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Staggered Contracts and Unemployment During Recessions

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Staggered Contracts and Unemployment during Recessions*

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Abstract

This paper studies the impact of downward wage rigidity on wage and employment dynamics after the outbreak of major recessions in Spain. Downward wage rigidity stems from collective agreements, which set province-sector-skill specific minimum wage floors for all workers. By exploiting variation in the renewal of collective contracts, we find that agreements signed before the onset of recessions settle on higher nominal negotiated wage growth than agreements signed after. Leveraging Social Security data and the distribution of the worker-level bite of minimum wage floors, we document that the negotiated wage rigidity translated into higher wage growth mainly among workers with wages close to the floors. Consequently, these workers experienced a substantial and highly persistent increase in the probability of non-employment but only if they were covered by collective agreements of long duration. Our findings highlight the interplay between rigidity at different parts of the wage distribution 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: Employment, Job Separation, Collective Bargaining, Wage Rigidity, Staggering, Social Security Data

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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 at 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 to date 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 major recessions over the last 30 years in Spain. The source of wage rigidity stems from collective agreements bargained at the province-sector level that automatically apply to all firms and their workforce in the bargaining unit, i.e., that are automatically extended with very limited possibility of opting out. 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 a negative macroeconomic shock (i.e., contracts are staggered). 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. On the contrary, firms and workers covered by contracts that expired and got 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 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. In addition, collective contracts in Spain set minimum wage floors, so within each agreement, the degree of wage rigidity varies depending on the distribution of wages around those floors. In sum, those multiple sources of cross-sectional variation in wage growth provides a unique way to estimate the role of downward nominal wage rigidity on job separations and non-employment after a large fall in aggregate demand.

We identify two arguably unanticipated aggregate shocks that resulted in a large fall in employment: the onset of the 1993 recession in the first quarter of 1993 and the onset of

[1] Adamopoulou 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.

[2] Collective contract length was, on average, around one year in the 90s and around three years in the 2000s.

the 2009 recession in the third quarter of 2008. As Figure A.1 in Appendix A shows, the unemployment rate rose sharply in 1993 and in 2009-2012. The two recessions had different characteristics. The first recession took place in a period of relatively high inflation and price volatility, with collective contracts of short duration. Therefore, wage rigidity due to the lack of renegotiation was rather short-lived even in presence of wage indexation practices. The second recession occurred instead during years of very low inflation (Figure A.2 in Appendix A) with lengthy collective contracts that were setting wage growth for several years ahead.

We use a register that contains all collective bargaining agreements signed in Spain in the period surrounding the onset of each recession. The dataset contains information about the exact date of signature and expiration of the collective contracts and the nominal negotiated wage growth that they set. Therefore, we can infer whether employers and unions could observe and possibly incorporate the deterioration of the macroeconomic conditions during the negotiations. In this way, we can classify collective contracts as "rigid" or not. We then match the information on all collective contracts at the sector-province level with longitudinal data from a 4% random sample of Spanish Social Security records (representative of the universe of private-sector employees). 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. Furthermore, we also use large subsamples (60% in the 1993 recession and 40% in the 2009 one) with information on wage floors to identify workers most exposed to wage rigidity.

Our empirical strategy exploits the exogenous exposure of different sectors-provinces to wage rigidity and controls for other factors that may correlate with the date of signature, wage growth and employment outcomes. First, we estimate specifications that include contract-level fixed effects thus absorbing 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. Second, we address any possible concerns regarding the endogeneity of the signature date (e.g., due to strategic considerations by the social partners) 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". By comparing those differential outcomes across contracts signed (or expired) before and after the onset of the recessions we can infer the impact of wage rigidity on employment outcomes. Third, we adopt a setting akin to an "event study" to verify that the parallel-trends assumption is satisfied in our setting and examine the persistence of the estimated effects.

Our analysis uncovers four main findings. First, contracts signed before the onset of the 1993 and 2009 recessions settled for a nominal negotiated wage growth for the first year of the recession between 1 and 1.5 percentage points (pp) higher than contracts signed afterwards. An analysis with a representative sample of workers with available information about their corresponding minimum wage floors shows that during both the 1993 and the 2009 recessions, negotiated wage rigidity was reflected into increases in the actual wages of workers whose pre-recession wages were close to (up to 20% above) the minimum wage floors and were covered

by "rigid" contracts. Second, we find that nominal wage rigidity has severe employment consequences only during recessionary periods of low inflation with collective contracts of long duration. More specifically, we find that wage rigidity had no employment consequences during the 1993 recession, when all collective contracts had a particularly short duration (around one year). Instead, wage rigidity led to an increased probability of non-employment during the 2009 recession. This increase was entirely concentrated on workers with wages close to the minimum wage floors who were covered by rigid contracts of two- or three-year duration.[3] According to our estimates, workers whose collective agreements were not able to quickly adjust remained non-employed even four years after the onset of the 2009 recession. 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 job separations to negotiated wages of up to 0.5 by the end of 2010. Among workers whose collective contracts could not be renegotiated, we detect 1.5 pp higher chances of non-employment rates by 2012. The effect is modest compared to the 13% overall non-employment rate in our sample, and it is confined among workers whose pre-recession wages were at most 20% higher than the corresponding wage floor. 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 driven by collective contracts, whose duration prolonged wage rigidity at least two years into the recession. Instead, employment protection legislation (EPL) does not seem to have played an important role as the estimated increase in the probability of non-employment is similar for short- and long-tenured workers or if we focus exclusively on permanent workers. Likewise, in the high-inflation recession of 1993, collective contracts subject to nominal or real wage rigidity experienced similar employment dynamics. Fourth, we show in an Appendix that although part-time work tends to be unresponsive to wage rigidity, the presence of specific policies, like short-time work schemes during the COVID-19 pandemic, may lead to adjustments at the intensive rather than the extensive margin of labor.

Overall, our results are consistent with the hypothesis that the degree of nominal wage rigidity generated by the automatic extension of sector-province agreements and contract staggering can amplify employment destruction during recessionary periods of low inflation with long collective contract duration. Given that collective contracts also set working hours, rules for night-shifts, part time work, and promotions, we also show that these elements do not act as confounding factors. To this end, we study the particular case of construction, a sector where the majority of province-level agreements have set the *same* (nationwide) wage growth since 2002. We find no differential employment responses by date of contract signature in that sector, 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 recession, contract staggering and infrequent negotiations contributed to the propagation of the aggregate shock by increasing job separations and the probability of non-employment.

Related literature. Our study adds to the literature that assesses the extent of wage

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

rigidity and its consequences. Firstly, our setup reproduces the framework 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. Secondly, we rely on the bite of wage floors in two different recessions. Both features allow us to cleanly identify which contracts and workers are exposed to wage rigidity at the onset of different recessions and reproduce the experiment of interest: Does the distribution of wage changes vary in comparable matches subject or not to wage rigidity?[4] This departs from the conceptually important literature that infers the relevance of wage rigidity by estimating the degree of cyclicality of wage changes –see Haefke et al. (2013), Gertler et al. (2020) and Schoefer (2021) for the US, Barwell and Sweitzer (2007) and Elsby et al. (2016) for the UK, Stüber (2017) for Germany, Devicienti et al. (2007) for Italy, Martins et al. (2012) for Portugal or De la Roca (2014) for Spain. Those papers test whether the cyclicality of wages differs between new matches and incumbents and adopt various strategies to control for the cyclical adjustment of match quality. Furthermore, our methods complement existing studies that identify wage rigidity by relying on parametric assumptions on the distribution of wage changes under different regimes –see Altonji and Devereux (2000) or Dickens et al. (2007).

By being able to identify which workers and sectors are exposed to rigidity, we contribute to the literature that studies the role of explicit forms of wage rigidity in shaping different forms of job separations and, ultimately, 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 (2023) exploits the staggered nature of collective bargaining in Italy and estimates large disemployment effects. A study for the Netherlands (Caloia et al., 2023) 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 (2024) 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 quantify how the lack of wage renegotiation affects job separations (both voluntary and involuntary) and, ultimately, the persistence of individual-level job losses. The last channel is important, as the aggregate effects of the lack of renegotiation can be modest if workers who lose their job quickly find new jobs. This is possible because our data allow us to track workers over time and provide estimates of the likelihood of affected workers remaining non-employed.[5] Thus, by identifying labor market dynamics during recessions our results cast light on how job losses associated to wage

[4] 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).

[5] Changes in firm's employment levels include job-to-job changes that leave aggregate employment constant.

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 collective contract duration. Lastly, we analyze in a unified framework both low- and high-inflation recessions and compare contracts with and without different forms of real wage rigidity to uncover that the labor market effects crucially depend on the macroeconomic environment. All in all, differences in the contract length and cushions can explain the wide range of estimated elasticities of employment to collective contract wage growth documented in the literature (e.g., Card and Cardoso, 2022; Fanfani, 2023; 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 briefly describe the main characteristics of collective contracts and other institutional features of the Spanish labor market that may interact with wage rigidity. In Appendix B we provide a more detailed description of the institutional background.

Sectoral collective contracts bargained by employer federations and unions in Spain are extended to *all* firms within a sector. 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, 2013). Extensions of sectoral contracts in Spain take place at various geographical levels but the most common geographical level of sectoral bargaining is the province.[6]

Sectoral collective contracts establish minimum wage floors that vary according to each employee's skill level. They also establish maximum working hours, the number of vacation days and the compensation for non-standard working conditions, like extra time or night shifts. 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%. The presence of escalation clauses was inconsequential during the 2009 recession as inflation rates reached very low levels.

Collective contracts set minimum wages and working conditions for a pre-specified period. Average collective contract duration was much shorter in 1993 (around one year) than in the 2009 recession (above three years). In some cases, the pre-specified validity period of a collective contract expires without unions and employers having reached an agreement to renew it. In

[6]Firm-level bargaining covers about 15% of workers. During the period we consider, wage floors set in those agreements could not fall short of sector-level ones.

these cases and during the period we analyze, all firms within the scope of the agreement are still subject to the working conditions and minimum wages set in the expired agreement. Regarding opting out, the Worker's Act mentions certain conditions that in theory may permit a firm to opt-out from a collective contract. However, a reform in 2012 attempted to determine verifiable conditions that permit opt-outs, because the procedure was particularly cumbersome.

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 Calmforms and Driffill (1988) or Jimeno and Thomas (2013). As a matter of fact, a labor reform in 2012 tried to weaken the automatic extension of sectoral agreements on the presumption that this contracting level prevents aggregate wage adjustments.

The Spanish labor market is characterized by duality (Bentolila and Dolado, 1994) with more than 29% of workers having a temporary contract in 2008, for whom no firing costs applied in case of lay-offs upon contract expiration or end of the task. 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 federal 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.

3 Conceptual framework

Under collective bargaining, employer federations and unions bargain over mandatory minimum wage floors and other working conditions in a sector or province. As collective contracts are renegotiated infrequently (every one or three years on average in the period surrounding the 1993 and 2009 recession, respectively), wage floors cannot readily adjust at the onset of a recession. 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 firms' and workers' outside options, which in turn hinge on the state of the economy –see Gertler and Trigari (2008), Olivei and Tenreyro (2010) or Faia and Pezone (2024). The infrequent renegotiation and staggered nature of these collective contracts implies that during a recession wages in the economy have been negotiated under very different economic conditions. Following an aggregate unexpected perturbation, and due to staggered contracts, unions and employer federations that are still renegotiating their contracts at the time of the perturbation can adjust wage floors. On the contrary, in sectors/provinces with already settled wage floors, there exists a set of low-productivity matches that are no longer viable at wage levels that were negotiated during good times. This is a source of wage rigidity that can act as a propagation mechanism and

generate unemployment, further aggravating recessions. In addition, the lengthier the validity period of already signed collective contracts, the larger the number of matches that are likely to be affected by the lack of renegotiation.

However, at least two factors may affect the magnitude and the distribution of employment losses due to wage rigidity. Firstly, the consequences of a lack of renegotiation of wage floors depend on firm-specific wage setting policies. While the macroeconomic models in Gertler and Trigari (2008), Olivei and Tenreyro (2010) or Faia and Pezone (2024) implicitly assume that collective contracts fix the wages of *all* workers in a sector, in many countries unions and employer federations fix *minimum* wages, and the estimates of the elasticity of workers' wages to collective contract wage floors lie well below one.[7] In this setting, a lack of renegotiation of wage floors does not necessarily translate into pervasive wage rigidity for all workers. The concentration of wages around minimum floors is then a key determinant of the magnitude of the employment losses and the distributional consequences of the inability to renegotiate collective contracts.

Secondly, labor market institutions may shape the labor market adjustment. The existence of firing costs for permanent workers which are increasing in workers' tenure may induce higher separation rates among workers with fixed-term contracts or short tenure (Goux et al., 2001). In addition, there are alternatives to job separations like short-time work schemes, which is a possible way to weather a recession.

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.[8] However, during periods of high 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 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

[7]Card and Cardoso (2022), Cardoso and Portugal (2005) and Fanfani (2023) document elasticities of wages to increases in minimum wage floors of about 0.5 in Portugal and Italy, respectively. Caloia et al. (2023) 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 sector in Spain and Italy. See Adamopoulou and Villanueva (2022b) for a review.

[8]Fehr 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 (2024) examine collective contracts in a period including the low-inflation recession of 2009 and find that monetary policy shocks lower the valuation of the stock market value of firms unable to renegotiate wages.

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. From the supply side, widespread employment losses during recessions may result in long-run unemployment through losses in human capital –associated to shocks in local labor markets (Yagan, 2019).

In sum, we can derive three 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 (2010).

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. Matches whose wages can be renegotiated should have a higher probability of survival than those whose wages cannot be renegotiated –both because they are covered by contracts already bargained at the time of the shock and because their wages are close to the floors. Furthermore, to the extent that the match destruction is due to a lack of wage renegotiation, separations should be employer-initiated, i.e., layoffs –see Hall and Lazear (1984) for a discussion of inefficient separations and Blanco et al. (2023) and Jäger et al. (2020) for different strategies to estimate their prevalence.

4. The consequences of the lack of renegotiation of the wage floors on the dissolution of matches should be increasing in collective contract duration, as wage floors agreed pre-recession (and subject to lack of renegotiation) bite for a longer period.

Finally, macroeconomic factors may cause wage rigidity due to staggered bargaining but impinge differently on wages and on employment across recessions. The severity of a recession or the degree of price adjustments may alter the real impacts of wage rigidity –see Card and Hyslop (1997), Tobin (1972) or Blanco et al. (2023), for the role of inflation. For those reasons, we analyze the 2009 recession in detail and, leveraging on those findings, discuss separately differential employment responses to wage rigidity during the 1993 recession (and during the first year of the COVID-19 recession in Appendix C).

4 Data

We use two main datasets for our analysis. The first one is the Census of Collective Bargaining Agreements (CCBA) 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*.[9] We describe each source in detail below.

Collective Bargaining Agreements (or CBAs from now on) 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. Until 2013 the Registry also contained information on whether the agreement specified an ex-post adjustment (escalation clause) if realized inflation exceeds a threshold specified in the contract.[10] 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 CBAs includes information on the validity period and on the date in which the agreement was signed. Other entries -like minimum wage floors- are not compulsory and unions do not always fill those, as we discuss below.

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 2012 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 1993 recession, we rely on retrospective information of workers present in the 2005 CSWH. Information collected before 2004 would not necessarily 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. As we discuss below, the skill levels correspond to those set in CBAs, a feature that allows us to assign to each worker the corresponding minimum wage.[11]

[9] In Appendix C we use the labor force survey to study the effects of wage rigidity during the COVID-19 pandemic. The main advantage of the labor force survey is that it also contains information on hours worked and on the incidence of short-time work.

[10] In that case, the staff of the Ministry updates the wage growth after communication with the unions and employer federations.

[11] The Social Security classification combines educational attainment and occupation. The upper four tiers

4.1 Linking datasets

The CCBA contains information about the province and the 2-digit sector that determine the coverage of the agreement. We read the text of provincial agreements to assign to each contract a 3-digit sector. We then match the Census of Collective Bargaining Agreements to the Social Security records using the 3-digit sector of economic activity and the province of the establishment where employees work. When the wording of a collective contract covered several 3-digit sectors, we assigned it at the 2-digit level.

Sample selection

Our sample is composed of all workers in the CSWH who are employed by establishments covered by a province-level agreement.[12] 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 sectors regulated by nation- or region-level contracts.[13]

We focus on provincial collective agreements for three reasons. Firstly, provincial-sectorial CBAs are the most common bargaining unit. Namely, provincial agreements regulate the working conditions of 55% of all workers covered by any form of collective bargaining contracts –see Card and de La Rica (2006). Secondly, province-level CBAs are arguably the most binding ones for a particular establishment, as they can only improve the conditions settled in national or regional agreements. Now, while some of the workers in our sample (less than 15%) could be covered by firm-level contracts, those are not readily identifiable in Social Security records, as firm identifiers are anonymized. Still, 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. Finally, the variation of wage settlements across provinces and 3-digit sectors allows us to control for separate trends across geographic areas and sectors.

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.[14]

correspond to (1) workers with a college degree, (2) workers with a short 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 CBAs, as we discuss below.

[12] There are 52 provinces in Spain but we mainly use 50 in the analysis, excluding Ceuta and Melilla.

[13] For 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.

[14] Including 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

4.2 Linked CSWH-CCBA data

Tables 1 and 2 report the descriptive statistics for the samples we use to analyze the effects of wage rigidity during the 2009 and 1993 recessions, respectively. All worker characteristics refer to December 2007 or 1991, respectively. As column (1) of Table 1 shows, around 51% of workers in the estimation sample are blue-collar and their average age is about 38 years Around 85% of workers are covered by an open-ended individual contract.[15] 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 1 and 2 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 1 and 2 reports the differences across these characteristics after controlling for province and 3-digit sector dummies. That is, columnn (4) reports the coefficients of a regression of each covariate on the dummy "Signed pre" (i.e., CBA signed before 1992m12 or 2008m9) and a full set of province dummies and 3-digit sector dummies. Although there were originally some differences in workers' characteristics (compare columns 2 and 3), their magnitude becomes negligible after controlling for province and sector dummies (see column 4). This implies that the treated and control groups of workers are comparable in terms of observable worker characteristics.

By contrast, a statistically significant difference remains in terms of negotiated wage growth across contracts signed before and after December 1992 (0.98 pp higher among those signed prior to the onset of the 1993 recession) or before and after September 2008 (0.61 pp higher among those signed prior to the onset of 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.[16] These patterns confirm i) the presence of wage rigidity among collective contracts signed pre-recession, which may accumulate over multiple years depending 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 Linked CSWH-CCBA-minimum wage floors sample

Unions and employers stopped recording minimum wage floors in the forms they submitted to the CCBA as of 2001. However, for the period spanning 1994-2001, the wage floor by skill level was available for around 70% of all province-level contracts.[17] We estimated the

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.

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

[16] During 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.

[17] The 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.

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. 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-sector cells (42% of all province-level contracts) in December 2007, i.e., about one year prior the 2009 recession. In Adamopoulou and Villanueva (2022b) we compare the wage floors computed with hand-collected information from collective agreements in the metal sector as of 2008, and found very similar estimates.[18] 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. To our knowledge, this is the first paper that merges negotiated minimum wage floors to Social Security records for a period spanning 1990 and late 2010. In later work, Card and Cardoso (2022) did something similar for Portugal in 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. To increase the accuracy of the wage cushion, we restrict the sample to full time employees. Table A.1 in Appendix A 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.[19] Similarly, we compute workers' wage cushion as of December 1991 (See Table A.2 in Appendix A). Concentration around the minima used to be higher in the 90's as 19% of workers have wages between 1 and 1.1 times their minimum wage floor, 8% between 1.1 and 1.2 times the minimum wage floors, 12% between 1.2 and 1.4 times the minimum wage floors and 38% of workers have wages 1.4 times their minimum wage floor or higher.

Tables A.3 and A.4 in Appendix A provide further information on the bite of CBAs distinguishing by workers' characteristics. Although the bite tends to be higher for females, low-skilled, younger and short-tenured workers, there is considerable bite also among males, middle/high-skilled, older and long-tenured workers. Differently from statutory minimum wages, the wage floors set in CBAs vary by occupation, thus affecting a much broader pool of workers.

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 A.1 and A.2 in Appendix A). Moreover, as Figure A.3 in Appendix A shows, the distribution of negotiated nominal wage growth settled in the sample of collective contracts for which we

[18] We make two extra checks of the reliability of the procedure. First, we draw on data from the CCBA. For all agreements with information on wage floors between 1994 and 2001, unions reported both the current wage structure for 10 skill levels in the agreement as well as that in the previous agreement. Thus we can test if the structure of wage floors varies over time. 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. Secondly, we have coded minimum wage floors in the construction sector between 2007 and 2014 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%).

[19] We 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.

observe minimum wage floors (panels B) is very similar to that of the full sample (panels A). Both in the subsample with information on wage floors 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.

5 Methods

Our empirical strategy follows three steps: first we use an event study to analyze whether the date of signature of a CBA influences negotiated wage growth in the onset of a recession. The second step analyzes how changes in negotiated wages associated to the moment of signature impinge on workers' wage growth, distinguishing by the bite of negotiated wages (the percent distance between pre-recession base wages and the minimum wage floors). Finally, we expand on the previous strategy to estimate the short and medium-run employment responses to wage rigidity, again depending on the bite of collective contracts.

The general idea is that due to the prevalence of sector-province agreements in Spain, unions and employer federations continuously bargain over minimum wage floors using all available information about sector-, province- and aggregate-economy-level shocks. The minimum wage floors set in a CBA at a particular date reflect the information available at the time of signature, so a sharp change of information -like the outbreak of a recession- should be reflected in lower growth of minimum wage floors of CBAs signed afterwards. In that setting, the negotiated wage response to the arrival of new macroeconomic information can be estimated by using the following reduced-form model of negotiated wage growth for a given year t (the first year of each recession, 1993 or 2009):

$$\Delta NW_{sp} = \alpha_0 + \sum_{\substack{q=-5 \\ q\neq 0}}^{+6} \alpha_q 1(SIGNED_{sp} = q) + sector_s + province_p + \epsilon_{sp}. \tag{1}$$

The dependent variable, ΔNW_{sp}, is the negotiated wage growth set in sector s and province p for either 1993 or 2009, $1(SIGNED_{sp} = q)$ is an indicator of whether the collective contract covering sector s and province p was signed in quarter q. The last pre-recession quarter of signature of the collective contract (q=0) is 1992q4 (quarter before the onset of the 1993 recession) or 2008q3 (quarter before the onset of the 2009 recession). Contracts signed before the onset of the recession form our treatment group of firms-workers exposed to "rigid wage growth", while contracts signed afterwards could adjust to the aggregate shock, and thus form our control group. The coefficients of the leads and lags, $\sum_{q=-5}^{+6} \alpha_q$, capture any discontinuous change in negotiated wage settlements. According to the staggered contracts hypothesis α_q for post-recession quarters should be negative. Furthermore, the different α_q for pre-recession quarters permit a test of anticipation of the recession. If parties were anticipating a downturn, CBAs reached closer to the onset of the recession (before but close to q=0) should set lower wages than those signed earlier (before q=0 but further apart from it). We make three notes regarding Model (1).

Firstly, note that the dependent variable in (1) is the negotiated wage growth in the first year of a recession (either year 1993 or 2009). Hence, we can abstract from inflation and look at nominal wages. Secondly, sectoral bargaining experiences delays in recessions, and parties may delay the signature of a contract for a given year t after that year has passed. For that reason, equation (1) includes wage settlements for 2009 signed up to six quarters after the onset of the recession (in the second quarter of 2010). Finally, to account for possible regional or sectoral shocks, model (1) includes controls for up to 112 3-digit sector dummies and 50 province indicators. Model (1) is conducted at the collective contract level.

In general, the date of signature is a decision of unions and employer federations. As we discuss below, signature dates are closely related to the date of expiration of a previous CBA. Namely, early signatures for year t denote that the pre-existing CBA expired in 2007 (2009 recession) or in 1992 (1993 recession) while later signature dates reflect long contracts expiring later in the recession (say, expiring at the end of 2008 or 1992, depending on the recession).

The second step is to study the propagation of wages negotiated in CBAs along the full distribution of base wages. Using data from Portugal, Italy and Spain, Card and Cardoso (2022) and Adamopoulou and Villanueva (2022a) document that minimum wages have a limited pass-through to wages higher than the minimum wage floors. Other studies, using Italian data, find a pass-through of about 0.5 (Fanfani, 2023) or higher (Faia and Pezone, 2024). We assess the pass-through of CBA wage growth into growth of base wages by analyzing the matched CSWH-CCBA-minimum wage floors sample. We identify job stayers through 2009 whose pre-recession wages were closest to their corresponding minimum wage floors to examine which set of wages follow the growth of negotiated wages.

This second step uses the same framework as in (1) with two modifications. Firstly, we collapse all pre-recession signature dummies into a single indicator $SIGNEDpre_{sp}$. As mentioned, a part of the variation in the date of signature of agreements signed post-recession is inconsequential for the evolution of base wages. If an agreement for 2009 was reached in 2010, firms could maintain base wages constant in 2009. Furthermore, by pooling agreements we obtain more precise estimates. Secondly, we allow for a differential effect of $SIGNEDpre_{sp}$ according to the pre-recession distance between each worker's base wage and his or her corresponding minimum wage floor. We implement the regression as a Difference-in-Differences framework, where the implicit assumption is that there is a set of wages high above the minimum wage floors for which collective contracts do not bind. We present evidence in Appendix A on whether this is a reasonable assumption in both recessions.

The second model we estimate is:

$$\Delta W_{isp} = \beta_0 + \sum_{k=k_1}^{k=k_3} \beta_{1k} SIGNEDpre_{sp} \times 1(W_{isp}^{pre} < kW_{isp}^{min}) + \sum_{k=k_1}^{k=k_3} \beta_{2k} 1(W_{isp}^{pre} < kW_{isp}^{min}) + \beta_3 SIGNEDpre_{sp} + contract_{sp} + \epsilon_{isp}. \quad (2)$$

ΔW_{isp} is the growth in the base wage of worker i in sector s and province p in the first year of each recession (either 1993 or 2009). For exposition, we momentarily drop the subindex t, as we estimate model (2) using a single year of data in each recession. Model (2) interacts date of

signature with indicators of the distance between the worker's pre-recession base wage and the corresponding negotiated minimum wage floor (itself a function of province, sector and worker's skill level s). We consider three groups k (and one omitted group): workers with pre-recession wages at most 1.1 times the minimum wage floors, between 1.1 and 1.2 times the minimum wage floors, and between 1.2 and 1.4 times the minimum wage floors the minima. The omitted group are workers whose wages in December 1991 or 2007 were at least 1.4 times the minimum wage floors.

The coefficients β_{1k} measure to what extent base wages of workers covered by collective contracts signed before the onset of the recession move with negotiated wages, i.e., they could not adjust to the business cycle. Note that if all wages in a collective agreement moved with the negotiated wage growth, the "distance from the floor indicators" would be irrelevant and all the coefficients of $1(W_{isp}^{pre} \leq kW_{isp}^{min})$ would be zero. That would be a case of almost full pass-through of negotiated wage growth into base wage growth. Also, note that if the degree of pass-through is less than one, there would be dispersion of base wage growth within a collective contract, so we could estimate β_{1k} net of collective contract fixed effects, $contract_{sp}$. That term would absorb both sector and province fixed effects as well as any characteristic of the collective contract that affects all wages in the agreement.

The third step examines how wage rigidity at the onset of a recession affects workers' employment outcomes in the short and medium run. The worker-level perspective is crucial to understand the longer run impacts of rigidity. Firm- or sector- level outcomes are not informative about the duration of individual-level job losses, a key parameter to understand the cost of rigidity. Hence, we fix a set of workers working in the sector-province at the time of negotiation of the corresponding CBA and trace their employment outcomes up to four years after the onset of the recession as well as up to two years before. We thus use a variant of model (2), where we extend the model four years after the onset of the recession and two years prior, and interact all regressors with year dummies.

We examine the effects of wage rigidity on wage and employment outcomes by estimating versions of the following linear probability model (LPM):

$$\begin{aligned} Y_{ispt} = \gamma_0 &+ \sum_{k=k_1}^{k=k_3} \sum_{\substack{t=-2 \\ t\neq 0}}^{t=+4} \gamma_{1kt} SIGNEDpre_{sp} \times 1(W_{isp}^{pre} < kW_{isp}^{min}) \times 1(year = t) \\ &+ \sum_{k=k_1}^{k=k_3} \sum_{\substack{t=-2 \\ t\neq 0}}^{t=+4} \gamma_{2kt} 1(W_{isp}^{pre} < kW_{isp}^{min}) \times 1(year = t) \\ &+ \sum_{\substack{t=-2 \\ t\neq 0}}^{t=+4} \gamma_{3t} SIGNEDpre_{sp} \times 1(year = t) + contract_{sp} \times 1(year = t) + \epsilon_{ispt}. \end{aligned} \tag{3}$$

We pool monthly observations of workers i, who were employed in December 2008 in sector s and province p and estimate a Difference-in-Differences specification in the spirit of an event study. The main dependent variable, Y_{ispt}, takes value 1 if worker i, who was employed in December 2008 in sector s and province p, is employed (has a job) in year t. We consider

that a worker works in a given month if he/she works more than 15 days in the month, and 0 otherwise. We also consider observations two years before the onset of the recession (to capture any differential employment trend among treated sectors) and four years afterwards.

The key independent variable is the triple interaction of two time-invariant variables with the year dummies, $SIGNEDpre_{sp} \times 1(W_{isp}^{pre} < kW_{isp}^{min}) \times 1(year = t)$. The first variable, $SIGNEDpre_{sp}$, is a time invariant variable that takes value 1 if the collective agreement covering sector s and province p during the first year of the recession was signed before or after the onset of the recession. As already described, the second variable, $1(W_{isp}^{pre} < kW_{isp}^{min})$, is a measure of the percent distance between each worker's base wage and their corresponding minimum wage floor in December 2007. In the benchmark specification we consider four groups of workers: workers with pre-recession wages 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 2007 were at least 1.4 times the minimum wage floors. As a robustness exercise, we also consider cushions in a continuous way. The coefficients γ_{1kt} for t=-2 or t=-1 capture any differential trend in the sector-province cell prior to the recession, while the coefficients after the recession (t=1 to 4) capture the outcome afterwards. Note that γ_{1kt} whenever t>0 measure both job separations from the original firm as well as any posterior persistent non-employment (like in Yagan, 2019). Both margins are relevant channels of wage rigidity and we discuss them separately.[20] Importantly, this specification allows the inclusion of a collective contract fixed effect.[21] Recall that the treatment (wage rigidity at the onset of a recession) is time invariant, hence we do not need to estimate a staggered Difference-in-Differences –see Roth et al. (2023).

As a final specification, we examine the effects of wage rigidity on employment outcomes by estimating versions of model (3) without using information on wage cushions. We do this for the 2009 recession first and then for the 1993 recession. The test uses the full sample of matched CSWH-CCBA data. We also use this sample to discuss the elasticity of job separations and employment status to CBA wage growth as well as to study the heterogeneous employment responses by CBA length, exposure to employment protection or other worker characteristics using large samples.

$$Y_{ispt} = \delta_0 + \sum_{\substack{t=-2 \\ t\neq 0}}^{t=+4} \delta_{1t} SIGNEDpre_{sp} \times 1(year = t) + province_p \times 1(year = t) \\ + sector_s \times 1(year = t) + \epsilon_{ispt}. \tag{4}$$

Our empirical specification identifies the employment response to wage rigidity as the result of an amplification mechanism of a macroeconomic shock caused by imperfect wage adjustment. However, 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

[20] The procedure is akin to estimating yearly regressions of Y_{ispt} on time-invariant $SIGNEDpre_{sp}$ and $1(W_{isp}^{pre} < kW_{isp}^{min})$ controlling for sector and province dummies (but pooling permits accounting for auto-correlation in the error term).

[21] In some specifications we control for worker characteristics fixed at the onset of the recession: age (five-year age dummies: below 30, 30-35, 36-40, 40-45, 45-50, above 50 and gender).

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, and as already mentioned, we test the assumption that 1993q1 and 2008q4 was indeed the moment when wage settlements changed abruptly by using equation 1 to examine adjustments in the negotiated wage growth set in collective contracts signed for several quarters before and after the onset of each recession.[22] Secondly, we deal with the possible endogeneity of the date of signature. Danziger and Neuman (2005) show that uncertainty may cause unions and employer federations to delay the renewal of collective contracts. We address this issue by using an alternative measure of exposure to wage rigidity similar to that in Faia and Pezone (2024) but in our case time-invariant. Namely, we use the expiration date of the contract prior to the onset of the recession rather than the signature date of the collective contract 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 but it is less informative about anticipation effects around the quarter when the recession starts.[23] 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. Finally, our methods focus on workers already employed in the sector at the onset of the recession. By keeping composition constant, we mitigate the problems associated to changes in worker quality over the business cycle –see Gertler et al. (2020). The drawback is that we can only pick the margin of exit from non-employment by examining flows of workers employed at the onset of the recession into other jobs during the recession.

6 Results

6.1 Negotiated wage growth set in collective contracts

We start by examining the negotiated nominal wage growth, settled in all province-sector contracts for the first year of each recession, as a function of the quarter when the contract was

[22] We group dates of signature by quarters, as on average we have around 20 collective contracts being signed every month. Grouping contracts by quarter of signature allows us to control for sector and province dummies, while finer disaggregation (e.g., months) would not.

[23] While collective contracts get renewed in all months, the vast majority of collective contracts tend to expire in the end of December, resulting in lower variation (see Figure B.2 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.

signed. In the regressions, we consider four quarters before and up to seven quarters after the onset of each recession and include separate province and 3-digit sector fixed effects. Figure 1 plots the estimated coefficients of equation 1, along with the estimated robust standard errors clustered at the province×3-digit sector 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.[24] Negotiated nominal wage growth adjusts downwards steadily as more information on the severity of the recession arrives to social partners.[25] 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 or not an employer was subject to wage rigidity was exogenous as both recessions were unlikely to be foreseen (also supported by the lack of pre-trends).[26] Furthermore, as we document shortly, the actual wage growth of job stayers suggests that those settlements were binding (especially for workers with pre-recession wages close to the wage floors).

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 stayers with pre-recession earnings close or far from the negotiated minima. To do so, we compute the wage cushion, i.e., the distance between workers' wages and their corresponding minimum wage floor (statutory minimum in their province-sector-skill group cell).

Table 3 presents the estimates of equation 2, 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). On top of the minimum tenure requirement as of December 1992 or 2008, that we impose throughout the analysis, the sample here is further 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).[27] 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

[24] Contracts signed in 1994 for the 1993 recession (panel A) and in 2010 for the 2009 recession (panel B) were signed with a delay and result in wage increases ex-post which were not observed as of 1993 and 2009.

[25] This pattern may also be due to some harder hit sectors 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.

[26] The estimated GDP growth of the Spanish economy was still above zero in the Economic Bulletin of Banco de España in July 2008 –see https://www.bde.es/f/webbde/Secciones/Publicaciones/InformesBoletinesRevistas/BoletinEconomico/be0807e.pdf.

[27] We also exclude workers whose earnings were censored at the Social Security maximum contribution and workers whose wages in December 1991 or 2007 were below the occupation-specific minimum wage floors envisaged by their corresponding collective contact.

2007 were 40% higher than the corresponding minimum. Standard errors are corrected for heteroscedasticity and arbitrary correlation across workers in the same 3-digit sector×province cell. The regressions control for 3-digit sector fixed effects in columns (1) and (2) and for collective contract fixed effects in column (3).[28]

The estimates in Table 3, 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 1.3 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 gender, age and occupation (column 2) and when we include collective contract fixed effects (column 3), suggesting that sample selection or idiosyncratic shocks affecting province-sector cells do not play an important role in determining wage growth.

To compute the overall pass-through, we regress the actual wage growth for 2009 or 1993 on negotiated wage growth for each year and instrument the latter with a dummy indicating whether or not the collective contract was signed before the onset of the recession. This gives for the 2009 and 1993 recession an estimated pass-through of 0.40** and 0.45** with a first stage F-statistic of 30 and 40, respectively.

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 3, 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 set lower wage increases, and those are binding –as in Olivei and Tenreyro (2007) and (2010). 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. Moreover, escalation clauses in 1993 could result in wage growth spillovers among workers with large cushions as we show below. However, any asymmetry in the real wage effects of the two recessions could have differential implications in terms of the distribution of job losses only in the case of "surprise" inflation (See Card, 1990). We examine the role of inflation and escalation clauses in Section 6.9.1.

Another important difference between the two recessions is the collective contract duration. As shown in Table 1, the average duration of collective contracts setting wage growth in 2009 was 3.6 years. Instead, the average duration of collective contracts setting wage growth in 1993

[28] While 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 still identified.

was just 1.3 years (See Table 2). We explore the implications of collective contract duration in Section 6.7.

6.3 Employment outcomes-2009 Recession

According to our analysis so far, staggered collective contracts across province-sector 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 event studies at the worker level to examine the short-run and medium-run effect of wage rigidity on the probability of being non-employed (unemployed or out of the labor force).[29]

We start our analysis with the 2009 recession, for which we have a larger and possibly more representative sample for female workers than in the 1993 recession, as discussed in Section 4. 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 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 the recession. We thus estimate equation 3, where the dependent variable is the average monthly probability of being non-employed in a given year (2008 is the omitted category).[30] The sample is restricted to full time employees with at least one year of tenure as of December 2007 and with available information on their corresponding minimum wage floors. The regression includes collective contract×year fixed effects. Standard errors are clustered at the collective contract level and allow for arbitrary correlation over time within collective contracts.

As shown in Table 3, panel B, nominal wages increased during the 2009 recession, especially among workers closer to the minima. As a result, we observe an increased incidence of non-employment mainly among workers very close or close to the minima (Figure 2, panels A and B) while there is no effect among workers further away from the minima (Figure 2, panel C). 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 3 pp higher in 2009 and 4 pp higher in 2010-2012 than among comparable workers whose collective contract was signed afterwards (See Table A.5, column 1).[31] Conversely, the results are essentially null for workers with cushions above 20%. We reach similar conclusions when we consider cushions (and their interaction with wage rigidity) in a continuous way (see Figure A.4). In this case the effect on the probability of non-employment decreases monotonically as the cushion becomes larger.

Moreover, the event studies confirm that the parallel trend assumption is satisfied in our

[29]Throughout the paper, we abuse the terminology and use interchangeably the terms non-employment and unemployment since they are indistinguishable in the Social Security data.

[30]The incidence of non-employment captures not only the inflow into unemployment but also its persistence.

[31]These estimates are relative to workers with cushions above 40% and are robust to the inclusion of demographic controls, namely gender and age in 2007m12 (See Table A.5, column 2). Also, take into account that the 4 pp employment estimates contain both the job separations effect and the posterior persistence of non-employment effect. We quantify each margin in Section 6.5.

Difference-in-Differences framework, as the estimated effect of wage rigidity is null in 2006 and 2007. Our results suggest that the probability of non-employment goes up for several years following a negative macroeconomic shock in a low-inflation environment. However, the effects are confined to the subgroup of workers close to the minima (i.e., the set of workers subject to wage rigidity).

6.4 Overall effect and heterogeneity by worker characteristics

The Difference-in-Differences estimates in Section 6.3 can be interpreted as relative effects between the two groups of workers close and far from the minimum wage floors rather than aggregate effects. To appraise the macro implications of wage rigidity we use the entire CSWH-CCBA sample and estimate equation 4 without distinguishing by workers' cushion.[32] The regression controls for year-specific 3-digit sector and year-specific province dummies. Standard errors are clustered at the collective contract level and 2008 is the omitted year. Table 4, column 1 and Figure 4 present the results. The estimated coefficients confirm the lack of pre-trends in 2006 and 2007. Moreover, they imply that being covered by a rigid collective contract increases workers' probability of non-employment by 0.3 pp in 2009 and by 0.9 pp in 2010. The increase in the probability of non-employment due to wage rigidity is persistent: it is 1.3 pp higher in 2011 and 2012.[33] The results remain practically unchanged if we control for workers' gender and pre-recession age and occupation (Table 4, column 2).

According to our estimates, the largest effect on employment takes some time to materialize. Collective contract duration may have played a role to this end as lengthier contracts could prolong rigid wages for more than just one year as we show in Section 6.6.2.

Next, we investigate whether the increase in the probability of non-employment was heterogeneous among groups with different characteristics (beyond their cushion). For example, marginal workers with a weak attachment to the labor market (typically female or younger workers) tend to be at a higher risk of unemployment than male or older workers. Columns (1)-(4) in Table 5 indicate that actually male workers were more severely affected by wage rigidity than female workers while there were no major differences by workers' age. Moreover, columns (5) and (6) in Table 5 show that employment losses pertained to both low-skilled (blue collars) and medium-/high-skilled (white collars/college educated, managers) workers. This is because, in our setting, minimum wage floors are occupation-specific and thus binding for a more ample pool of workers than federal minimum wages (See Table A.3). As it is the case in many European countries, employees under a fixed-term contract in Spain can be dismissed cheaply after contract expiration, while employers must incur rather large redundancy costs to dismiss workers on an open-ended contract.[34] We thus examine whether the effects differ by workers' tenure or if we exclude temporary workers from the analysis.[35] Columns (7) and (8) in Table 5 show that although the increase in the probability of non-employment is higher

[32] Also here we impose a minimum tenure restriction (at least one year of tenure as of December 2008).

[33] Recall that wage rigidity is defined at the onset of the recession and remains time-invariant thereafter.

[34] In the period we analyze, severance payments amounted up to 45 wage days per year worked, with a limit of two full year wages and depended on tenure.

[35] As Table A.1 shows, temporary workers account for around 13% of our sample.

for short-tenured workers, long-tenured workers also experience significant employment consequences as a result of wage rigidity. Moreover, column (9) in Table 5 shows that the results are very similar to the benchmark estimates in terms of magnitude and persistence if we only focus on permanent workers.[36] Therefore, it is unlikely that adjustment costs are driving our results. Lastly, columns (10) and (11) explore the role of firm size.[37] We see that the increase in non-employment is rather homogeneous among workers originally employed by firms with size below or above the median.

6.5 Quantifying the effect of wage rigidity on job separations

The increase in workers' probability of non employment, though informative, does not directly indicate job separations. This disparity arises from two main factors. First, non-employment encompasses both the transition into unemployment but also the persistence of the unemployment status. Second, a fraction of job separations can be worker-initiated if poorer prospects on the match make outside options more attractive (e.g., quits or transitions to early retirement).

To assess the effects of rigid contacts on inflows into unemployment, we define the binary variable "job separation" that is equal to 0 if the individual has not experienced a job separation from the original (2007m12) firm by the end of a given year, and equal to 1 if the individual has experienced a job separation from the original firm by the end of a given year (and equal to missing in the years after). We then restrict the sample to December of each year (instead of using all months) and estimate equation 4 with the probability of job separation as an outcome variable. Table 6, column 1, presents the results. We find that the workers' probability of job separation increases by around 1 pp in 2010 and 2011 and fades away in 2012. As column 3 shows, this corresponds to a similar increase in the probability of non-employment, which persists also in 2012.

To put magnitudes in perspective, we find that the wage rigidity in province-sector cells (measured by whether firms could renegotiate wages after a large aggregate shock) resulted in 1.5 pp higher cumulative nominal negotiated wage growth in 2009-2010 (see Table 1, column 4). Moreover, workers in those province-cell sectors experienced 1 pp higher probability of job separation by the end of 2010 (see Table 6, column 1).

However, the literature on the propagation of shocks due to wage rigidity emphasizes the role of employer-initiated separations (i.e., those that are involuntary from the point of view of the worker), which correspond to the definition in Hall and Lazear (1984) of inefficient separations. To test this hypothesis, a crucial feature of CSWH is that it also includes information on the nature of job separation, thus allowing us to pin down involuntary lay-offs (including the expiration of temporary contracts). This speaks more closely to job separations initiated by the firm side. Table 6, column 2 shows the results of a regression where the dependent variable takes the value 1 if a separation is involuntary, and 0 if the match continues. The estimates

[36] One possible explanation for the persistence 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.

[37] Firm size refers to the number of employees of the firm where the worker was employed in 2007m12.

in column 2 indicate that involuntary separations among CBAs signed before the recession were 0.5 pp more likely to end in an involuntary separation in 2010, while the corresponding estimate for all separations is 1 pp. An interpretation is that about half of the job separations that took place in 2010 were involuntary. Similarly, comparing the coefficients of column 3 (non-employment after a separation) and column 4 indicates that in 2010 two-thirds of non employment was due to inefficient separations.

To estimate an elasticity of job separation to negotiated wages, we regress the average monthly probability of job separation in 2009 and 2010 on negotiated wage growth in 2009-2010. Given that the latter is endogenous, we instrument it with whether the respective collective contracts were signed before the onset of the 2009 recession. The estimate we get is consistent with an elasticity of job separation to negotiated wages of 0.52** (in absolute value).[38] If we only consider involuntary job separations, the elasticity becomes smaller (0.27**).

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 (2023) and Martins (2021) for Italy [0.8; 1.1] 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). Our results suggest that elasticities at the firm or local labor market level should be considered with caution, as not all job separations due to the inability to renegotiate wages are involuntary (i.e., they do not reflect only movements along the labor demand curve). Furthermore, as we show below, the elasticity of job separations to CBA wage growth is not a stable parameter across recessions.

6.6 Robustness

In this section we conduct a battery of tests regarding the validity of our identification strategy. More specifically, we examine i) whether the province-sector variability that we exploit throughout the analysis is plausible and reflects only differences in the degree of wage rigidity and ii) whether the cross-sectional dispersion in the degree of wage rigidity is indeed exogenous (i.e., not subject to strategic behavior by social partners).

6.6.1 A case study with construction

In our benchmark specification we exploit variation by sector-province in the date of contract signature to achieve identification. Our main hypothesis is that differences in the wage growth across provinces and sectors caused by the information available to the parties at the time of contract signature is associated to employment 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

[38] The F-statistic of the first stage is 32. We could only match information for about 2/3 of CBAs in 2011, as the register was discontinued. Hence, we do not compute elasticities for 2011.

negotiated at the province level but do not set province-specific wage growth. Construction is one such sector as since 2002 the majority of province-level contracts in the sector 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. We confirm this by regressing the negotiated wage growth for 2009 on the "Signed pre" indicator for collective contracts in the construction sector and in all sectors. Table A.7, column 2 in Appendix A shows that when we consider all sectors, negotiated wage growth for 2009 was higher among collective contracts that were signed pre-recession. Instead, the coefficient of "Signed pre" is very small and not statistically significant when we restrict the analysis to collective contracts of the construction sector (Table A.7, column 1).

Collective bargaining 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 employment losses. Moreover, this variation in signature dates permits identifying the role of confounding factors that correlate both with signature date and with the incidence of non-employment.

We use the distinctive features of collective bargaining in the construction sector to provide evidence in support of our identifying assumptions. To this end, we focus on low-cushion workers (pre-recession cushion below 20%) employed in all sectors and in the construction sector alone (with at least one year of tenure in December 2008 as in the rest of the analysis) and estimate equation 3. Figure 3 presents the Difference-in-Differences estimates of this exercise for all workers and compares them to those in the construction sector. In line with the results in Figure 2, when we analyze all sectors, we detect a significant and persistent increase in the probability of non-employment for low-cushion workers (Figure 3, panel A). By contrast, when we focus on low-cushion workers in the construction section, the increase in the probability of non employment due to wage rigidity is small and insignificant in the entire period of analysis (Figure 3, panel B). The results of this case study confirm that signature dates do not correlate with employment losses for reasons unrelated to wage growth.

6.6.2 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 employment 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. 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 of the previous collective contract (rather than the date of signature of the collective contract setting negotiated wage growth for 2009) as a proxy for wage rigidity. The date of expiration is arguably exogenous since unions and employment federations cannot possibly manipulate it ex post.[39] We thus use the expiration dates and define the ordinal variable "Expired pre" as follows:

$$\text{Expired pre} = \begin{cases} 0 & \text{if contract expired by 2008m12} \\ 1 & \text{if contract expired in 2009m1-2009m12} \\ 2 & \text{if contract expired in 2010m1-2010m12} \\ 3 & \text{if contract expired in 2011m1-2011m12} \end{cases}$$

In this case, the treatment is ordinal and ranges from 0 for collective contracts that expired shortly after the onset of the recession (no wage rigidity) to 3 for collective contracts that expired two years after the onset of the recession (prolonged wage rigidity).

Figure 5 reports the results on the probability of non-employment with this alternative identification strategy. The effects are very similar to the benchmark estimates: the incidence of non-employment remains persistently high for about two years after the onset of the 2009 recession. These results support our benchmark identification strategy and demonstrate that the effects on employment are robust to an alternative definition of wage rigidity.

6.7 Underlying mechanism: Collective contract duration

One key dimension through which wage rigidity may affect employment dynamics is collective contract duration as emphasized in Sorkin (2015).[40] As Table 1 shows, collective agreements setting wage growth for 2009 had an average duration of 3.6 years (3.7 years among those signed before the onset of the recession and 3.5 years among those signed afterwards). Using the information on the date of expiration and the definition of wage rigidity in Section 6.6, we find that among all rigid collective contracts (Expired pre>0), 43% were expiring by the end 2009, 42.5% by the end of 2010 and 14.5% by the end of 2011. Hence, rigid contracts setting higher wage growth up until 2010 or 2011 implied a much larger cumulative increase in the cost of labor than contracts of shorter duration that expired already by the end of 2009 and could be renegotiated shortly after the macroeconomic shock.[41] The lengthy duration of collective contracts may explain why non-employment during the 2009 recession was rather long-lasting

[39] Almost 90% of collective contracts expire in the end of December, thus resulting in lower variation than in the case we use signature dates for identification (see Figure B.2 in Appendix B).

[40] Sorkin (2015) shows 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.

[41] Renegotiating wages during the validity period of a collective agreement was practically impossible for a given firm as opting out was very cumbersome at least until 2012.

(as laid-off workers had limited employment prospects in the sector-province market where they were originally employed).

To test the hypothesis that longer contract duration is driving the persistent employment losses during the 2009 recession, we consider two "treatment" groups within the set of workers covered by rigid collective contracts (Expired pre>0). The first group comprises workers under short contracts set to expire at the end of 2009 (Expired pre=1) while the second group are workers under long contracts, that set wage growth until the end of 2010 or even 2011 (Expired pre= 2 or 3). All the workers under contracts expiring by the end of 2008 are "controls", in the sense that the bargaining parties could adjust wages to the aggregate recessionary shock immediately after its onset.

Panel A of Figure 6 shows the effects of wage rigidity on the average monthly probability of non-employment in a given year among workers covered by short collective contracts (those that could be renegotiated by the end of 2009, at the latest) while panel B shows the effects among workers covered by long contracts (those that could be renegotiated only starting in 2010 at the earliest). The results point to a statistically significant increase in the probability of non-employment only among workers covered by long contracts.

These results imply that longer contract duration is a plausible mechanism behind the long-lasting disemployment effects of wage ridigity during the 2009 recession. Moreover, they partially explain why, in Figures 4 and 5, we find a modest increase in the probability of non-employment in 2009, the year right after the macroeconomic shock: among workers covered under rigid collective contracts, there was a fraction whose wages could be renegotiated already at the end of 2009, thus experiencing negligible employment consequences. On the contrary, increases in the probability of non-employment were concentrated among workers in province-sector cells exposed to at least two years of wage rigidity, thus picking in 2010. To the extent that employers covered by long contracts did not dismiss workers immediately in the onset of the recession, the variation in contract length can account for delayed layoffs.

6.8 Employment outcomes-1993 Recession

After having analyzed the effects of wage rigidity in the 2009 recession, we proceed with the analysis of the 1993 recession. As shown in Sections 6.1 and 6.2, collective contracts signed before the onset of the 1993 recession (i.e., signed in December 1992 or earlier) envisaged higher negotiated wage growth than collective contracts signed after that date. This resulted in higher workers' wage growth in 1993. We thus investigate whether rigid contracts led to employment losses during the 1993 recession in the same way as in the 2009 recession. We focus on workers with at least one year of tenure in 1991m12 and estimate equation 4.[42] As in Section 6.4, the dependent variable is the average monthly probability of non-employment in a given year but now the period of analysis is 1991-1996 and the main regressor, "Signed pre", takes the value 1 for collective contracts setting wages for 1993 that got signed before 1992m12. Figure 7 presents the results. The estimated coefficients are practically null in all years.

[42] As we discuss in Section 6.9.1 below, the Difference-in-Differences estimator is not suitable for the analysis of the employment effects during the 1993 recession as escalation clauses increased wages of all workers, even of those far away from the minimum wage floors.

Our findings suggest that in the case of the 1993 recession, wage rigidity did not result in an increase in workers' probability of non-employment. This may appear surprising at first since wages behaved similarly in both recessions. However, the degree and the impact of wage rigidity also depends on its duration. In a context of high inflation, the average duration of the collective contracts setting wage growth for 1993 onwards was just 1.3 years (see Table 2). Based on the analysis in Section 6.7, which exploited differences in collective contract duration *within* the 2009 recession, we found that contracts with a duration of less than two years did not result in any employment losses. Hence, a plausible explanation behind the lack of employment response to rigid contracts during the 1993 recession is that the particularly short duration of collective contracts during that period rendered wage rigidity inconsequential.[43] In the case of the 2009 recession instead, wage rigidity led to a steady increase in the incidence of non-employment and the effects were visible even four years after the outbreak of the macroeconomic shock, with workers covered by rigid collective contracts of long duration suffering the non-employment toll.

6.9 Other mechanisms and intensive margin adjustments

The main results of our analysis indicate that rigid contracts had no discernible impact on employment during the 1993 recession but led to a persistent increase in the probability of non-employment during the 2009 recession. Besides the duration of collective contracts, other institutional features may interact with wage rigidity, shaping its overall impact. For example, certain labor market institutions or policies may augment the pool of workers subject to wage rigidity, e.g., through escalation clauses or influence the way firms respond to wage rigidity amid recessions, e.g., through short-time work schemes instead of layoffs. In this section we shed light on other possible mechanisms that could drive our results and consider employment adjustments at the intensive margin as an additional outcome.

6.9.1 Inflation rate and escalation clauses

A competing explanation behind the absence of labor market consequences during the 1993 recession is its relatively high inflation rate (around 4.6%). The idea is that high inflation could countervail nominal wage increases set by collective contracts. However, a high inflation rate can effectively grease the wheels of the labor market only if it is unexpected. This is rather unlikely given that in the beginning of the 90s' the inflation rate in Spain was above 5% and it even dropped during the 1993 recession (see Figure A.2 in Appendix A).

Still, high inflation rates during the 90s' were likely to trigger escalation clauses envisaged in the collective contracts. Escalation clauses typically consist of a minimum nominal wage increase guarantee for *all* workers covered by the collective contract, thus rending wage rigidity more widespread. Figure B.3 in Appendix B provides an example of an escalation clause in the collective contract of the food sector signed in 1992, which envisaged a minimum wage increase guarantee for 1993 equal to the realized inflation minus 1 pp. Therefore, the triggering of the

[43]The 1993 recession differed from the 2009 recession in several other dimensions (labor union strength, exchange rate, etc.). To address this issue, our analysis exploits variation in contract duration within the 2009 recession (i.e., ceteris paribus). There is not enough variation to do something similar for the 1993 recession as more than 90% of all treated collective contracts expired already before the beginning of 1994.

escalation clauses during the 1993 recession could even affect workers with wages far from the minimum wage floors.[44]

One way to verify this is to exploit contract-level variation in the presence of escalation clauses *within* the 1993 recession. We thus distinguish between workers covered by collective contracts with and without escalation clauses and re-estimate the Difference-in-Differences specification (equation 2) for workers' wage growth in 1993. Table A.8 presents the results. If we only consider contracts with escalation clauses (column 2), we see that wage growth for 1993 is higher for all workers whose contacts got signed before the onset of the recession, independently from their distance from the minima (the p-value of the test for the equality of coefficients for workers earning at most 10%, 10-20% or 20-40% above the minimum wage floors is 0.32). Instead, for workers covered by collective contracts without escalation clauses, wage rigidity is confined among those very close to the minimum wage floors (column 3).

Our findings point to significant spillover effects of escalation clauses on workers' wages during an inflationary period. Whenever escalation clauses increase the wages of all workers, there is no group of workers that can serve as a control group in a setting that exploits workers' distance from the minimum wage floors. Therefore, to study the employment effects of wage rigidity during the 1993 recession in Section 6.8 we relied on model 4 (see Table A.9, column 1 and Figure 7). We re-estimate equation 4 separately for workers covered by collective contracts that envisaged or not an escalation clause. Table A.9, columns 2 and 3 and Figure A.5 present the results. They show that the probability of non-employment was totally unaffected among workers whose collective contracts did not include an escalation clause. Among workers under escalation clauses, the probability of non-employment responded slightly more but it is not statistically significant either. An indirect consequence of high inflation rates is the short duration of collective contracts. Presumably, social partners incorporated the high price volatility during the 90s' by signing contracts of short duration which could be renegotiated more frequently. This effectively restricted any potential wage rigidity–even in the presence of escalation clauses–to a maximum of one year following the start of the recession, thereby preventing employment losses.

6.9.2 Intensive margin

The analysis so far has considered employment adjustments at the extensive margin (probability of job separation and probability of non-employment). However, adjustments at the intensive margin may be an alternative way to respond to wage rigidity. These adjustments take the form of reductions in the number of hours worked either through the use of part-time contracts or by resorting to short-time work schemes.

We thus examine whether wage rigidity led to an increase in workers' probability of having a part time contract (conditional on being employed). We focus on workers who are currently employed and have at least one year of tenure as of 1991m12 or 2007m12 and estimate equation 4 for the probability of having a part-time contract in each recession. The regressions control

[44]Escalation clauses were a common practice both in 1993 and in 2009, with more than 60% of collective contracts containing one (see Tables 1 and 2). However, escalation clauses do not get triggered in absence of inflation (the inflation rate was around 0 in 2009).

for gender and include province-year and sector-year fixed effects. Table A.10, panels A and B, reports the estimates for the 1993 and the 2009 recession, respectively. None of the estimated coefficients are statistically significant. These findings suggest that part time work did not act as an alternative margin of adjustment to wage rigidity.

The employment response to rigid contracts midst macroeconomic downturns may depend more strongly on labor market policies specifically aimed to maintain worker-firm matches during periods of distress (Balleer et al., 2016). For example, during the COVID-19 pandemic in Spain, there has been an unprecedented use of subsidized short-time work schemes (see Figure C.1 in Appendix C), which may have acted as a buffer. Indeed, using available information from the Spanish Labor Force Survey, we show in Appendix C that wage rigidity during the COVID-19 pandemic led to an increase in workers' probability of being in short time work while the probability of non-employment remained unaffected.

7 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 wage floors that apply to all workers employed by firms in the same sector-province cell. More specifically, we use the numerous sectoral and provincial agreements to identify the impact on employment of the cross-sectional dispersion in wage growth caused by wage rigidity due to contract staggering, i.e., due to contracts that got signed (or expired) 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. Furthermore, 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 wage rigidity can have severe employment consequences only if it is rather prolonged. In the low-inflation recession of 2009, collective contracts had a long duration and wage rigidity due to the lack of renegotiation led to a highly persistent increase in the probability of non-employment. These effects were entirely driven by workers with wages at most 20% above the minimum wage floors, who were covered by collective contracts that expired two years or more after the onset of the recession. During the high-inflation recession of 1993 instead, wage rigidity was limited and short-lived as collective contracts could be frequently renegotiated due their short duration. All together, the evidence is consistent with the notion that contract staggering during low-inflation recessionary periods with lengthy contract duration 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 once one

takes flows into non-employment into account, minimum wages in collective contracts may have not fully 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 job separations during the 2009 recession while short duration of collective agreements during the 1993 high-inflation recession rendered wage rigidity short-lived and inconsequential. In that sense, an assessment of the employment impacts of collective contracts crucially depends on the distribution of wage cushions and on contract duration. These factors may explain the wide range of estimated elasticities reported in the literature. Third, in presence of particular policies (e.g., short time work schemes) wage rigidity may be accompanied 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.

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Tables and Figures

TABLE 1: Descriptive statistics: 2009 recession, linked CSWH-CCBA sample

	All	Treated (Signed pre=1)	Control (Signed pre=0)	
	mean (s.d.)	mean (s.d.)	mean (s.d.)	Diff. net of prov., sec. FE
	(1)	(2)	(3)	(4)
		Sectoral distribution		
Agriculture (%)	2.120	3.419	1.538	
Industrial sector (%)	30.446	49.886	21.738	
Services sector (%)	67.434	46.695	76.724	
		Collective contract characteristics		
Negotiated wage growth for 2009 (%)	2.339	3.088	2.004	0.614***
Negotiated wage growth for 2009-2010 (%)	4.480	6.176	3.720	1.468***
Multi-year (%)	93.428	100.000	90.484	15.272***
Collective contract duration (in years)	3.572	3.744	3.495	0.345*
	(1.301)	(1.025)	(1.401)	
Escalation clause (%)	54.099	76.271	44.166	15.787***
		Worker characteristics		
Male (%)	58.588	66.765	54.925	1.382*
Age (in years)	38.016	37.946	38.047	-0.127
	(9.997)	(10.138)	(9.933)	
Blue collar workers(%)	51.315	59.868	47.483	0.705
Fixed-term contract (%)	13.156	16.590	11.616	0.676
Part time (%)	14.513	11.833	15.714	-0.414
Monthly actual wage	1521.441	1510.368	1526.401	5.176
	(696.487)	(659.652)	(712.317)	
Tenure (in years)	4.552	4.575	4.542	0.090
	(5.687)	(5.714)	(5.675)	
Short-tenured (%)	37.774	37.918	37.710	0.437
Observations	144068	44570	99497	144068

Note: 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. Short-tenured are workers with at most 2 years of tenure. Column (4) reports the OLS coefficients of a regression of differences between columns (2) and (3), net of province and sector FE.

TABLE 2: Descriptive statistics: 1993 recession, linked CSWH-CCBA sample

	All	Treated (Signed pre=1)	Control (Signed pre=0)	
	mean (s.d.)	mean (s.d.)	mean (s.d.)	Diff. net of prov., sec. FE
	(1)	(2)	(3)	(4)
		Sectoral distribution		
Agricultural sector (%)	0.753	0.167	1.103	
Industrial sector (%)	39.652	41.145	38.757	
Services sector (%)	59.595	58.688	60.139	
		Collective contract characteristics		
Negotiated wage growth for 1993 (%)	5.719	6.360	5.335	0.976***
Multi-year (%)	25.407	34.139	20.174	14.567***
Collective contract duration (in years)	1.291	1.369	1.242	0.116**
	(0.529)	(0.534)	(0.520)	
Escalation clause (%)	61.742	64.009	60.384	5.734
		Worker characteristics		
Male (%)	71.168	73.667	69.671	1.953***
Age (in years)	36.497	36.435	36.534	-0.122
	(10.980)	(10.962)	(10.991)	
Blue collar workers(%)	53.974	56.196	52.642	0.312
Part time (%)	4.807	4.091	5.235	-0.712*
Monthly actual wage	1422.932	1429.149	1419.207	7.628
	(638.867)	(608.398)	(656.425)	
Short-tenured (%)	45.456	45.809	45.245	1.145
Observations	75083	28132	46951	75083

Note: 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. Short-tenured are workers with at most 2 years of tenure. Column (4) reports the OLS coefficients of a regression of differences between columns (2) and (3), net of province and sector FE.

TABLE 3: Effects of rigid contracts on workers' wage growth in 1993 and 2009, DiD estimates by distance from minW

Panel A. 1993 recession			
	Dep. Var.: $\Delta\log(\text{wage})_{1993}$		
	(1)	(2)	(3)
Signed pre×(Wmin≤ W1991m12<1.1Wmin)	0.013**	0.013**	0.016**
	(0.006)	(0.006)	(0.007)
Signed pre×(1.1Wmin≤ W1991m12<1.2Wmin)	0.009*	0.009	0.010
	(0.005)	(0.005)	(0.006)
Signed pre×(1.2Wmin≤ W1991m12<1.4Wmin)	0.003	0.004	0.005
	(0.005)	(0.005)	(0.006)
Observations	8,164	8,164	8,164
R-squared	0.050	0.062	0.152
FE	Province, sector	Province, sector	Collective contract
Controls	No	Yes	Yes
Mean of dep. var.	0.0200	0.0200	0.0200

Panel B. 2009 recession			
	Dep. Var.: $\Delta\log(\text{wage})_{2009}$		
	(1)	(2)	(3)
Signed pre×(Wmin≤ W2007m12<1.1Wmin)	0.019***	0.019**	0.023***
	(0.007)	(0.007)	(0.007)
Signed pre×(1.1Wmin≤ W2007m12<1.2Wmin)	0.016**	0.016**	0.017***
	(0.006)	(0.006)	(0.007)
Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)	0.001	0.001	0.001
	(0.005)	(0.005)	(0.005)
Observations	21,514	21,514	21,514
R-squared	0.018	0.020	0.042
FE	Province, sector	Province, sector	Collective contract
Controls	No	Yes	Yes
Mean of dep. var.	0.0124	0.0124	0.0124

Note:*** p<0.01, ** p<0.05. * p<0.10. Robust s.e. clustered at the 3-digit sector×province (collective contract) level. The Table reports the estimated coefficients of equation 2. 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). Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (1992m12 in panel A or 2008m9 in panel B) and set wages for 1993 in panel A or 2009 in panel B. Minimum wage floors are specific of each occupation (10) and province (50). All regressions include the indicator Signed pre, intercepts for (Wmin≤W1991m12<1.1Wmin), (1.1Wmin≤W1991m12<1.2Wmin) and (1.2Wmin≤W1991m12<1.4Wmin) in panel A and intercepts for (Wmin≤W2007m12<1.1Wmin), (1.1Wmin≤W2007m12<1.2Wmin) and (1.2Wmin≤W2007m12<1.4Wmin) in panel B. Additionally, the regressions in col. (1) include province and 3-digit sector fixed effects, in col. (2) province, 3-digit sector fixed effects, gender, age, and occupation dummies, and in col. (3) collective contract fixed effects, gender, age, and occupation dummies. Omitted category: Signed pre×(1.4Wmin<W1991m12) in panel A and Signed pre×(1.4W2007m12<Wmin) in panel B.

TABLE 4: Effects of rigid collective contracts on workers' probability of non-employment, 2009 recession

	Dep. Var.: Prob(Non employment)	
	(1)	(2)
Signed pre×2006	0.001	0.000
	(0.002)	(0.002)
Signed pre×2007	-0.000	-0.000
	(0.001)	(0.001)
Signed pre×2009	0.003	0.003
	(0.002)	(0.002)
Signed pre×2010	0.009***	0.008***
	(0.003)	(0.003)
Signed pre×2011	0.013***	0.012***
	(0.004)	(0.003)
Signed pre×2012	0.013***	0.013***
	(0.004)	(0.004)
Observations	10,225,126	10,225,126
R-squared	0.093	0.100
Province-Year FE	Yes	Yes
Sector-Year FE	Yes	Yes
Controls	No	Yes
Mean of dep. var.	0.131	0.131

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The Table reports the estimated coefficients of equation 4. The dependent variable is the average monthly probability of non employment in a given year. Sample restricted to workers with at least one year of tenure as of 2008m12. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. All regressions control for year-specific 3-digit sector and year-specific province dummies. Additionally, the regression in col. (2) includes gender, age, and occupation dummies. Omitted year: 2008.

TABLE 5: Effects of rigid collective contracts on workers' probability of non-employment, 2009 recession, heterogeneity by workers' characteristics

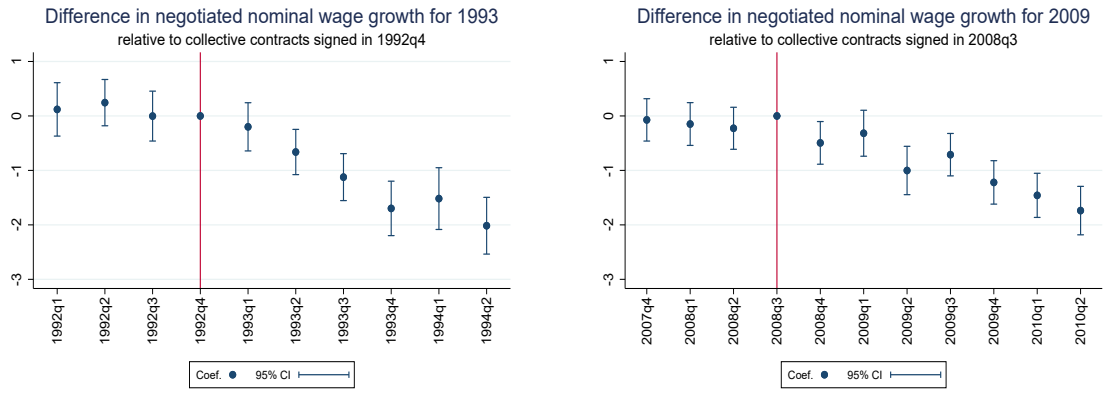
	Dep. Var.: Prob(Non employment)										
	Males (1)	Females (2)	Young (3)	Old (4)	Low skilled (5)	High skilled (6)	Short tenured (7)	Long tenured (8)	Permanent (9)	Small firms (10)	Large firms (11)
Signed pre×2006	-0.000	0.000	-0.002	0.002	-0.001	0.001	-0.004	-0.000	-0.000	-0.002	0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)	(0.000)	(0.002)	(0.002)	(0.002)
Signed pre×2007	-0.000	-0.001	-0.001	-0.001	-0.000	-0.001	-0.003	0.000	-0.000	-0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.001)
Signed pre×2009	0.005**	-0.000	0.004*	0.001	0.003	0.000	0.002	0.003	0.002	0.001	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Signed pre×2010	0.012***	0.003	0.011***	0.004	0.008**	0.004	0.012***	0.006**	0.008***	0.006**	0.008**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Signed pre×2011	0.016***	0.007	0.013***	0.012***	0.011***	0.009**	0.017***	0.009**	0.013***	0.011***	0.012**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.004)	(0.005)
Signed pre×2012	0.015***	0.009	0.014***	0.011**	0.008*	0.013**	0.015**	0.011**	0.015***	0.009**	0.014**
	(0.004)	(0.006)	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.006)
Observations	6,044,827	4,259,182	6,225,632	4,078,377	5,283,022	5,020,987	3,813,594	6,394,402	8,954,882	5,137,373	5,070,623
R-squared	0.109	0.060	0.073	0.117	0.109	0.056	0.078	0.117	0.079	0.098	0.073
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No	No	No	No
Mean of dep. var.	0.139	0.125	0.137	0.127	0.158	0.107	0.201	0.0924	0.121	0.164	0.102

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The Table reports the estimated coefficients of equation 4 for different groups of workers. The dependent variable is the average monthly probability of non employment in a given year. Sample restricted to workers with at least one year of tenure as of 2008m12. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. All regressions control for year-specific 3-digit sector and year-specific province dummies. The sample is further restricted to males in col. (1), females in col. (2), young (40 or below) in col. (3), old (above 40) in col. (4), low skilled (blue collars) in col. (5), medium/high skilled (white collars/college educated, managers) in col. (6), short tenured (at most 2 years of tenure) in col. (7), long tenured (more than 2 years of tenure) in col. (8), permanent in col. (9), small firms (number of employees below or equal to the median) in col. (10), large firms (number of employees above the median) in col. (11). All characteristics refer to 2007m12. Omitted year: 2008.

TABLE 6: Effects of rigid collective contracts on workers' probability of job separation and non-employment, 2009 recession, distinguishing by reason of job separation

	Job separation	Involuntary job separation	Non employed	Non employed after an involuntary job separation
	(1)	(2)	(3)	(4)
Signed pre×2009	0.006	0.003	0.007***	0.002
	(0.004)	(0.003)	(0.003)	(0.002)
Signed pre×2010	0.010**	0.005***	0.012***	0.007***
	(0.004)	(0.002)	(0.004)	(0.002)
Signed pre×2011	0.009**	0.003	0.014***	0.009***
	(0.004)	(0.002)	(0.004)	(0.003)
Signed pre×2012	0.003	0.001	0.012***	0.009***
	(0.005)	(0.001)	(0.004)	(0.003)
Observations	434,890	432,431	576,254	573,753
R-squared	0.045	0.048	0.082	0.039
Province-Year FE	Yes	Yes	Yes	Yes
Sector-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Mean of dep. var.	0.171	0.0604	0.195	0.0881

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The Table reports the estimated coefficients of a variant of equation 4. The dependent variable is the probability of experiencing a job separation from the 2007m12 firm by the end of a given year in col. (1), the probability of experiencing an involuntary job loss (including temporary contracts' expiration) from the 2007m12 firm by the end of a given year in col. (2), the probability of being non-employed by the end of a given year in col. (3), and the probability of being non-employed by the end of a given year after an involuntary job loss in col. (4). Sample restricted to December (m12) of each year and to workers with at least one year of tenure as of 2008m12. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. All regressions control for year-specific 3-digit sector and year-specific province dummies. Omitted year: 2008.

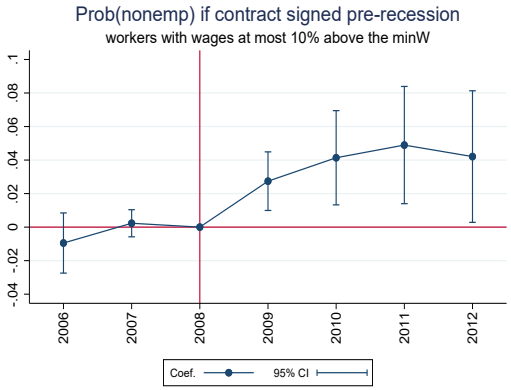


(A) 1993 recession (0=1992q4)

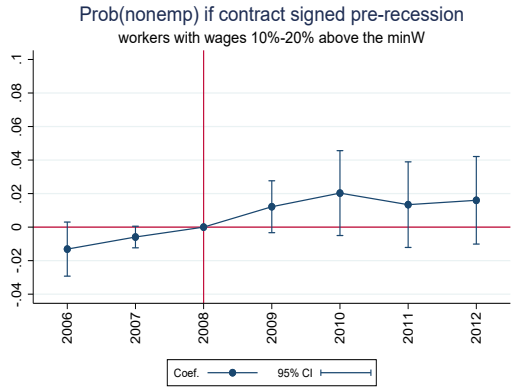
(B) 2009 recession (0=2008q3)

FIGURE 1: Difference in the negotiated wage growth relative to collective contracts signed in the quarter prior to the onset of each recession

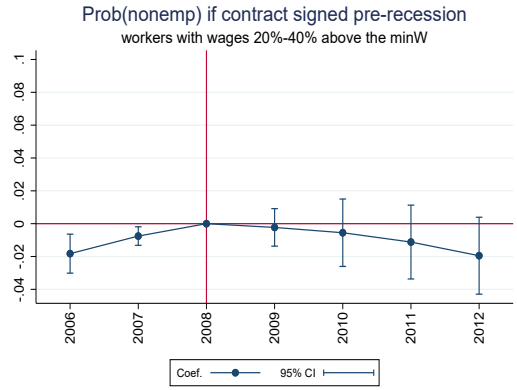
Note: The Figures plot the OLS estimates and 95% confidence intervals of quarter of signature on average nominal wage growth for 1993 (panel A) or 2009 (panel B) set by collective contracts–see equation 1. Contracts signed in 1994 for the 1993 recession (panel A) and in 2010 for the 2009 recession (panel 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 3-digit sector fixed effects. Robust s.e. clustered at the 3-digit sector×province (collective contract) level.



(A) Workers with wages at most 10% above the minW



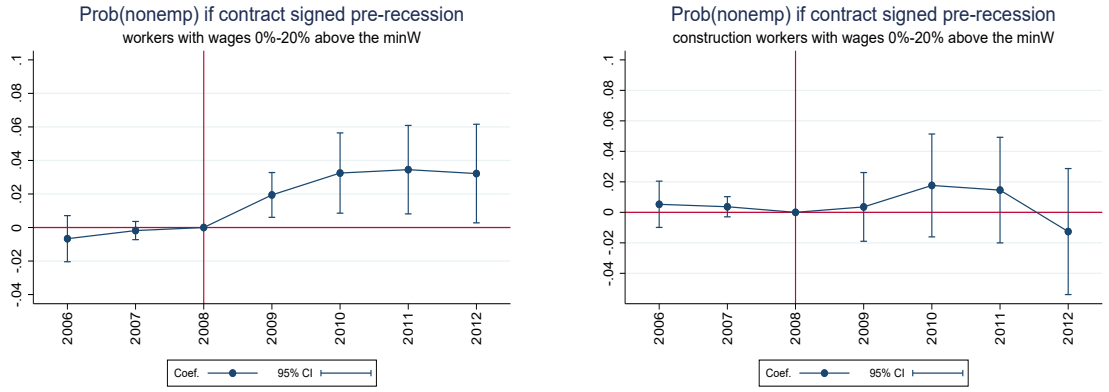
(B) Workers with wages 10-20% above the minW



(C) Workers with wages 20-40% above the minW

FIGURE 2: Effect of rigid collective contracts on workers' probability of non-employment, 2009 recession, DiD estimates by distance from minW

Note: The Figure on the left plots the OLS estimates and 95% confidence intervals of the interaction term "Signed pre×(Wmin≤ W2007m12<1.1Wmin)" in an event study. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working 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 "Signed pre×(1.1Wmin≤W2007m12<1.2Wmin)" from the same regression 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 "Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)" from the same regression as above but refer to workers whose pre-recession earnings were 20-40% above the minimum wages. All regressions control for collective contract×year fixed effects. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. Minimum wage floors are specific of each occupation (10) and province (50). Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted category: Signed pre×(1.4Wmin≤ W2007m12). Omitted year: 2008. See Table A.5 for all estimates.



(A) Workers in any sector with wages at most 20% above the minW

(B) Workers in the construction sector with wages at most 20% above the minW

FIGURE 3: Effect of rigid collective contracts on workers' probability of non-employment, 2009 recession, case study with the construction sector

Note: The Figure on the left plots the OLS estimates and 95% confidence intervals of the interaction term "Signed pre×(Wmin≤ W2007m12<1.2Wmin)" in an event study. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working in any sector as of 2008m12. The estimates refer to workers whose pre-recession earnings 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 "Signed pre×(Wmin≤W2007m12<1.2Wmin)" in an event study. The dependent variable is the yearly probability of non-employment among individuals who were working in the construction sector as of 2008m12. In the construction sector, collective contracts are signed at different dates at the province level but most of them set the same wage growth nationwide. The estimates refer to workers whose pre-recession earnings were at most 20% above the minimum wages. All regressions control for collective contract×year fixed effects. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. Minimum wage floors are specific of each occupation (10) and province (50). Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted category: (W2007m12≥1.4Wmin), omitted year: 2008.

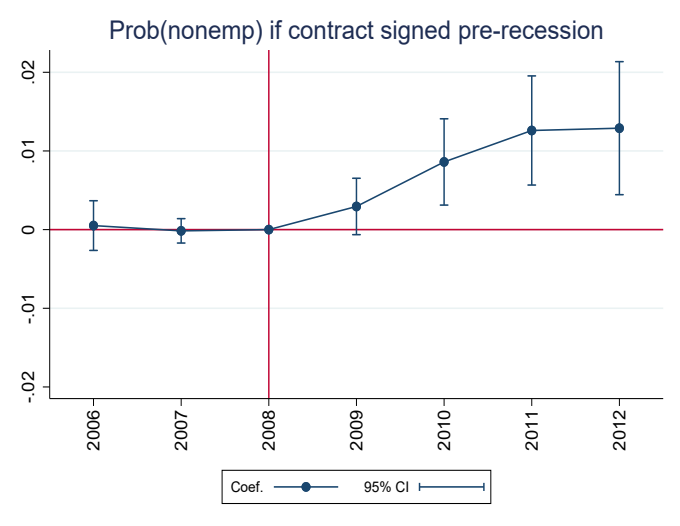


FIGURE 4: Effect of rigid collective contracts on workers' probability of non-employment, 2009 recession, without distinguishing by distance from minW

Note: The Figure plots the OLS estimates and 95% confidence intervals of the term "Signed pre" in an event study. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working as of 2008m12. All regressions control for 3-digit sector×year and province×year fixed effects. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted year: 2008. See Table 4 for all estimates.

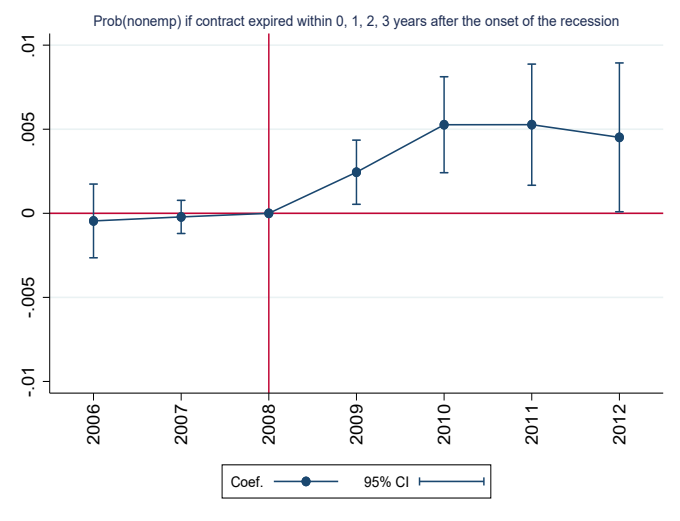
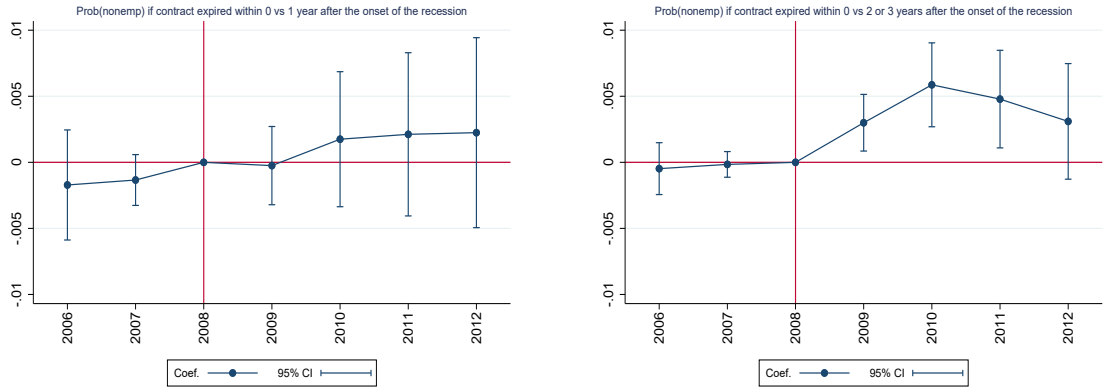


FIGURE 5: Effect of rigid collective contracts on workers' probability of non-employment, 2009 recession, robustness in the definition of rigidity (expiration instead of signature of collective contracts)

Note: The Figure plots the OLS estimates and 95% confidence intervals of the term "Expired pre" in an event study. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working as of 2008m12. All regressions control for 3-digit sector×year and province×year fixed effects. "Expired pre" is an ordinal variable whose values range from 0 for province-sector collective agreements that expired by the end of 2008 (no wage rigidity) to 3 for collective contracts that expired by the end of 2011 (prolonged wage rigidity). Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted year: 2008. See Table A.6, column 1, for all estimates.



(A) Workers covered by short contracts (B) Workers covered by long contracts

FIGURE 6: Effect of rigid collective contracts on workers' probability of non-employment, 2009 recession, robustness in the definition of rigidity (expiration instead of signature of collective contracts) and heterogeneity by collective contract duration

Note: The Figure on the left plots the OLS estimates and 95% confidence intervals of the term "Expired pre" in an event study. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working as of 2008m12. The sample includes workers covered by collective contracts that expired immediately after the onset of the recession (by the end of 2008, Signed pre=0) or shortly after (by the end of 2009, Signed pre=1). The Figure on the right plots the OLS estimates and 95% confidence intervals of the term "Expired pre" in an event study. The dependent variable is the yearly probability of non-employment among individuals who were working as of 2008m12. The sample includes workers covered by collective contracts that expired immediately after the onset of the recession (by the end of 2008, Signed pre=0) or long after (by the end of 2010 or 2011, Signed pre=2 or 3). All regressions control for 3-digit sector×year and province×year fixed effects. "Expired pre" is an ordinal variable whose values range from 0 for province-sector collective agreements that expired by the end of 2008 (no wage rigidity) to 3 for collective contracts that expired by the end of 2011 (prolonged wage rigidity). Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted year: 2008. See Table A.6, columns 2 and 3, for all estimates.

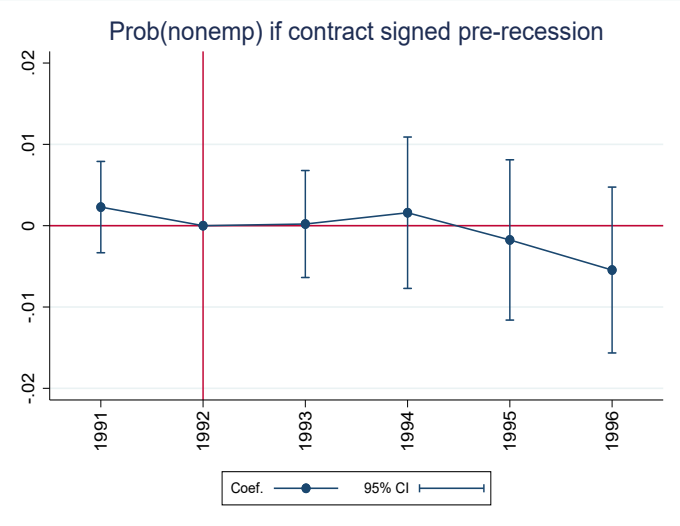


FIGURE 7: Effect of rigid collective contracts on workers' probability of non-employment, 1993 recession

Note: The Figure plots the OLS estimates and 95% confidence intervals of the term "Signed pre" in an event study. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working in any sector as of 1992m12. All regressions control for 3-digit sector×year and province×year fixed effects. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (1992m12) and set wages for 1993 onwards. Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted year: 1992. See Table A.9, column 1, for all estimates.

Staggered Contracts and Unemployment during Recessions: Online Appendix

A Appendix. More on the empirical analysis

TABLE A.1: Descriptive statistics: 2009 recession, linked CSWH-CCBA sample versus linked CSWH-CCBA-wage floor sample

	CSWH-CCBA sample	CSWH-CCBA-wage floor sample
	mean (s.d.)	mean (s.d.)
	(1)	(2)
	Sectoral distribution	
Agriculture (%)	2.120	1.155
Industrial sector (%)	30.446	42.181
Services sector (%)	67.434	56.664
	Collective contract characteristics	
Negotiated wage growth for 2009 (%)	2.339	2.479
Negotiated wage growth for 2009-2010 (%)	4.480	4.686
Multi-year (%)	93.428	92.112
Collective contract duration (in years)	3.572	3.632
	(1.301)	(1.288)
Escalation clause (%)	54.099	59.455
	Worker characteristics	
Male (%)	58.588	68.786
Age (in years)	38.016	37.563
	(9.997)	(9.467)
Blue collar workers(%)	51.315	54.720
Fixed-term contract (%)	13.156	11.222
Part time (%)	14.513	-
Monthly actual wage	1521.441	1675.029
	(696.487)	(628.436)
Tenure (in years)	4.552	4.692
	(5.687)	(5.713)
Short-tenured (%)	37.774	36.903
		Distribution of wage cushion
W2007m12<Wmin		7.859
Wmin≤ W2007m12<1.1Wmin		9.394
1.1Wmin≤ W2007m12<1.2Wmin		11.827
1.2Wmin≤ W2007m12<1.4Wmin		18.902
1.4Wmin≤ W2007m12		52.018
Observations	144068	58545

Note: Sample of workers' Social Security records linked to their corresponding provincial collective contract (col. 1) and with information on minimum wage floors (col. 2). The sample in col. 2 is resticted to full-time employees. 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. Short-tenured are workers with at most 2 years of tenure. The wage cushion is the distance between actual wages and the worker-specific wage floor as of 2007m12. Minimum wage floors are specific of each occupation (10) and province (50).

TABLE A.2: Descriptive statistics: 1993 recession, linked CSWH-CCBA sample versus linked CSWH-CCBA-wage floor sample

	CSWH-CCBA sample	CSWH-CCBA-wage floor sample
	mean (s.d.)	mean (s.d.)
	(1)	(2)
	Sectoral distribution	
Agricultural sector (%)	0.753	0.895
Industrial sector (%)	39.652	46.213
Services sector (%)	59.595	52.892
	Collective contract characteristics	
Negotiated wage growth for 1993 (%)	5.719	5.665
Multi-year (%)	25.407	26.171
Collective contract duration (in years)	1.291	1.287
	(0.529)	(0.501)
Escalation clause (%)	61.742	64.650
	Worker characteristics	
Male (%)	71.168	72.293
Age (in years)	36.497	36.201
	(10.980)	(10.848)
Blue collar workers(%)	53.974	52.724
Part time (%)	4.807	-
Monthly actual wage	1422.932	1459.435
	(638.867)	(632.132)
Short-tenured (%)	45.456	44.943
	Distribution of wage cushion	
W1991m12<Wmin		21.703
Wmin≤ W1991m12<1.1Wmin		19.190
1.1Wmin≤ W1991m12<1.2Wmin		8.452
1.2Wmin≤ W1991m12<1.4Wmin		12.429
1.4Wmin≤ W1991m12		38.226
Observations	75083	45137

Note: Sample of workers' Social Security records linked to their corresponding provincial collective contract (col. 1) and with information on minimum wage floors (col. 2). The sample in col. 2 is resticted to full-time employees. 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. Short-tenured are workers with at most 2 years of tenure. The wage cushion is the distance between actual wages and the worker-specific wage floor as of 1991m12. Minimum wage floors are specific of each occupation (10) and province (50).

TABLE A.3: Percentage of workers close to the minimum wage floors in 2007m12 in various subgroups

Subgroup	% workers close to the minima (1)
By gender	
Females	9.90
Males	8.27
By age in 2007m12	
Below or equal 40	10.27
Above 40	7.97
By skill in 2007m12	
Low skilled	12.47
Middle/high skilled	5.68
By tenure in 2007m12	
At most 2 years	11.78
More than two years	7.98
By type of contract in 2007m12	
Temporary	15.31
Permanent	8.65

Note: Percentage of workers with Wmin$\leq$W2007m12$<$1.1Wmin in subgroups by gender, age, skill, tenure, and type of contract. Minimum wage floors are specific of each occupation (10) and province (50).

TABLE A.4: Percentage of workers close to the minimum wage floors in 1991m12 in various subgroups

Subgroup	% workers close to the minima (1)
By gender	
Females	19.96
Males	16.96
By age in 1991m12	
Below or equal 40	20.73
Above 40	15.75
By skill in 1991m12	
Low skilled	24.65
Middle/high skilled	12.97
By tenure in 1991m12	
At most 2 years	21.69
More than two years	16.92

Note: Percentage of workers with Wmin$\leq$W1991m12$<$1.1Wmin in subgroups by gender, age, skill, and tenure. Minimum wage floors are specific of each occupation (10) and province (50).

TABLE A.5: Effects of rigid collective contracts on workers' probability of non-employment, 2009 recession, DiD estimates by workers' distance from minW

	Dep. Var.: Prob(Non employment)	
	(1)	(2)
Signed pre×(Wmin≤ W2007m12<1.1Wmin)×2006	-0.009	-0.009
	(0.009)	(0.009)
Signed pre×(Wmin≤ W2007m12<1.1Wmin)×2007	0.002	0.003
	(0.004)	(0.004)
Signed pre×(Wmin≤ W2007m12<1.1Wmin)×2009	0.027***	0.028***
	(0.009)	(0.009)
Signed pre×(Wmin≤ W2007m12<1.1Wmin)×2010	0.041***	0.042***
	(0.014)	(0.014)
Signed pre×(Wmin≤ W2007m12<1.1Wmin)×2011	0.049***	0.050***
	(0.018)	(0.018)
Signed pre×(Wmin≤ W2007m12<1.1Wmin)×2012	0.042**	0.043**
	(0.020)	(0.020)
Signed pre×(1.1Wmin≤ W2007m12≤1.2Wmin)×2006	-0.013	-0.013
	(0.008)	(0.008)
Signed pre×(1.1Wmin≤ W2007m12≤1.2Wmin)×2007	-0.006*	-0.006*
	(0.003)	(0.003)
Signed pre×(1.1Wmin≤ W2007m12≤1.2Wmin)×2009	0.012	0.013
	(0.008)	(0.008)
Signed pre×(1.1Wmin≤ W2007m12≤1.2Wmin)×2010	0.020	0.021
	(0.013)	(0.013)
Signed pre×(1.1Wmin≤ W2007m12≤1.2Wmin)×2011	0.013	0.014
	(0.013)	(0.013)
Signed pre×(1.1Wmin≤ W2007m12≤1.2Wmin)×2012	0.016	0.016
	(0.013)	(0.013)
Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)×2006	-0.018***	-0.018***
	(0.006)	(0.006)
Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)×2007	-0.008***	-0.007**
	(0.003)	(0.003)
Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)×2009	-0.002	-0.002
	(0.006)	(0.006)
Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)×2010	-0.006	-0.005
	(0.010)	(0.011)
Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)×2011	-0.011	-0.011
	(0.011)	(0.012)
Signed pre×(1.2Wmin≤ W2007m12<1.4Wmin)×2012	-0.020	-0.019
	(0.012)	(0.012)
Observations	3,864,249	3,864,249
R-squared	0.116	0.117
Collective contract-Year FE	Yes	Yes
Controls	No	Yes
Mean of dep. var.	0.128	0.128

Note:*** p<0.01, ** p<0.05. * p<0.10. Robust s.e. clustered at the 3-digit sector×province (collective contract) level. The Table reports the estimated coefficients of equation 3. The dependent variable is the average monthly probability of non employment in a given year. Sample restricted to full-time employees with available information on their corresponding minimum wage floor and with at least one year of tenure as of 2008m12. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. Minimum wage floors are specific of each occupation (10) and province (50). All regressions include intercepts for (Wmin≤W2007m12<1.1Wmin), (1.1Wmin≤W2007m12≤1.2Wmin) and (1.2Wmin≤W2007m12<1.4 Wmin) interacted with the year dummies, "Signed pre" interacted with the year dummies, and collective contract×year fixed effects. Additionally, the regression in col. (2) includes gender and age dummies. Omitted category: Signed pre×(W2007m12≥1.4Wmin), omitted year: 2008.

TABLE A.6: Effects of rigid collective contracts on workers' probability of non-employment, 2009 recession, expiration instead of signature and heterogeneity by collective contract duration

	Dep. Var.: Prob(Non employment)		
	All contracts (1)	Short contracts (2)	Long contracts (3)
Expired pre×2006	-0.000	-0.002	-0.000
	(0.001)	(0.002)	(0.001)
Expired pre×2007	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.000)
Expired pre×2009	0.002**	-0.000	0.003***
	(0.001)	(0.002)	(0.001)
Expired pre×2010	0.005***	0.002	0.006***
	(0.001)	(0.003)	(0.002)
Expired pre×2011	0.005***	0.002	0.005**
	(0.002)	(0.003)	(0.002)
Expired pre×2012	0.005**	0.002	0.003
	(0.002)	(0.004)	(0.002)
Observations	9,353,952	7,182,381	7,707,590
R-squared	0.082	0.062	0.081
Province-Year FE	Yes	Yes	Yes
Sector-Year FE	Yes	Yes	Yes
Controls	No	No	No
Mean of dep. var.	0.140	0.120	0.132

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The dependent variable is the average monthly probability of non employment in a given year. Sample restricted to workers with at least one year of tenure as of 2008m12. "Expired pre" is an ordinal variable whose values range from 0 for province-sector collective agreements that expired by the end of 2008 (no wage rigidity) to 3 for collective contracts that expired by the end of 2011 (prolonged wage rigidity. All regressions control for year-specific 3-digit sector and year-specific province dummies. The sample in col. (1) includes workers covered by any collective contract, in col. (2) only workers covered by collective contracts that expired immediately after the onset of the recession (by the end of 2008, Signed pre=0) or shortly after (by the end of 2009, Signed pre=1), and in col. (3) only workers covered by collective contracts that expired immediately after the onset of the recession (by the end of 2008, Signed pre=0) or long after (by the end of 2010 or 2011, Signed pre=2 or 3). Omitted year: 2008.

TABLE A.7: Difference in the negotiated wage growth for 2009 relative to collective contracts signed prior to the onset of the 2009 recession, construction versus other sectors

	Dep. var.: Negotiated wage growth for 2009	
	Construction	All other sectors
	(1)	(2)
Signed pre	0.329	1.083***
	(0.294)	(0.150)
Observations	40	529
R-squared	0.270	0.629
Collective contract FE	Yes	Yes
Mean of dep. var.	3.488	2.342

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The dependent variable is the average nominal wage growth for 2009 set by collective contracts. The regression in col. (1) only includes collective contracts in the construction sector while the regression in col. (2) includes collective contracts in all other sectors. Signed pre=1 if the collective contract got signed before the onset of the recession (2008m9) and set wages for 2009. All regressions control for collective contract fixed effects.

TABLE A.8: Effects of rigid contracts on workers' wage growth in 1993, by presence of escalation clauses

	Dep. Var.: $\Delta\log(\text{wage})_{1993}$		
	All contracts	Contracts with escalation clauses	Contracts without escalation clauses
	(1)	(2)	(3)
Signed pre×(Wmin≤ W1991m12<1.1Wmin)	0.013**	0.009	0.027**
	(0.006)	(0.007)	(0.012)
Signed pre×(1.1Wmin≤ W1991m12<1.2Wmin)	0.009*	0.012*	0.004
	(0.005)	(0.006)	(0.010)
Signed pre×(1.2Wmin≤ W1991m12<1.4Wmin)	0.003	0.005	0.004
	(0.005)	(0.006)	(0.010)
Observations	8,164	5,341	2,823
R-squared	0.050	0.059	0.084
FE	Province, sector	Province, sector	Province, sector
Controls	No	No	No
p-value of equality of coefficients test	0.121	0.317	0.065
Mean of dep. var.	0.0200	0.0211	0.0180

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The Table reports the estimated coefficients of equation 2. The dependent variable is nominal wage growth in 1993. Sample restricted to full-time employees with available information on their corresponding minimum wage floor, who stayed in the same firm all through 1993. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (1992m12) and set wages for 1993 onwards. Minimum wage floors are specific of each occupation (10) and province (50). All regressions include the indicator Signed pre, intercepts for (Wmin≤W1991m12<1.1Wmin), (1.1Wmin≤W1991m12<1.2Wmin) and (1.2Wmin≤W1991m12<1.4Wmin), province and 3-digit sector fixed effects. The sample in col. (1) includes workers covered by any collective contract, in col. (2) only workers covered by collective contracts with escalation clauses, and in col. (3) only workers covered by collective contracts without escalation clauses.

TABLE A.9: Effects of rigid collective contracts on workers' probability of non-employment, 1993 recession, by presence of escalation clauses

	Dep. Var.: Prob(Non employment)		
	All contracts	Contracts with escalation clauses	Contracts without escalation clauses
	(1)	(2)	(3)
Signed pre×1991	0.002	-0.000	0.007
	(0.003)	(0.003)	(0.013)
Signed pre×1993	0.000	-0.000	0.001
	(0.003)	(0.004)	(0.005)
Signed pre×1994	0.002	0.005	0.003
	(0.005)	(0.007)	(0.006)
Signed pre×1995	-0.002	0.004	-0.000
	(0.005)	(0.007)	(0.008)
Signed pre×1996	-0.005	-0.000	-0.003
	(0.005)	(0.008)	(0.010)
Observations	4,504,980	2,759,875	1,745,104
R-squared	0.058	0.059	0.070
Province-Year FE	Yes	Yes	Yes
Sector-Year FE	Yes	Yes	Yes
Controls	No	No	No
Mean of dep. var.	0.179	0.180	0.176

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The Table reports the estimated coefficients of equation 4. The dependent variable is the average monthly probability of non employment in a given year. Sample restricted to workers with at least one year of tenure as of 1992m12. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (1992m12) and set wages for 1993 onwards. All regressions control for year-specific 3-digit sector and year-specific province dummies. The sample in col. (1) includes workers covered by any collective contract, in col. (2) only workers covered by collective contracts with escalation clauses, and in col. (3) only workers covered by collective contracts without escalation clauses. Escalation clause takes value one if the contract stipulates an adjustment for realized inflation whenever it exceeds a threshold level. Omitted year: 1992.

TABLE A.10: Effects of rigid collective contracts on workers' probability of non-employment, 2009 and 1993 recession, intensive margin

Panel A. 1993 recession	
Dep. var.:	Prob(Part time) if employed
	(1)
Signed pre×1991	-0.006
	(0.003)
Signed pre×1993	-0.006
	(0.004)
Signed pre×1994	-0.006
	(0.004)
Signed pre×1995	-0.005
	(0.004)
Signed pre×1996	-0.005
	(0.004)
Observations	3,700,339
R-squared	0.060
Province-Year FE	Yes
Sector-Year FE	Yes
Controls	Yes
Mean of dep. var.	0.0458
Panel B. 2009 recession	
Dep. var.:	Prob(Part time) if employed
	(1)
Signed pre×2006	-0.002
	(0.003)
Signed pre×2007	-0.002
	(0.003)
Signed pre×2009	-0.002
	(0.003)
Signed pre×2010	-0.004
	(0.003)
Signed pre×2011	-0.002
	(0.004)
Signed pre×2012	-0.002
	(0.004)
Observations	8,930,851
R-squared	0.169
Province-Year FE	Yes
Sector-Year FE	Yes
Controls	Yes
Mean of dep. var.	0.153

Note: Robust s.e. clustered at the 3-digit sector×province (collective contract) level, *p<.10; **p<.05; ***p<.01. The dependent variable is the average monthly probability of working part time in a given year (if employed). Sample restricted to workers currently employed and with at least one year of tenure as of 1991m12 (panel A) or 2008m12 (panel B). Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of each recession (1992m12 in panel A or 2008m9 in panel B) and set wages for 1993 or 2009 onwards. All regressions control for year-specific 3-digit sector and year-specific province dummies. Additional controls: gender. Omitted year: 1992 in panel A and 2008 in panel B.

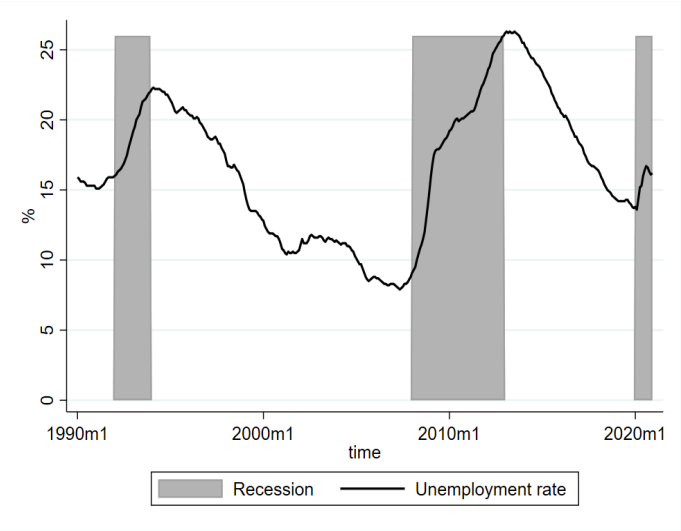


FIGURE A.1: Unemployment rate in Spain, 1990m1-2020m12

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).

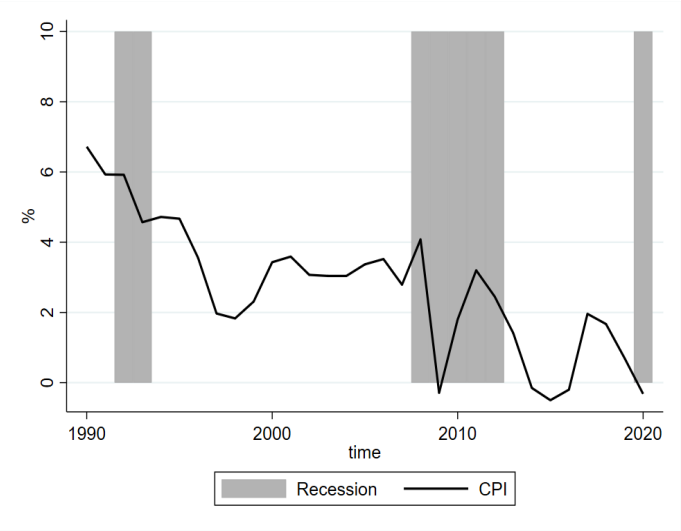
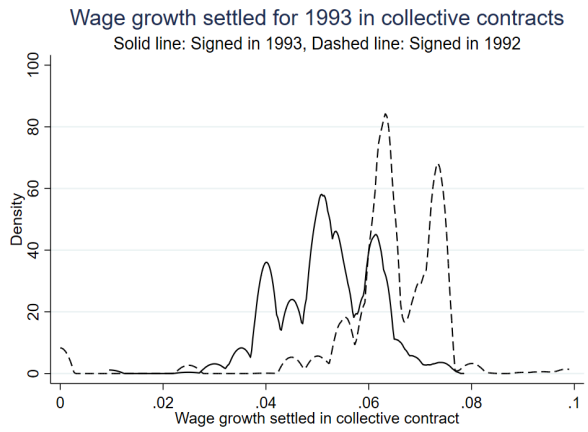
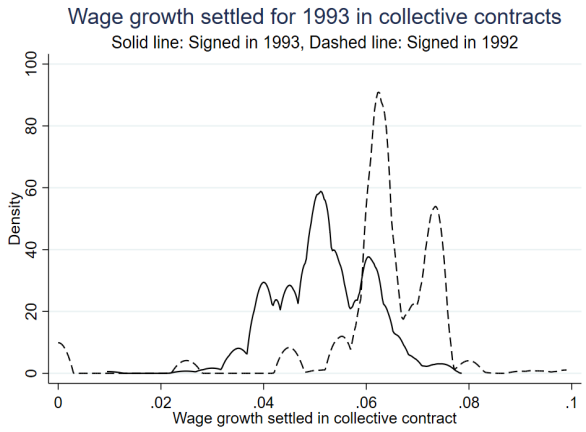


FIGURE A.2: Consumer Price Index in Spain, 1990-2020

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.

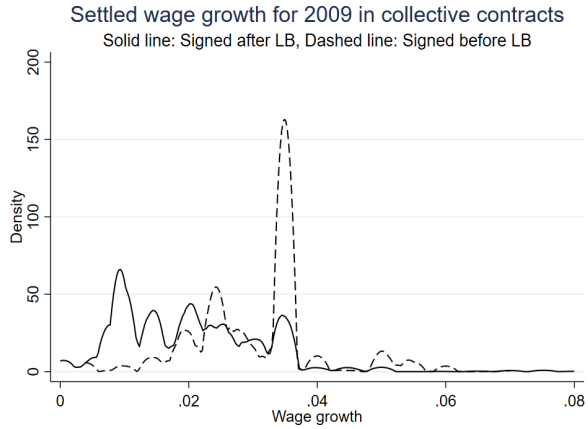


(A) Full sample

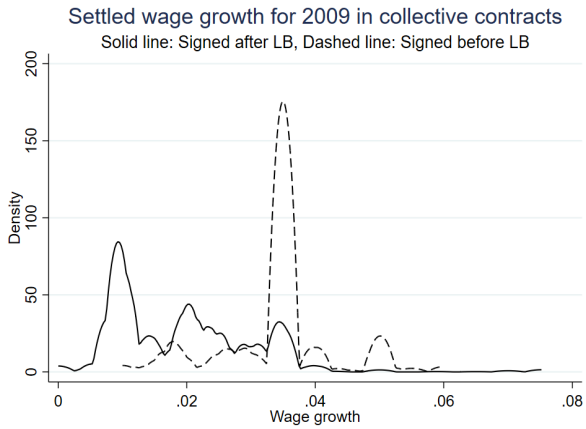


(B) Subsample with wage floors

1993 recession



(C) Full sample



(D) Subsample with wage floors

2009 recession

FIGURE A.3: Wage growth settled in collective contracts

Note: 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.

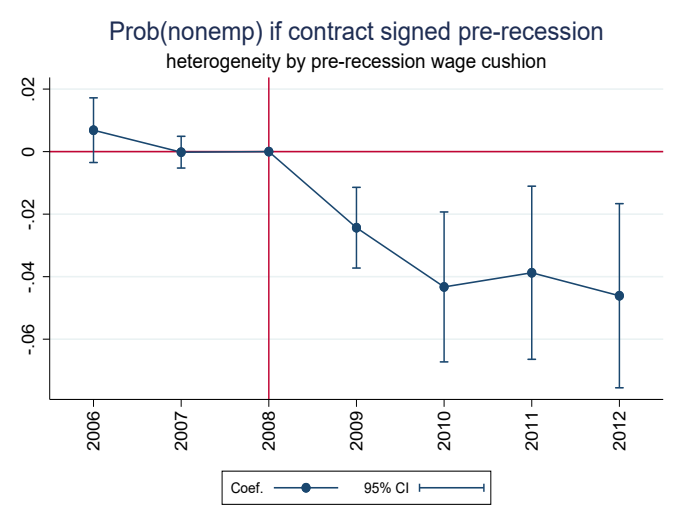
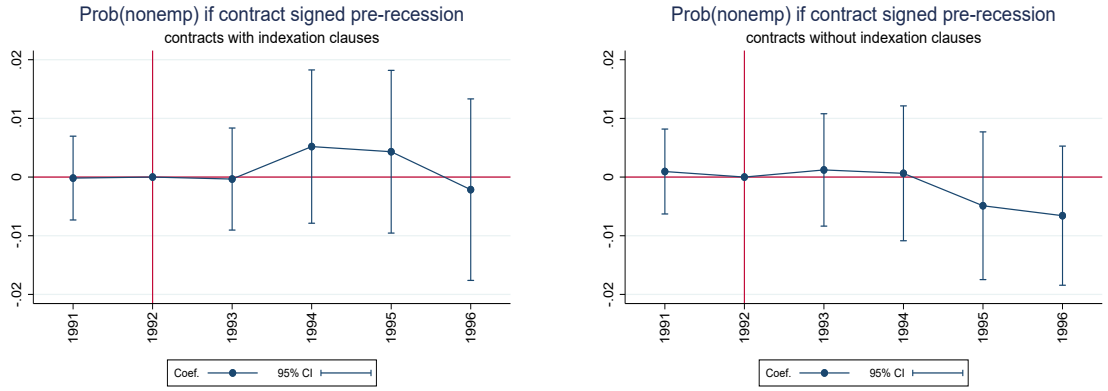


FIGURE A.4: Effect of rigid collective contracts on workers' probability of non-employment, 2009 recession, by pre-recession wage cushion

Note: The Figure plots the OLS estimates and 95% confidence intervals of the term "Signed pre×Wage cushion 2007m12" in an event study. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working as of 2008m12. The wage cushion is the distance between actual wages and the worker-specific wage floor as of 2007m12. All regressions control for collective contract×year fixed effects. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (2008m9) and set wages for 2009 onwards. Minimum wage floors are specific of each occupation (10) and province (50). Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted year: 2008.



(A) Workers covered by contracts with indexation clauses

(B) Workers covered by contracts without indexation clauses

FIGURE A.5: Effect of rigid collective contracts on workers' probability of non-employment, 1993 recession, heterogeneity by presence of indexation clauses in the collective contract

Note: The Figure on the left plots the OLS estimates and 95% confidence intervals of the term "Signed pre" in an event study. The estimates refer to workers who were covered by collective contracts with indexation clauses. The dependent variable is the average monthly probability of non-employment in a given year among individuals who were working in any sector as of 1992m12. The Figure on the right plots the OLS estimates and 95% confidence intervals of the term "Signed pre" in an event study. The estimates refer to workers who were covered by collective contracts without indexation clauses. The dependent variable is the yearly probability of non-employment among individuals who were working in any sector as of 1992m12. All regressions control for 3-digit sector×year and province×year fixed effects. Rigid contracts (Signed pre=1) are province-sector collective agreements that got signed before the onset of the recession (1992m12) and set wages for 1993 onwards. Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Omitted year: 1992. See Table A.9, columns 2 and 3, for all estimates.

B Appendix. More on the institutions

B.1 Collective Contracts

B.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 a sector. 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, 2013). 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.[45] 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.

B.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. Figure B.1 provides an example of minimum wage floors set in the construction sector 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 get renewed in different months and set minimum wages and working conditions for a pre-specified period, typically expiring in December of a given year (see Figure B.2). Collective contract duration varies over the business cycle but 93% of contracts signed in the period around the onset of the 2009 recession had a validity period that exceeded one year –see Table 1. This fraction was much smaller (25%) for contracts signed in the period around the onset of the 1993 recession (a period of high inflation) –see Table 2. Concordantly, the average contract duration was much shorter in 1993 (1.3 years) than in the 2009 recession (3.6

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

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. During the period of our analysis, the interpretation by the Supreme Court in such cases was that all firms within the scope of the agreement were still subject to the working conditions and minimum wages set in the expired agreement –see Ministerio de Trabajo (2008).

B.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 B.3 shows an example of an escalation clause in the 1993 collective contract of the meat sector. First, 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. 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 in theory permit a firm to opt-out from a collective contract. Namely, in a period of economic hardship, opting out from a sectoral agreement may be 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, a labor reform in 2012 attempted to determine verifiable conditions that permit opt-outs, the reason being that the procedure was cumbersome.

On top of sectoral agreements, worker representatives and managers may negotiate wages and other working conditions in firm-level contracts. Around 15% of workers subject to an 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).

B.2 Other Institutions: EPL and short time work

The Spanish labor market is characterized by duality (Bentolila and Dolado, 1994). According to INE, more than 29% of workers were temporary in 2008, for whom no firing costs apply in case of lay-offs upon contract expiration or end of the task (Barceló and Villanueva, 2016). By contrast, permanent workers are under Employment Protection Legislation (EPL) and in case of lay-offs they are entitled to severance payments. The exact amount of the severance payment depends on workers' tenure (33 days of salary per year worked, up to a maximum of 24 months' salary).

In Spain, there are also short-time work schemes (Boeri and Bruecker, 2011) that firms can resort to during periods of hardship. These schemes, known as "Expediente Temporal de Regulación de Empleo", 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). In particular, the Spanish government implemented a dedicated program in response to unforeseeable circumstances, aimed at assisting businesses directly impacted by the mobility restrictions enforced during the state of emergency. All employees of qualifying firms were entitled to assistance, regardless of their tenure with the company or employment contract type. They received a benefit equivalent to 70 percent of their last salary, capped at approximately EUR 1,100 for the hours not worked. Additionally, full social security rebates were provided to firms free of charge.

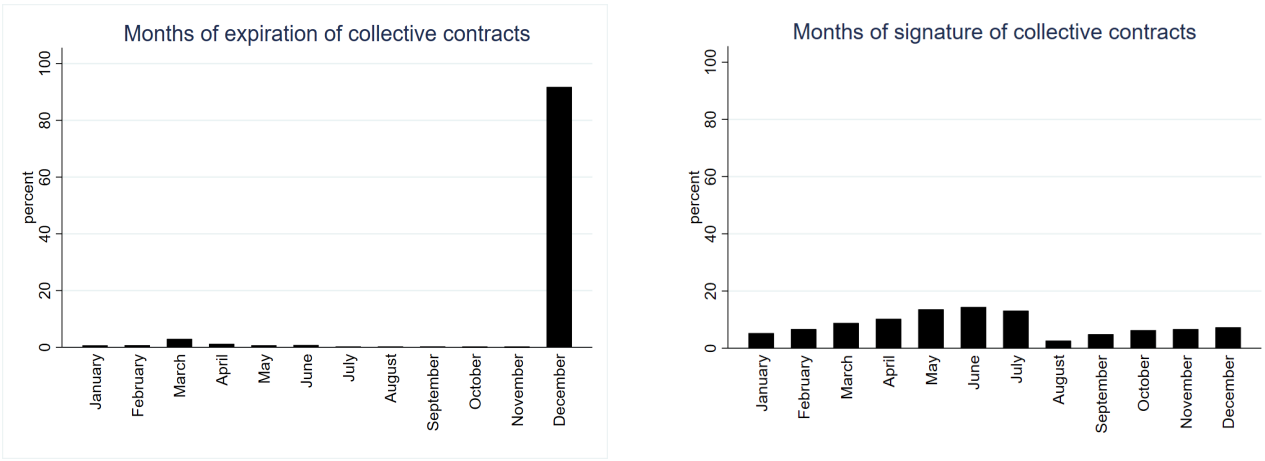
TABLA DE RETRIBUCIONES BRUTAS DEL CONVENIO DE LA CONSTRUCCIÓN Y OBRAS PÚBLICAS DE NAVARRA

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

FIGURE B.1: An example of minimum wage floors set in a collective agreement (construction sector in Navarre, 2010)

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

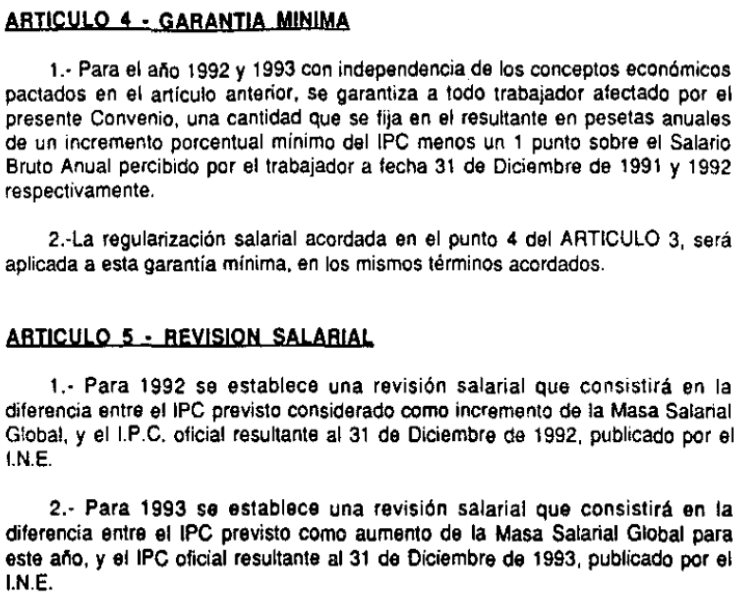


(A) Months of expiration

(B) Months of signature

FIGURE B.2: Months of expiration and signature of collective contracts, 1990-2010

Note: 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 in the end of December, signature dates are distributed fairly homogeneously across months.



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 B.3: Extract of an escalation clause in a collective contract signed in 1993

Note: The Figure shows an extract of an escalation clause in the 1993 collective contract of the meat sector. 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.

C Appendix. 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. Importantly for our analysis, 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.).[46] As Figure C.1 shows, there has been an unprecedented use of short-time work schemes in 2020.

Using the information on province and sector of activity we are able to match workers in EPA to the register of collective contracts. 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 C.2). 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 C.3, 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 C.3, 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 estimate two regressions (LPMs) on repeated cross sections:

$$Y_{ispt} = \alpha_{0t} + \alpha_{1t} \sum_{\substack{q=-9 \\ q\neq 0}}^{+3} \alpha_q 1(SIGNED_{sp} = q) + \gamma X_{it} + sector_s + province_p \times quarter_t + \epsilon_{ispt}.$$

In the first regression, the dependent variable, Y_{ispt}, takes the value 1 if worker i, who is employed in sector s and province p is in short time work in quarter t and 0 otherwise. In the second regression, the dependent variable, Y_{ispt}, takes the value 1 if worker i, who is/used to be employed in sector s and province p is non-employed in quarter t and 0 otherwise. $1(SIGNED_{sp} = q)$ is the quarter of signature of the collective contract in sector s and province p. The last pre-recession quarter of signature of the collective contract (q=0) is 2020q1 (quarter before the onset of the COVID-19 pandemic). The regressions include sector and province×quarter of the survey fixed effects and use survey weights. X_{it} 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.

Figure C.4, 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 C.4, right panel). Our findings suggest that wage rigidity during the COVID-19 pandemic led to employment adjustments at

[46] This type of information is not readily available in the CSWH.

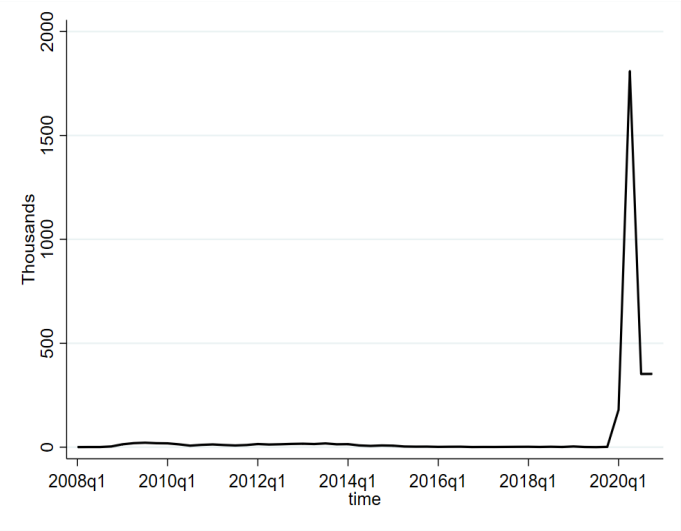
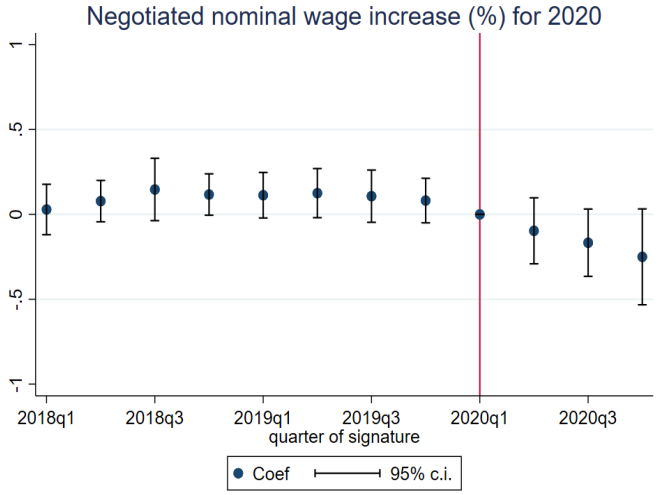


FIGURE C.1: Number of workers in short time work (STW) in Spain, 2008q1-2020q4

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).

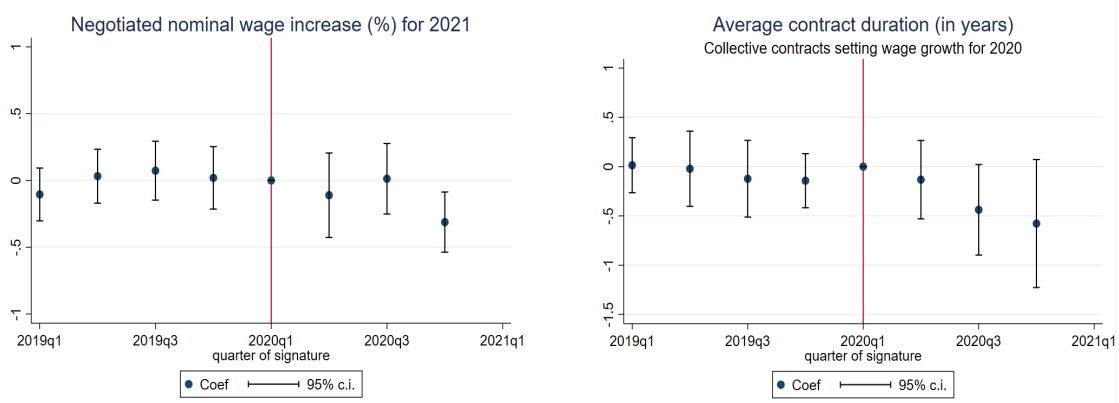
the intensive rather than the extensive margin.



COVID-19 pandemic (0=2020q1)

FIGURE C.2: Negotiated wage growth for 2020 by quarter of signature before and after the onset of the COVID-19 pandemic

Note: 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 sector fixed effects. Robust s.e. clustered at the 3-digit sector×province level.



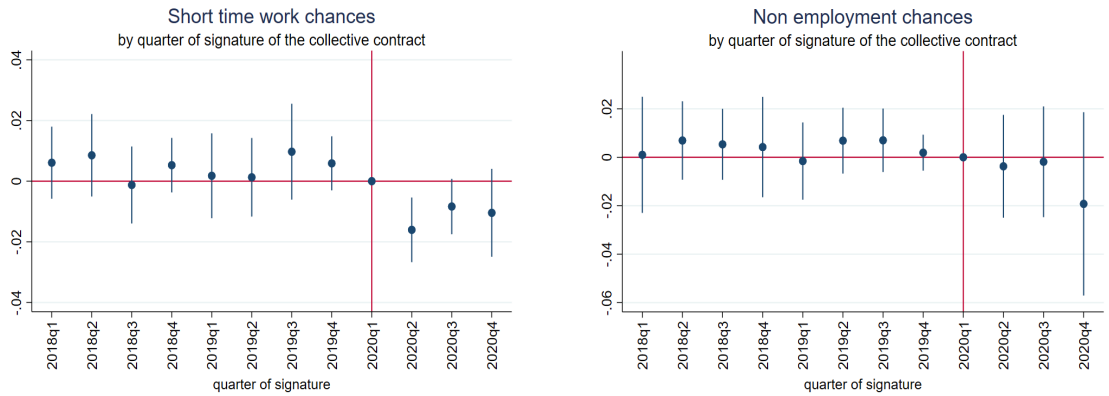
(A) Workers in any sector with wages at most 20% above the minW

(B) Workers in the construction sector with wages at most 20% above the minW

COVID-19 pandemic (0=2020q1)

FIGURE C.3: Negotiated wage growth for 2021 and contract duration by quarter of signature before and after the onset of the COVID-19 pandemic

Note: 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 sector fixed effects. Robust s.e. clustered at the sector×province level.



COVID-19 pandemic (0=2020q1)

FIGURE C.4: Short time work and non employment by quarter of signature before and after the onset of the COVID-19 pandemic

Note: 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 3-digit sector, and province×quarter fixed effects. Robust s.e. clustered at the 3-digit sector×province (collective contract) level. Survey weights used.

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