	(1)	(2)	(3)	(4)
Months since 2007m1	9	12	24	48
Contract signed before 2008m9	-0.001 (0.002)	-0.000 (0.002)	0.002 (0.003)	-
N	91,054	91,055	90,354	-
Adj. R ²	0.015	0.016	0.047	-

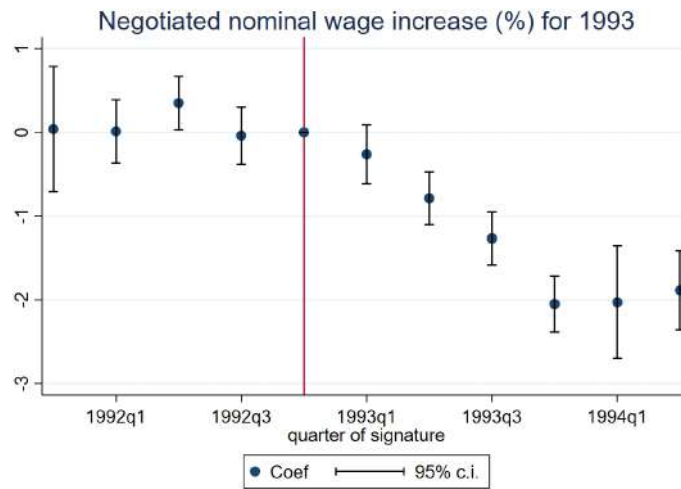
Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust s.e. clustered at the 3-digit industry \times province level. Separate monthly regressions of the probability of non-employment 9, 12 and 24 months after 1993m1 (panel a) or 9, 12, 24 and 48 months after 2009m1 (panel b) or 9, 12 and 24 months after 2007m1 (panel c). Sample restricted to workers with at least one year of tenure as of 1992m12 (panel a) or 2008m12 (panel b) or 2006m12 (panel c). All regressions control for month-specific three-digit industry and month-specific province dummies.

Table 5. Effects on actual wage growth, heterogeneity by presence of escalation clauses

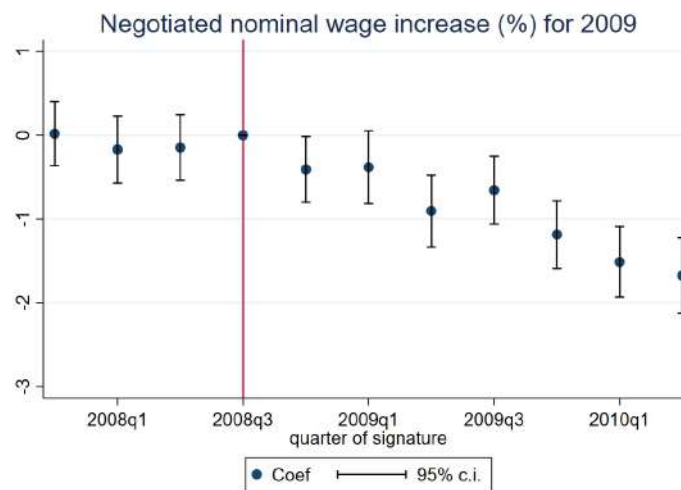
a. 1993 recession		
	Dep. Var.: $\Delta \log(wage)_{1993}$	
	(1)	(2)
	All contracts	Contracts with escalation clauses
Contract signed before 1992m12* ($W_{\min} < W_{1991m12} \leq 1.1 * W_{\min}$)	0.016** (0.007)	0.009 (0.008)
Contract signed before 1992m12* ($1.1 * W_{\min} < W_{1991m12} \leq 1.2 * W_{\min}$)	0.011* (0.006)	0.012* (0.007)
Contract signed before 1992m12* ($1.2 * W_{\min} < W_{1991m12} \leq 1.4 * W_{\min}$)	0.004 (0.006)	0.003 (0.007)
Test of equality of coefficients: Prob>F		0.221
FE	Collective contract	Collective contract
N of observations (contracts)	7,911 (463)	5,223 (273)
Adj. R ²	0.035	0.026
b. 2009 recession		
	Dep. Var.: $\Delta \log(wage)_{2009}$	
	(1)	(2)
	All contracts	Contracts with escalation clauses
Contract signed before 2008m9* ($W_{\min} < W_{2007m12} \leq 1.1 * W_{\min}$)	0.023*** (0.007)	0.024*** (0.009)
Contract signed before 2008m9* ($1.1 * W_{\min} < W_{2007m12} \leq 1.2 * W_{\min}$)	0.017*** (0.007)	0.015* (0.008)
Contract signed before 2008m9* ($1.2 * W_{\min} < W_{2007m12} \leq 1.4 * W_{\min}$)	0.001 (0.005)	0.001 (0.006)
Test of equality of coefficients: Prob>F		0.007
FE	Collective contract	Collective contract
N of observations (contracts)	21,514 (528)	12,799 (316)
R ²	0.017	0.017

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust s.e. clustered at the 3-digit industry \times province level. The dependent variable is nominal wage growth in 1993 (panel a) or in 2009 (panel b). Sample restricted to full time employees with available information on their corresponding minimum wage floor, who stayed in the same firm all through 1993 (panel a) or 2009 (panel b). All regressions include collective contract fixed effects. The sample in col. (2) only includes workers covered by collective contracts with escalation clauses. Omitted category: "(Collective Contract signed before 1992m12)*(1.4*Wmin <W1991m12)" in panel a and "(Collective Contract signed before 2008m9)*(1.4*W2007m12<Wmin)" in panel b. Additional controls: gender and age dummies.

Figures



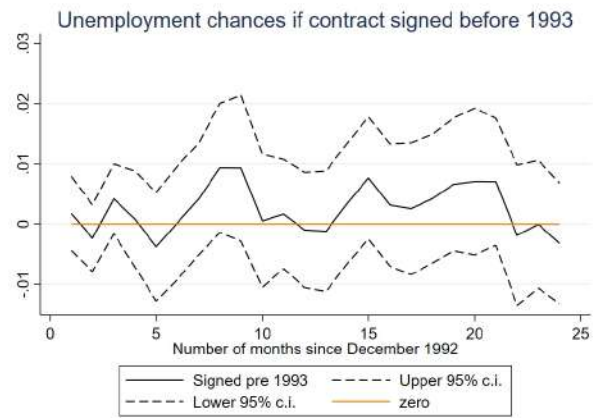
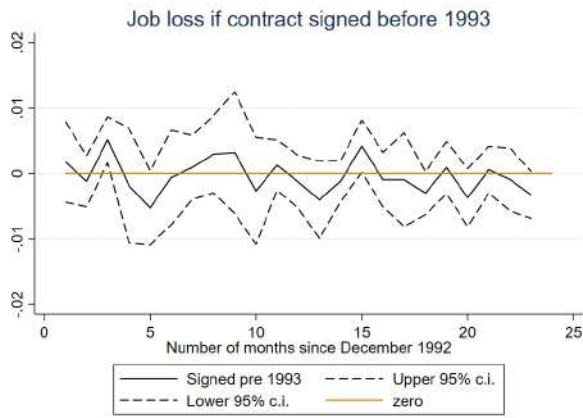
a) 1993 recession (0=1992q4)



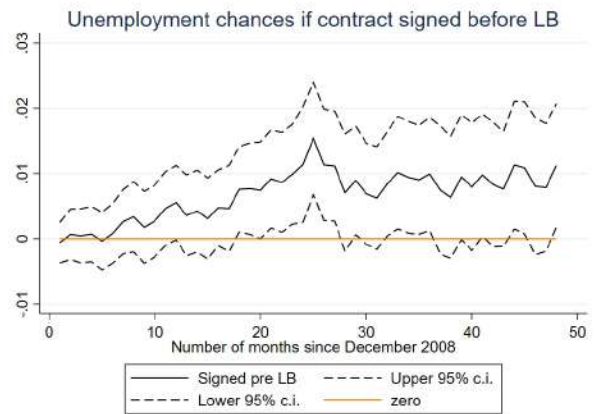
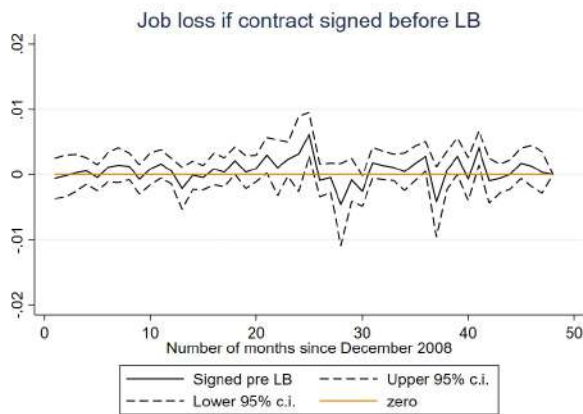
b) Lehman Brothers (0=2008q3)

Figure 1. Negotiated wage growth by quarter of signature before and after the onset of two recessions

Notes: The Figures show the OLS estimates and 95% confidence intervals of quarter of signature on average nominal wage growth set by collective contracts. Contracts signed in 1994 for the 1993 recession (panel a) and in 2010 for the Lehman brothers recession (panel b) were signed with a delay and result in wage increases ex-post (not observed as of 1993 and 2009). All regressions control for province and three-digit industry fixed effects. Robust s.e. clustered at the 3-digit industry \times province level.



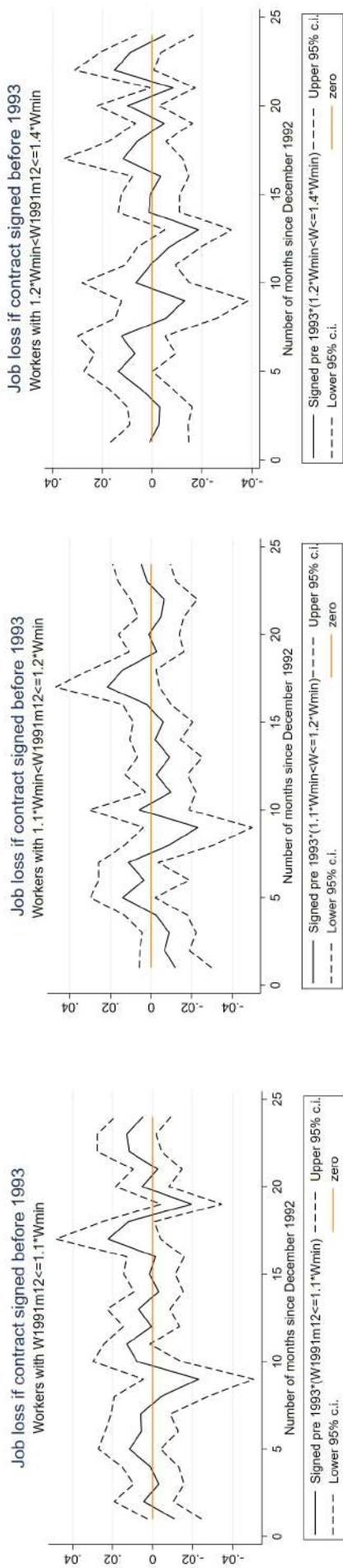
a) 1993 recession (0=1992m12)



b) 2009 recession (0=2008m12)

Figure 2. Job loss and subsequent employment outcomes among workers covered by a province-industry agreement

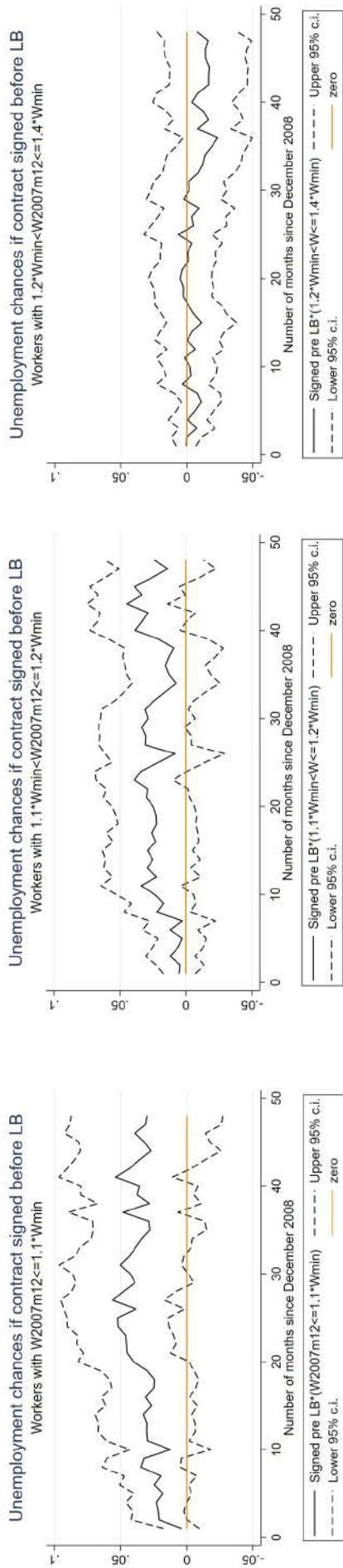
Notes: The Figures on the left plot the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before 1992m12 (panel a) or 2008m09 (panel b)" in separate monthly regressions with an indicator of job loss in each subsequent month. The Figures on the right plot the same regressions but using as a dependent variable the probability of non-employment among individuals who were working in the firm as of 1992m12 (panel a) or 2008m12 (panel b). All regressions control for month-specific three-digit industry and month-specific province dummies. Robust s.e. clustered at the 3-digit industry \times province level.



1993 recession (0=1992m12)

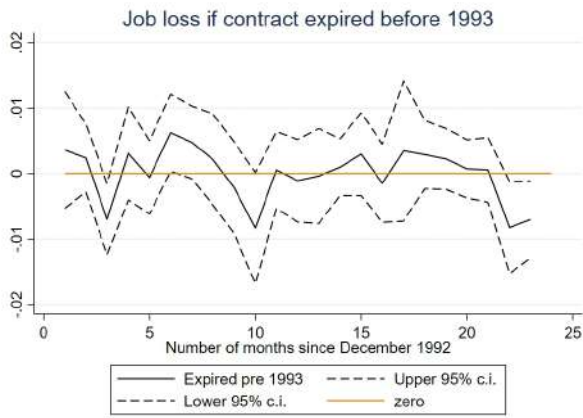
Figure 3a. Job loss among workers covered by a province-industry agreement, by distance from the minimum wages

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the interaction term $(Collective\ Contract\ signed\ before\ 1992m12) * (Wmin \leq W1991m12 \leq 1.1 * Wmin)$ in separate monthly regressions. The dependent variable is the monthly probability of job loss among individuals who were working in the firm as of 1992m12. The estimates refer to workers whose pre-recession earnings were at most 10% above the minimum wages. The Figure in the center plots the OLS estimates and 95% confidence intervals of the interaction term $(Collective\ Contract\ signed\ before\ 1992m12) * (1.1 * Wmin \leq W1991m12 \leq 1.2 * Wmin)$ from the same regressions as above but refer to workers whose pre-recession earnings were 10-20% above the minimum wages. The Figure on the right plot the OLS estimates and 95% confidence intervals of the interaction term $(Collective\ Contract\ signed\ before\ 1992m12) * (Wmin < W1991m12 <= 1.2 * Wmin)$ from the same regressions as above but refer to workers whose pre-recession earnings were 20-40% above the minimum wages. All regressions control for collective contract fixed effects. Omitted category: $(Collective\ Contract\ signed\ before\ 1992m12) * (1.4 * Wmin < W1991m12)$. Minimum wages are specific of each occupation (10) and province. Robust s.e. clustered at the 3-digit industry X province level.

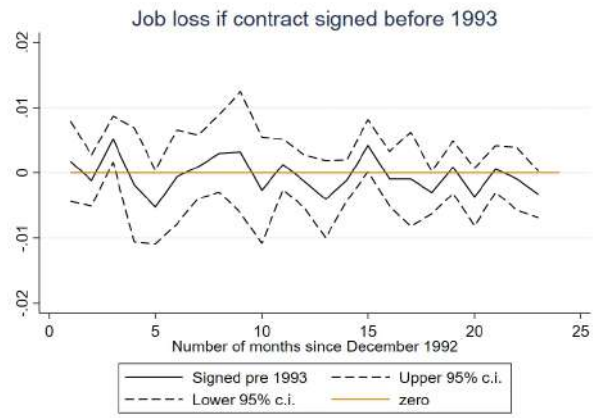


2009 recession (0=2008m12)
Figure 3b. Unemployment changes among workers covered by a province-industry agreement, by distance from the minimum wages

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the interaction term $\beta(\text{Collective Contract signed before } 2008m9) * (W_{min} \leq W_{2007m12} \leq 1.1 * W_{min})$ in separate monthly regressions. The dependent variable is the monthly probability of non-employment among individuals who were working in the firm as of 2008m12. The estimates refer to workers whose pre-recession earnings were at most 10% above the minimum wages. The Figure in the center plots the OLS estimates and 95% confidence intervals of the interaction term $\beta(\text{Collective Contract signed before } 2008m9) * (1.1 * W_{min} < W_{2005m12} \leq 1.2 * W_{min})$ from the same regressions as above but refer to workers whose pre-recession earnings were 10-20% above the minimum wages. The Figure on the right plots the OLS estimates and 95% confidence intervals of the interaction term $\beta(\text{Collective Contract signed before } 2008m9) * (W_{min} < W_{2005m12} \leq 1.2 * W_{min})$ from the same regressions as above but refer to workers whose pre-recession earnings were 20-40% above the minimum wages. All regressions control for collective contract fixed effects. Omitted category: $\beta(\text{Collective Contract signed before } 2008m9) * (1.4 * W_{min} < W_{2005m12})$. Minimum wages are specific of each occupation (10) and province. Robust s.e. clustered at the 3-digit industry \times province level.



a) Expiration date

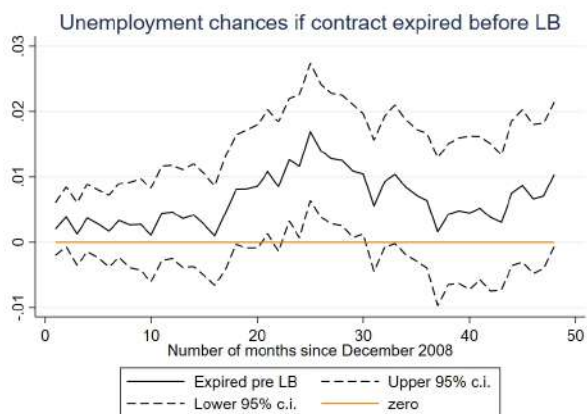


b) Signature date

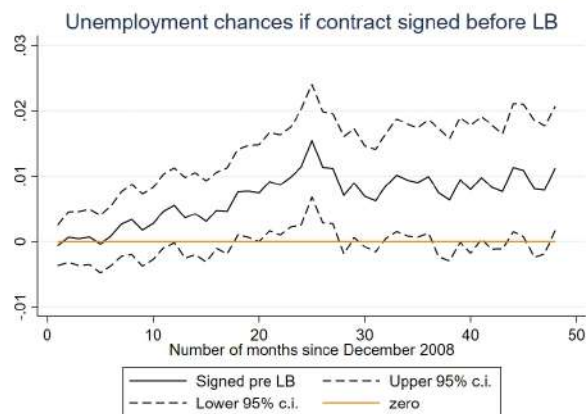
1993 recession (0=1992m12)

Figure 4a. Job destruction among workers covered by province-industry agreements
Robustness using the month of collective contract expiration versus benchmark using
the month of collective contract signature

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract expired before 1993" in 24 separate monthly regressions with an indicator of job loss in each subsequent month. The Figure on the right plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before 1993" in separate monthly regressions with an indicator of job loss in each subsequent month. All regressions control for month-specific three-digit industry and month-specific province dummies. Robust s.e. clustered at the 3-digit industry \times province level.



a) Expiration date



b) Signature date

2009 recession (0=2008m12)

Figure 4b. Unemployment chances among workers covered by province-industry agreements
Robustness using the month of collective contract expiration versus benchmark using
the month of collective contract signature

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract expired before 2008" in 48 separate monthly regressions of the probability of non-employment among individuals who were working in the firm as of 2008m12. The Figure on the right plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before 2008" in separate monthly regressions of the probability of non-employment among individuals who were working in the firm as of 2008m12. All regressions control for month-specific three-digit industry and month-specific province dummies. Robust s.e. clustered at the 3-digit industry \times province level.

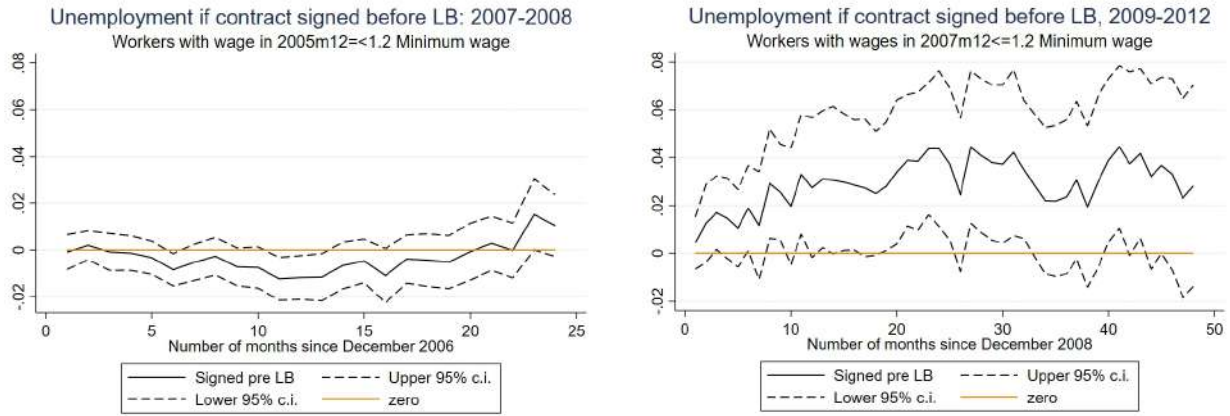
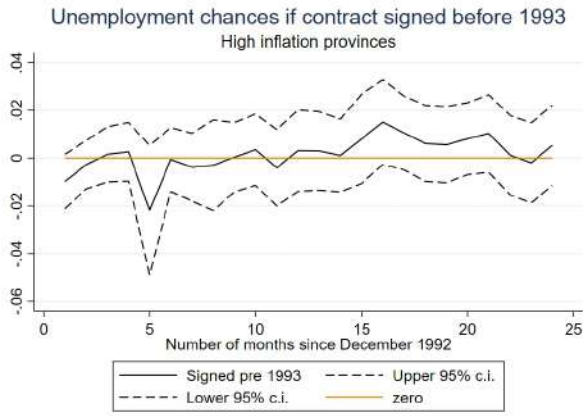
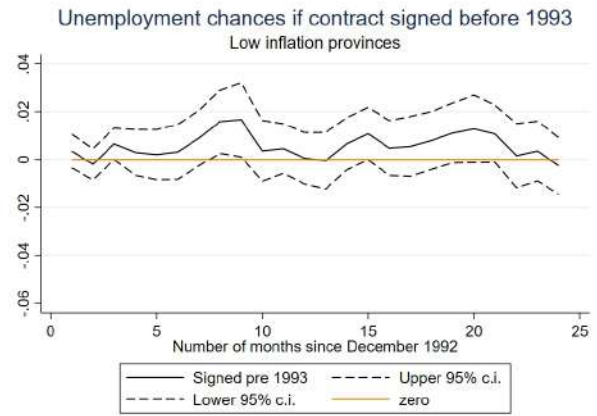


Figure 5. Parallel trends vs benchmark, workers close to the minima
(0=2006m12)

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the interaction term "(Collective Contract signed before 2008m9)*(Wmin<=W2005m12<=1.2*Wmin)" in 24 separate regressions. The dependent variable is the probability of non employment among individuals who were working in the firm as of 2006m12. The estimates refer to workers whose earnings in 2005m12 were at most 20% above the minimum wages. The Figure on the right plots the OLS estimates and 95% confidence intervals of the interaction term "(Collective Contract signed before 2008m9)*(Wmin<=W2007m12<=1.2*Wmin)" in 48 separate regressions. The estimates refer to workers whose earnings in 2007m12 were at most 20% above the minimum wages. The regressions control for collective contract and group fixed effects and include the interaction term "(Collective Contract signed before 2008m9)*(1.2*Wmin<W2005m12<=1.4*Wmin)". Omitted category: "(Collective Contract signed before 2008m9)*(1.4*Wmin<W2005m12)". Minimum wages are specific of each occupation (10) and province. Robust s.e. clustered at the 3-digit industry \times province level.



a) High inflation in 1993

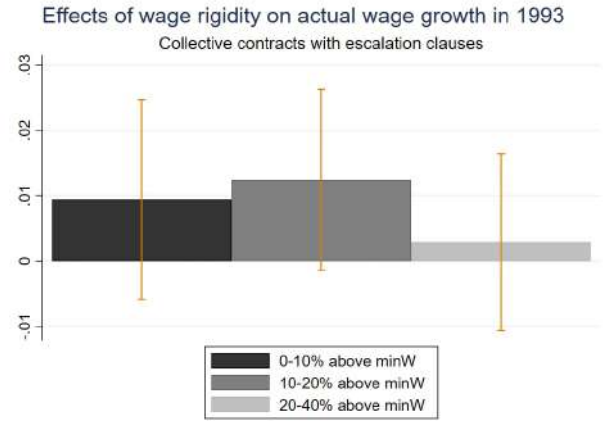
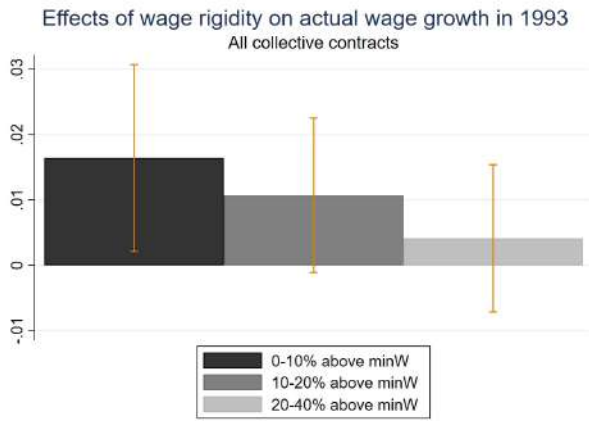


b) Low inflation in 1993

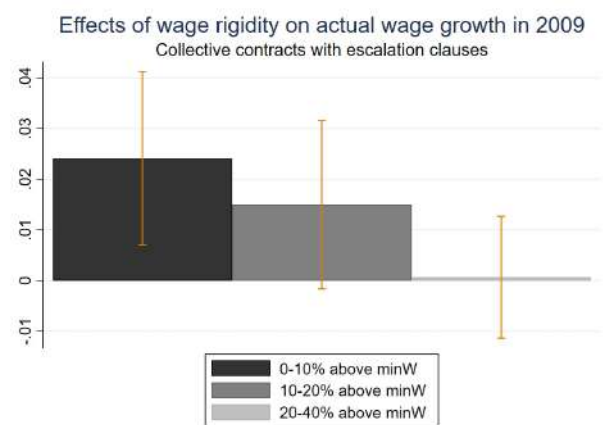
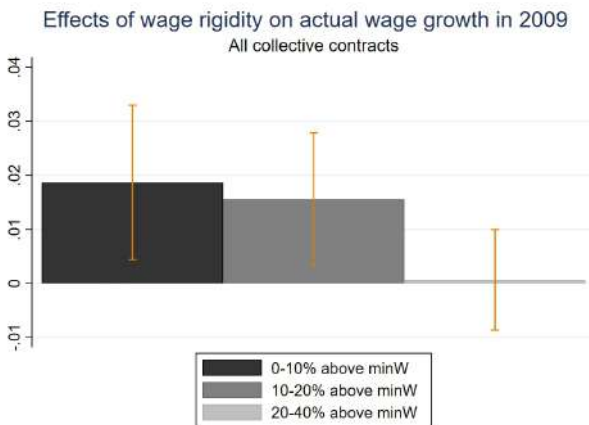
1993 recession (0=1992m12)

Figure 6. Employment outcomes among workers covered by a province-industry agreement, by level of inflation

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before September 2008" in 24 separate regressions using as a dependent variable the probability of non-employment among individuals who were working in the firm as of 2008m12. Sample restricted to collective contracts in provinces with inflation rates in 1993 higher than the national one. The Figure on the right plots the same regressions restricting the sample to collective contracts in provinces with inflation rates in 1993 lower than the national one. All regressions control for 111 month-specific three-digit industry and 24 month-specific province dummies. Robust s.e. clustered at the 3-digit industry \times province level.



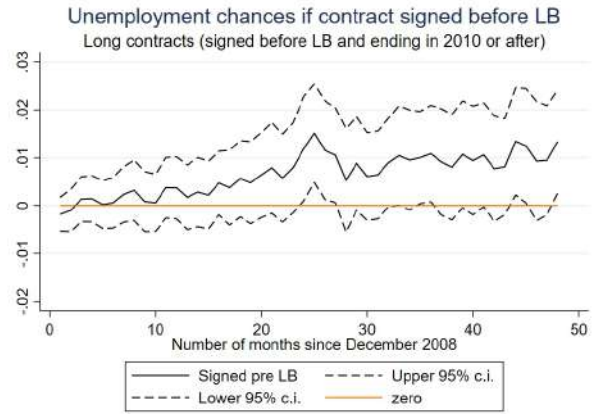
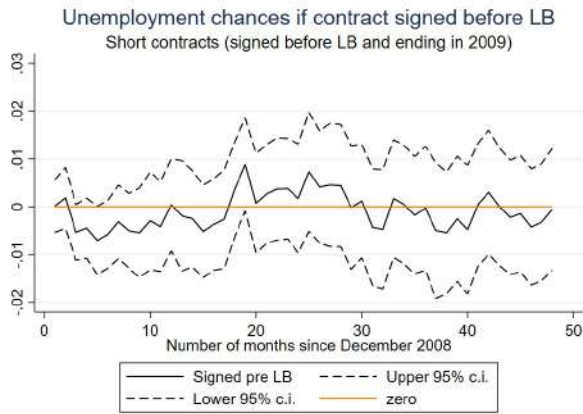
a) 1993 recession (0=1992m12)



b) 2009 recession (0=2008m12)

Figure 7. Actual wage growth among workers covered by a province-industry agreement, by distance from the minimum wages

Notes: The Figures plot the OLS estimates and 95% confidence intervals of the interaction terms "(Collective Contract signed before 1992m12)*(Wmin<=W1991m12<=1.1*Wmin)", "(Collective Contract signed before 1992m12)*(1.1*Wmin<=W1991m12<=1.2*Wmin)", and "(Collective Contract signed before 1992m12)*(1.2*Wmin<=W1991m12<=1.4*Wmin)" in panel a and "(Collective Contract signed before 2008m9)*(Wmin<=W2007m12<=1.1*Wmin)", "(Collective Contract signed before 2008m9)*(1.1*Wmin<=W2007m12<=1.2*Wmin)" and "(Collective Contract signed before 2008m9)*(1.2*Wmin<=W2007m12<=1.4*Wmin)" in panel b. The dependent variable is nominal wage growth in 1993 (panel a) or in 2009 (panel b). Sample restricted to full time employees with available information on their corresponding minimum wage floor, who stayed in the same firm all through 1993 (panel a) or 2009 (panel b). All regressions control for collective contract fixed effects. Omitted category: "(Collective Contract signed before 1992m12)*(1.4*Wmin<W1991m12)" in panel a and "(Collective Contract signed before 2008m9)*(1.4*Wmin<W2007m12)" in panel b. The Figures on the left are for workers covered by any collective contract while the Figures on the right are for the restricted sample of workers covered by collective contracts with escalation clauses. Robust s.e. clustered at the 3-digit industry \times province level.



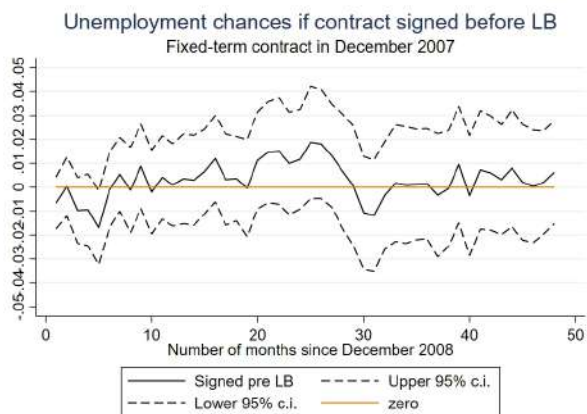
a) Treated contracts expiring in 2009

b) Treated contracts expiring in 2010-11

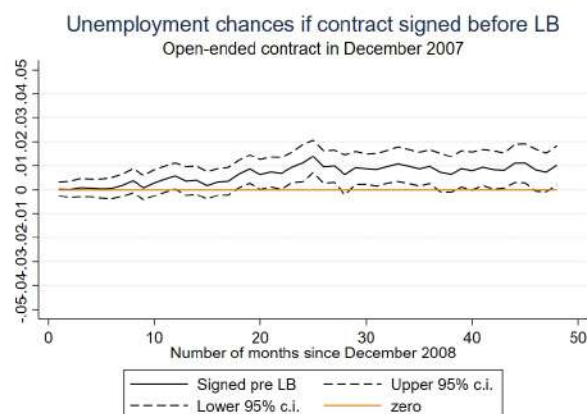
2009 recession (0=2008m12)

Figure 8. Employment outcomes among workers covered by a province-industry agreement, by collective contract length

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before September 2008" in 48 separate regressions using as a dependent variable the probability of non-employment among individuals who were working in the firm as of 2008m12. Sample restricted to contracts signed after LB or before, that expired in 2009. The Figure on the right plots the same regressions restricting the sample to contracts signed after LB or before, that expired after 2009. All regressions control for 111 month-specific three-digit industry and 48 month-specific province dummies. Robust s.e. clustered at the 3-digit industry \times province level.



a) Fixed-term contract

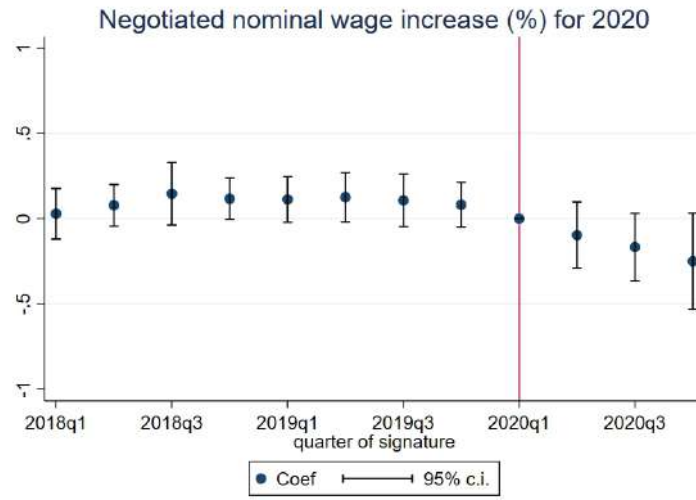


b) Open-ended contract

2009 recession (0=2008m12)

Figure 9. Employment outcomes among workers covered by a province-industry agreement, by type of contract

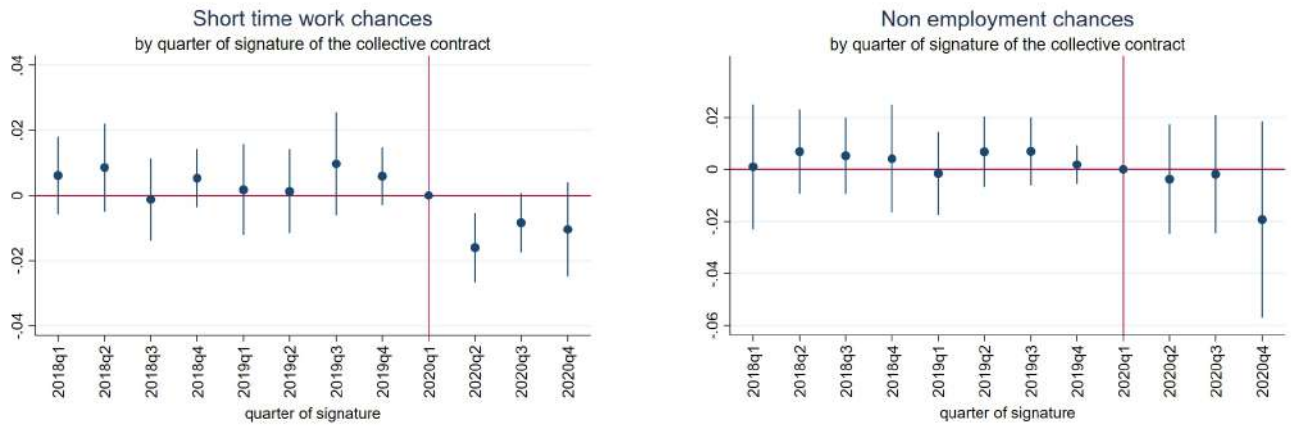
Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract signed before September 2008" in 48 separate regressions using as a dependent variable the probability of non-employment among individuals who were working in the firm as of 2008m12. Sample restricted to workers with a fixed term contract in 2007m12. The Figure on the right plots the same regressions restricting the sample to workers with an open-ended contract in 2007m12. All regressions control for 111 month-specific three-digit industry and 48 month-specific province dummies. Robust s.e. clustered at the 3-digit industry \times province level.



COVID-19 pandemic (0=2020q1)

Figure 10. Negotiated wage growth for 2020 by quarter of signature before and after the onset of the COVID-19 pandemic

Notes: The Figure shows the OLS estimates and 95% confidence intervals of quarter of signature on average wage growth set by collective contracts. The regression controls for province and 3-digit industry fixed effects. Robust s.e. clustered at the 3-digit industry \times province level.



COVID-19 pandemic (0=2020q1)

Figure 11. Short time work and non employment by quarter of signature before and after the onset of the COVID-19 pandemic

Notes: The Figures plot the OLS estimates and 95% confidence intervals of the collective contract quarter of signature on the probability of short time work (left panel) and non employment (unemployment or non participation, right panel). All regressions include three-digit industry, and province*quarter fixed effects. Robust s.e. clustered at the 3-digit industry \times province level. Survey weights used.

Appendix A. Additional Figures

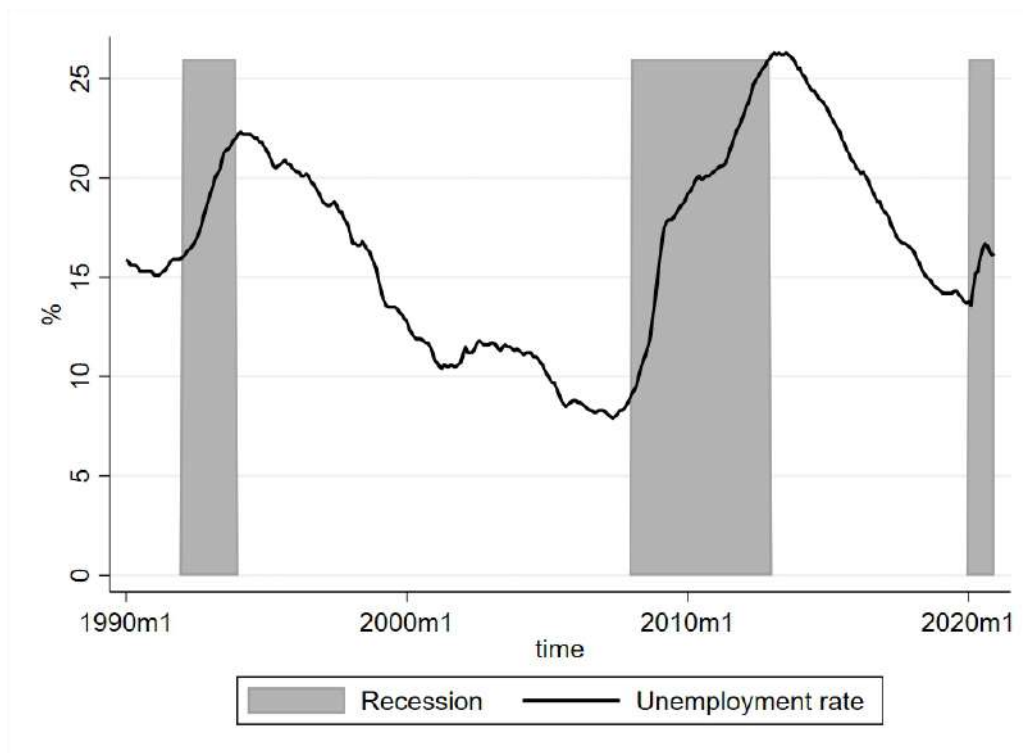


Figure A1. Unemployment rate in Spain, 1990m1-2020m12

Note: The Figure shows the evolution of the average monthly unemployment rate in Spain in the last 30 years.

There are sharp increases in 1993, 2008-2012 and 2020.

Source: INE, Labour Force Survey (EPA).

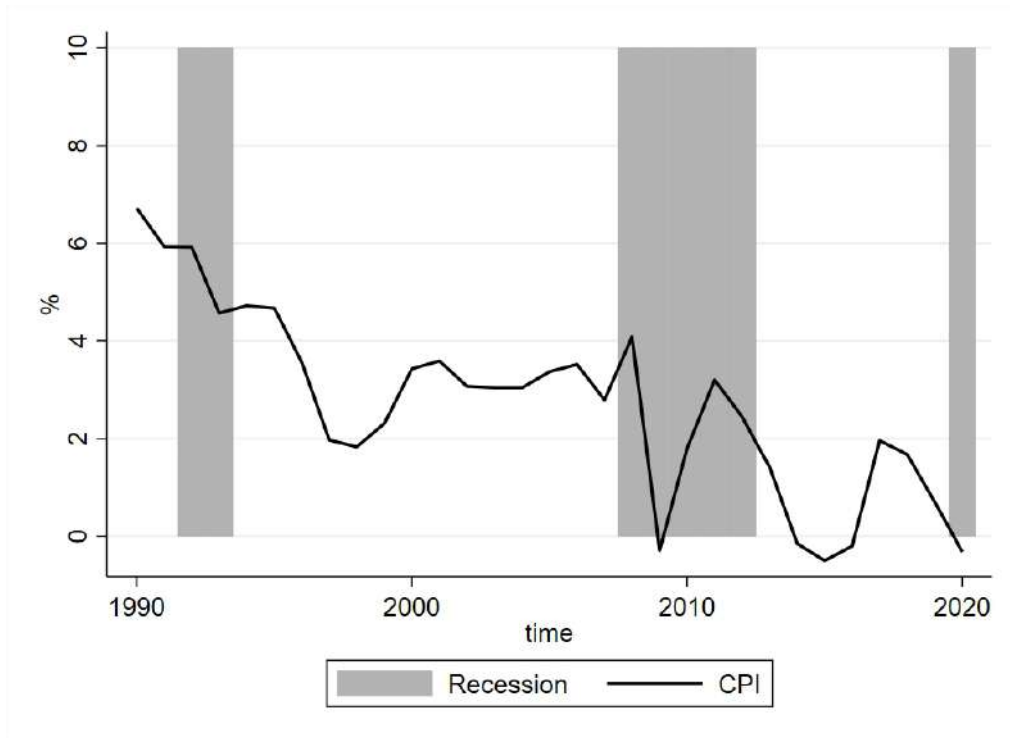


Figure A2. Consumer Price Index in Spain, 1990-2020

Note: The Figure shows the evolution of the inflation rate in Spain in the last 30 years. It was high in 1993 and particularly low in 2009 and 2020.

Source: INE.

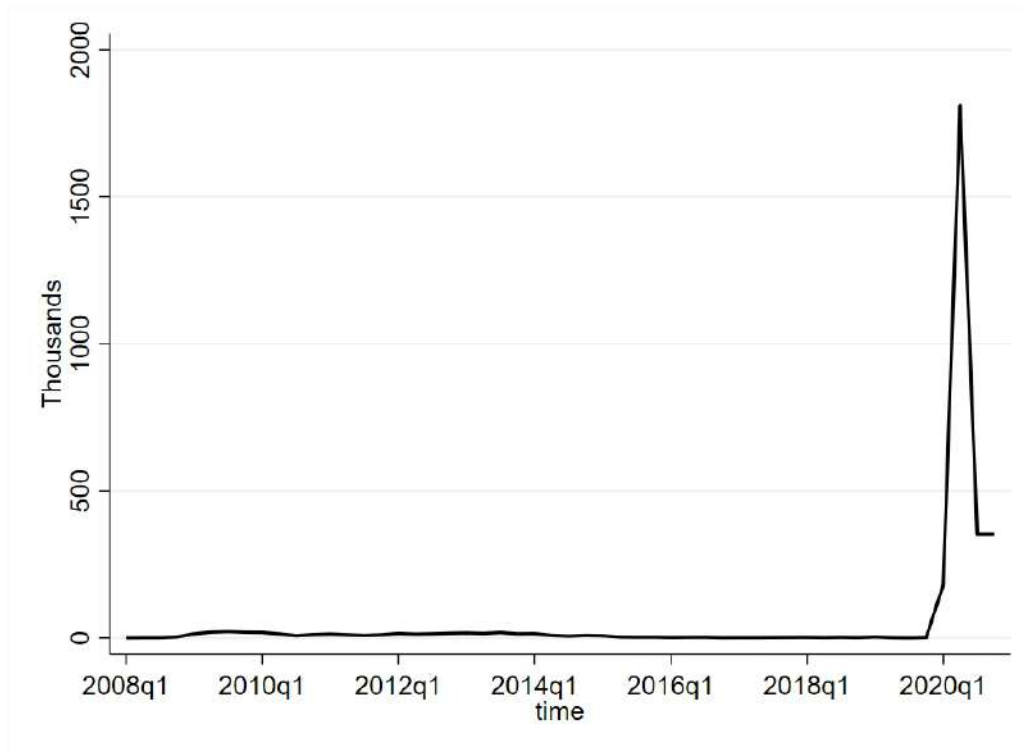


Figure A3. Number of workers in short time work (STW) in Spain, 2008q1-2020q4

Note: The Figure shows the average yearly number of workers in short time work (in thousands) in Spain. There is a particularly sharp increase only in 2020 (there are no data available before 2008).

Source: INE, Labour Force Survey (EPA).

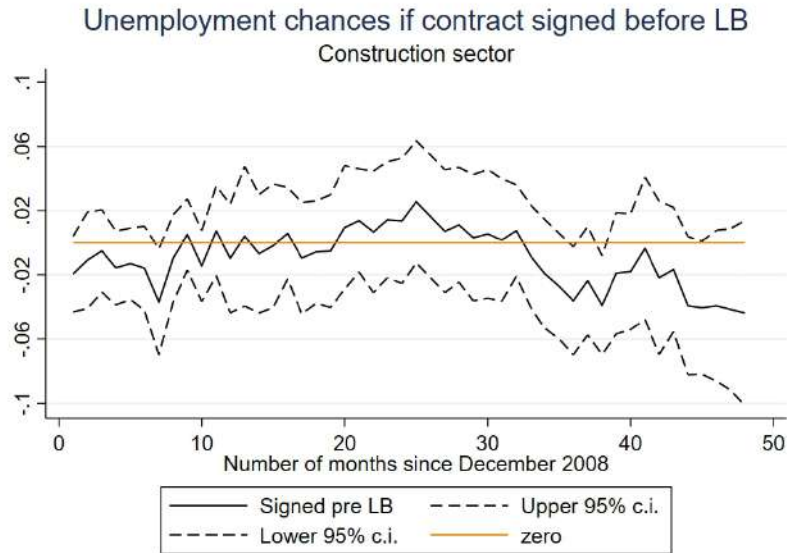
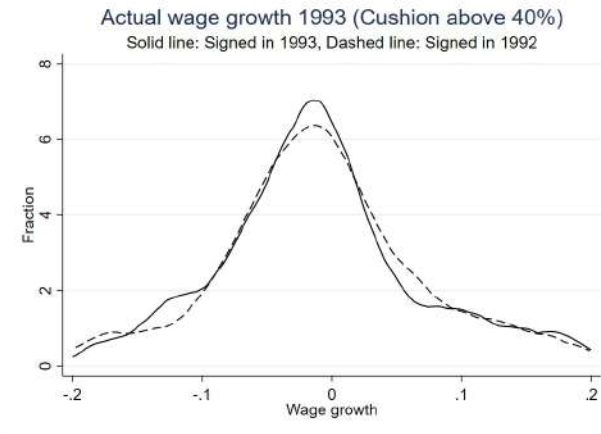
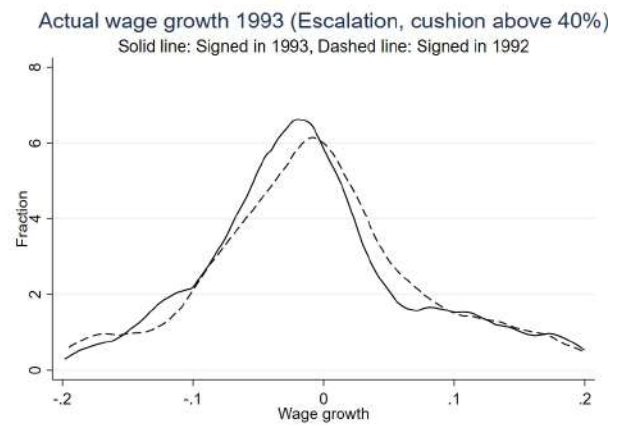


Figure A4. A case study with construction
2009 recession (0=2008m12)

Notes: The Figure plots the OLS estimates and 95% confidence intervals of the variable "Collective Contract expired before 2008m9" in 48 separate regressions using as a dependent variable the probability of non-employment among individuals who were working in the firm as of 2008m12. The sample is restricted to workers in the construction sector, whose collective contracts are signed at different dates at the province level but all set the same wage growth nationwide. The regressions control for month-specific three-digit industry dummies within the construction sector and month-specific province dummies. Robust s.e. clustered at the 3-digit industry \times province level.

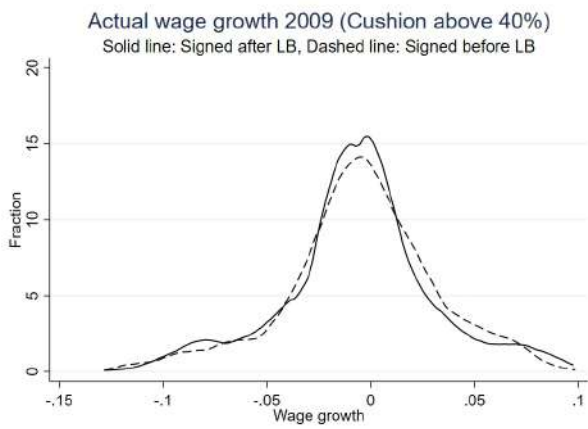


a) All contracts

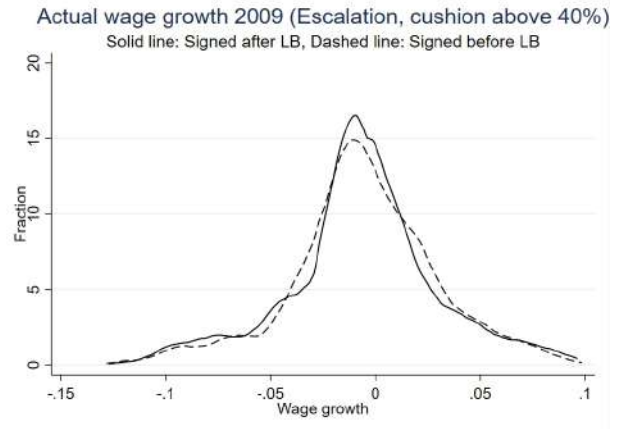


b) Contracts with escalation clauses

1993 recession



a) All contracts

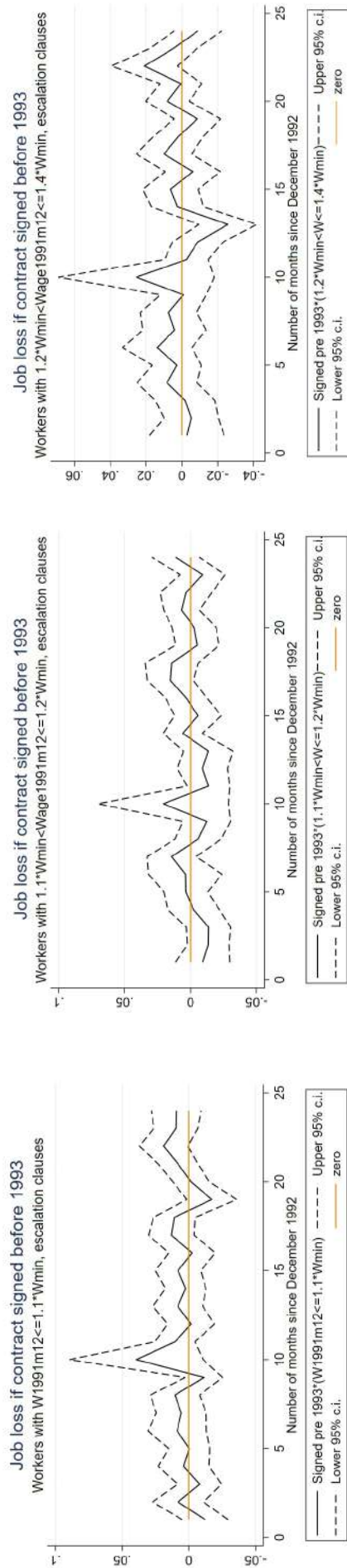


b) Contracts with escalation clauses

2009 recession

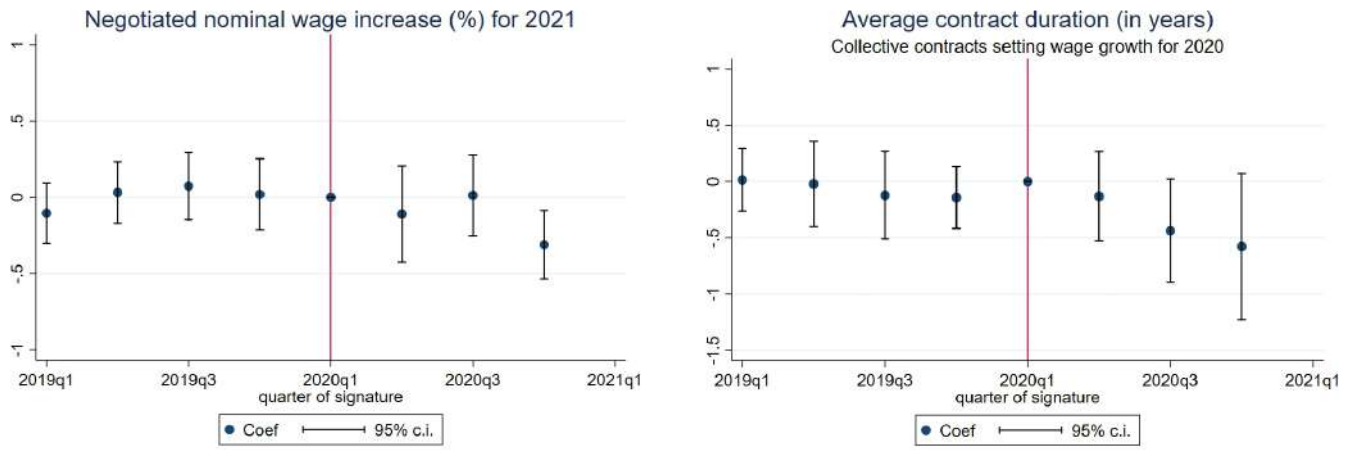
Figure A5. Actual wage growth among workers covered by a province-industry agreement, by presence of escalation clauses

Notes: The Figures plot the distribution of actual wage growth in 1993 (upper panels) or in 2009 (lower panels) for workers with a large cushion (above 40% in 1991m12 or 2007m12). The left panels are for workers covered by any province level contract while the right panels are for workers covered by province level contracts with escalation clauses.



1993 recession (0=1992m12), agreements with escalation clauses
Figure A6. Job loss among workers covered by a province-industry agreement, by presence of escalation clauses

Notes: The Figure on the left plots the OLS estimates and 95% confidence intervals of the interaction term " $(\text{Collective Contract signed before } 1992m12) * (Wmin \leq W1991m12 \leq 1.1 * Wmin)$ " in separate monthly regressions. The dependent variable is the monthly probability of job loss among individuals who were working in the firm as of 1992m12. The estimates refer to workers whose pre-recession earnings were at most 10% above the minimum wages. The Figure in the center plots the OLS estimates and 95% confidence intervals of the interaction term " $(\text{Collective Contract signed before } 1992m12) * (1.1 * Wmin \leq W1991m12 \leq 1.2 * Wmin)$ " from the same regressions as above but refer to workers whose pre-recession earnings were 10-20% above the minimum wages. The Figure on the right plots the OLS estimates and 95% confidence intervals of the interaction term " $(\text{Collective Contract signed before } 1992m12) * (Wmin \leq W1991m12 \leq 1.2 * Wmin)$ " from the same regressions as above but refer to workers whose pre-recession earnings were 20-40% above the minimum wages. All regressions control for collective contract fixed effects. Omitted category: " $(\text{Collective Contract signed before } 1992m12) * (1.4 * Wmin < W1991m12)$ ". Minimum wages are specific of each occupation (10) and province. Sample restricted to workers covered by collective contracts with escalation clauses. Robust s.e. clustered at the 3-digit industry \times province level.



COVID-19 pandemic (0=2020q1)

Figure A7. Negotiated wage growth for 2021 and contract duration by quarter of signature before and after the onset of the COVID-19 pandemic

Notes: The Figures plot the OLS estimates and 95% confidence intervals of the collective contract quarter of signature on the negotiated wage growth for 2021 (left panel) and average duration of collective contracts setting wage growth for 2020 (right panel). The regressions control for province and 3-digit industry fixed effects. Robust s.e. clustered at the industry \times province level.

Appendix B. More on collective contracts

Table B1. An example of minimum wage floors set in a collective agreement (construction sector in Navarre, 2010)

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. ^a	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. ^a	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. ^a	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. ^a	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. ^{er} 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. ^o 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. ^{er} 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. ^o año	760,12	228,56	65,54	1.054,22	1.314,97	1.314,97	1.314,97	15.541,33

Notes: The first column lists all occupations and the last column lists the total minimum wage floor (annual) by occupation.

Table B2a. Descriptive statistics: Sample means (standard deviations)-1993 recession

Characteristic	Full sample (1)	Subsample with wage floors (2)
Sectoral distribution		
Agriculture, manufacturing & utilities (%)	18.5	22.9
Construction (%)	21.6	22.4
Trade, Food & Accomodation (%)	32.5	30.0
Transportation (%)	6.20	4.87
Finance and real estate (%)	0.20	0.32
Services to businesses, health & educ. (%)	20.0	19.4
Collective contract characteristics		
Multi-year (%)	59.5	56.5
Collective contract duration (in years)	1.43 (0.61)	1.40 (0.59)
Escalation clause (%)	62.0	65.2
Negotiated wage growth for 1993 (%)	5.20 (2.23)	5.33 (2.18)
Worker characteristics		
Age (in years)	37.4 (11.0)	36.3 (10.8)
Female (%)	29.2	29.3
College, managers (%)	15.2	16.7
White collar workers (%)	30.6	31.7
Blue collar workers(%)	54.2	51.6
Fixed-term contract (%)	n.a.	n.a.
N	73,183	44,334

Notes: Sample of workers' Social Security records linked to their corresponding provincial collective contract (col. 1) and with information on minimum wage floors (col. 2). All worker characteristics refer to December 1991. Escalation clause takes value one if the contract stipulates an adjustment for realized inflation whenever it exceeds a threshold level.

Table B2b. Descriptive statistics: Sample means (standard deviations)-2009 recession

Characteristic	Full sample	Subsample with wage floors
	(1)	(2)
Sectoral distribution		
Agriculture, manufacturing & utilities (%)	12.6	10.9
Construction (%)	23.0	29.4
Trade, Food & Accomodation (%)	32.7	27.3
Transportation (%)	4.63	4.35
Finance and real estate (%)	0.31	0.00
Services to businesses, health & educ. (%)	26.3	28.1
Collective contract characteristics		
Multi-year (%)	88.2	93.0
Collective contract duration (in years)	3.29 (1.22)	3.44 (1.21)
Escalation clause (%)	64.3	52.0
Negotiated wage growth for 2009 (%)	2.09 (1.51)	1.91 (1.65)
Worker characteristics		
Age (in years)	37.3 (10.7)	37.3 (9.55)
Female (%)	41.3	40.3
College, managers (%)	14.8	16.5
White collar workers (%)	35.0	35.0
Blue collar workers(%)	50.1	48.5
Fixed-term contract (%)	14.0	13.1
N	99,735	43,209

Notes: Sample of workers' Social Security records linked to their corresponding provincial collective contract (col. 1) and with information on minimum wage floors (col. 2). All worker characteristics refer to December 2007. Escalation clause takes value one if the contract stipulates an adjustment for realized inflation whenever it exceeds a threshold level.

ARTICULO 4 - GARANTIA MINIMA

1.- Para el año 1992 y 1993 con independencia de los conceptos económicos pactados en el artículo anterior, se garantiza a todo trabajador afectado por el presente Convenio, una cantidad que se fija en el resultante en pesetas anuales de un incremento porcentual mínimo del IPC menos un 1 punto sobre el Salario Bruto Anual percibido por el trabajador a fecha 31 de Diciembre de 1991 y 1992 respectivamente.

2.- La regularización salarial acordada en el punto 4 del ARTICULO 3, será aplicada a esta garantía mínima, en los mismos términos acordados.

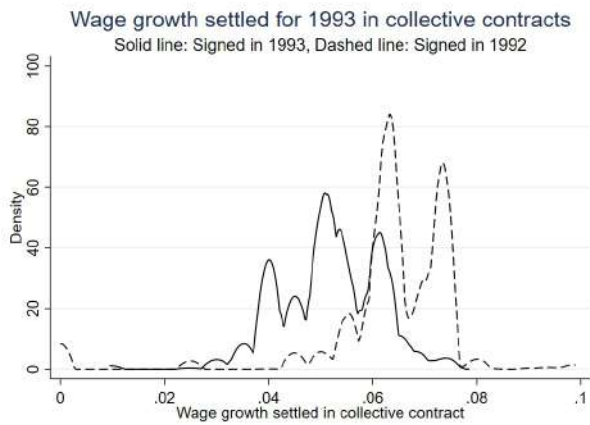
ARTICULO 5 - REVISION SALARIAL

1.- Para 1992 se establece una revisión salarial que consistirá en la diferencia entre el IPC previsto considerado como incremento de la Masa Salarial Global, y el I.P.C. oficial resultante al 31 de Diciembre de 1992, publicado por el I.N.E.

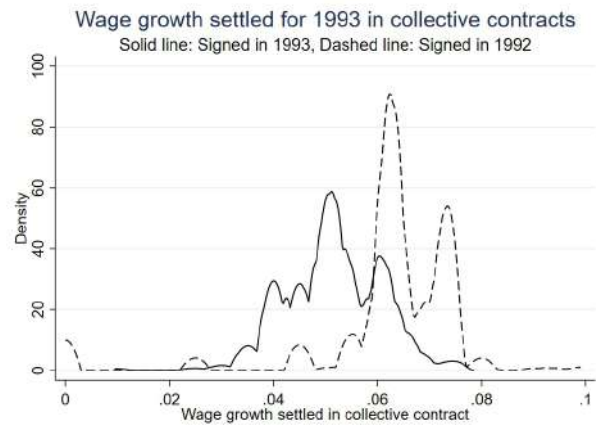
2.- Para 1993 se establece una revisión salarial que consistirá en la diferencia entre el IPC previsto como aumento de la Masa Salarial Global para este año, y el IPC oficial resultante al 31 de Diciembre de 1993, publicado por el I.N.E.

Figure B1. Extract of an escalation clause in a collective contract signed in 1993

Notes: The Figure shows an extract of an escalation clause in the 1993 collective contract of the meat industry. It establishes a minimum guaranteed wage growth for all workers for 1992 (1993) equal to the realized inflation by the end of 1991 (1992) minus 1 pp. It also states that wage floors for 1992 are revised (ex post) to account for the deviation between the inflation that was expected for 1992, when the collective contract was signed, and the realized inflation. Lastly, it states that a similar revision of the 1993 wage floors may take place in the future in case there is a deviation between the expected and realized inflation.

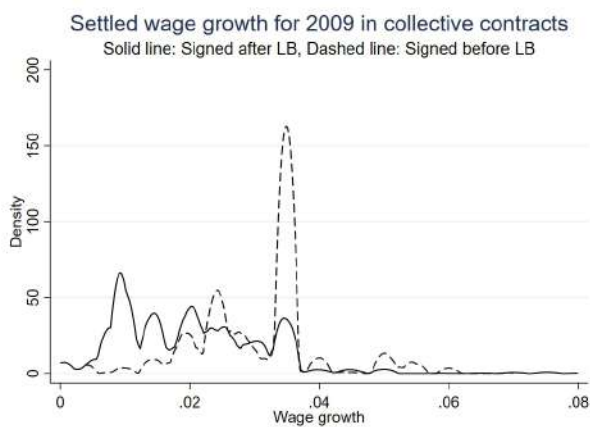


a) Full sample

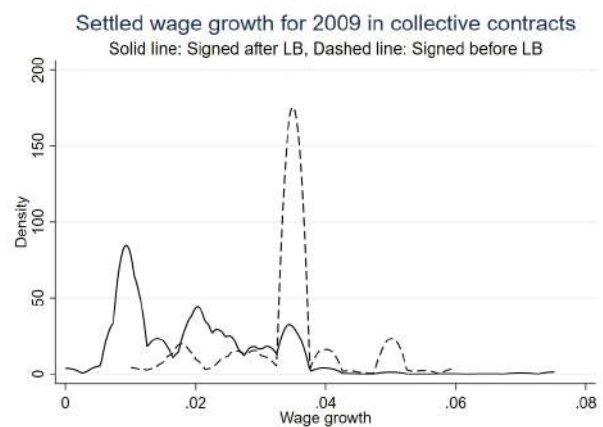


b) Subsample with wage floors

1993 recession



a) Full sample

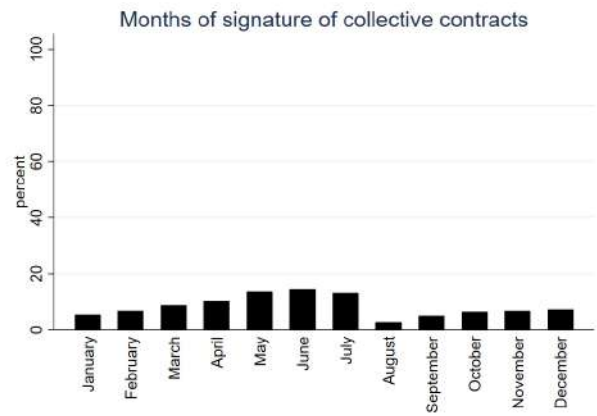
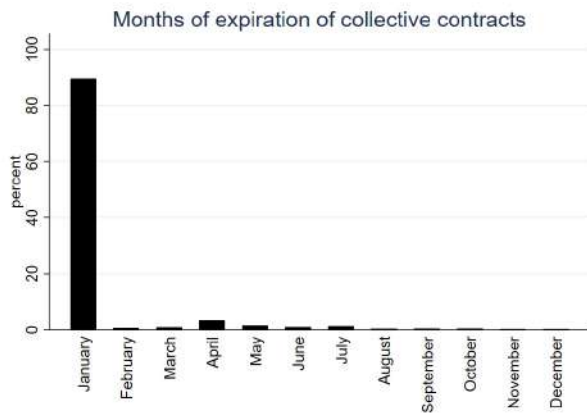


b) Subsample with wage floors

2009 recession

Figure B2. Wage growth settled in collective contracts

Notes: The Figures on the left plot the negotiated nominal wage growth for 1993 or 2009 settled by the full sample of collective contracts while the Figures on the right plot the negotiated nominal wage growth for 1993 or 2009 settled in the restricted sample of collective contracts in which there is available information on workers' minimum wage floors.



a) Months of expiration

b) Months of signature

Figure B3. Months of expitation and signature of collective contracts, 1990-2010

Notes: The Figure on the left plots the months when collective contracts in Spain expired in the period 1990-2010 and the Figure on the right plots the months when collective contracts in Spain got signed (renewed) in the same period. While the vast majority of collective contracts expires on January 1st, signature dates are distributed fairly homogeneously across months.

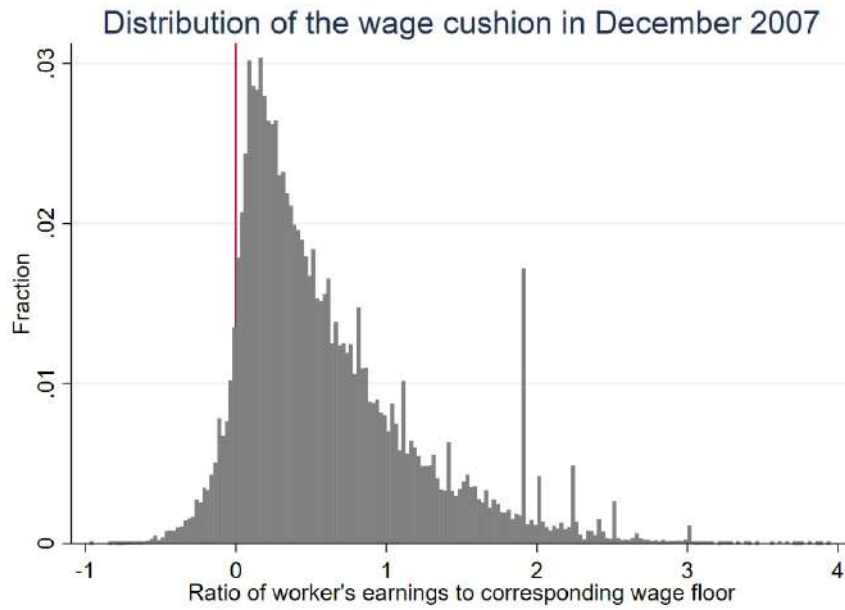


Figure B4. Distribution of actual wages around the minimum wage floors in December 2007.

Notes: The Figure plots the distribution of workers' nominal wages around the minimum wage floors in December 2007. The sample is restricted to workers with available information on minimum wage floors. The red vertical line indicates cases for which the actual wage coincides with the minimum wage floor that corresponds to the worker's sector, province and skill.

Appendix C. More on COVID-19 pandemic

To analyze the employment consequences of wage rigidity during the first stage of the COVID-19 pandemic, we use the 2018-2020 waves of the Spanish Labor Force Survey (Encuesta de Población Activa, EPA). EPA is a representative survey of the entire population and takes place at a quarterly frequency. It contains information on individuals' employment status, occupation, province and sector of activity as well as demographics and educational attainment. For non-employed individuals who ceased to be employed a year ago or less, the occupation, province and sector of activity refer to the most recent ones. EPA allows us to directly identify workers in short time work as employed individuals who state that they do not work during the week of reference are asked to report the reason. Short time work is one possibility (other possible reasons include sickness, strike, training etc.). Using the information on province and sector of activity we are able to match workers in EPA to the register of collective contracts.

We study employment outcomes (intensive and extensive margin) by estimating two regressions (LPMs) on repeated cross sections:

$$Y_{jspt} = \alpha_{0t} + \alpha_{1t} \sum_{l=-9}^{+3} Q_l \text{SIGNED}_{sp} + \gamma X_{jt} + \text{ind}_s + \text{prov}_p * q_t + \varepsilon_{jspt}. \quad (1)$$

In the first regression, the dependent variable, Y_{jspt} , takes the value 1 if worker j , who is employed in industry s and province p is in short time work in quarter t and 0 otherwise. In the second regression, the dependent variable, Y_{jspt} , takes the value 1 if worker j , who is/used to be employed in industry s and province p is non-employed in quarter t and 0 otherwise. $Q_l \text{SIGNED}_{sp}$ is the quarter of signature of the collective contract in industry s and province p . The regressions include industry and province*quarter fixed effects and use survey weights. X_{jt} is a vector of controls at the worker level, namely age, gender, occupation, immigrant background, and a dummy variable that takes the value 1 after 2020q1 if the workers' sector of activity is classified as critical infrastructure, and 0 otherwise.