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The employment effects of collective wage bargaining☆

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ABSTRACT

This study examines the wage and employment effects of Italian collective wage bargaining. It analyzes monthly data on the population of private-sector employees, matched with the information on contractual pay levels set by industry-wide agreements, which were bargained by the representatives of trade unions and employers at the national level. The research design exploited the generalized wage growth induced by changes in the contractual pay levels, whose timing and size differ across collective agreements. The specification adopted compared the outcomes of interest within sectors and geographical locations, and among workers subject to different collective contracts. The study results show that contractual wage growth raised the actual pay levels and had significant negative effects on employment. These employment effects were broadly consistent with the Hicks–Marshall laws and with several hypotheses of traditional centralized wage bargaining models.

1. Introduction

Wage-setting institutions are often considered important for explaining the differences in economic performance among countries (Nickell, 1997). Indeed, the provisions characterizing collective or decentralized wage bargaining can potentially influence several economic variables.[1] Despite this interest, abundant micro-based evidence on the effects of wage-setting institutions is available only for a few policies (mostly minimum wages). Other forms of pay determination, such as collective

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[1] Important outcomes have been linked to the wage-setting structure using either theoretical arguments or empirical evidence, most notably: economic growth (Dustmann et al., 2014); employment (Kahn, 2000, Bertola et al., 2007, Murtin et al., 2014); wage distributions and inequality (Blau and Kahn, 1996, Koeniger et al., 2007, Card et al., 2013); wage rigidities (Agell and Lundborg, 2003, Messina et al., 2010); firms' average productivity (Moene and Wallerstein, 1997, Hibbs and Locking, 2000, Haucap and Wey, 2004); investments in training (Acemoglu and Pischke, 1999); technology adoption choices (Davis and Henrekson, 2005, Acemoglu, 2010, Alesina et al., 2018); monetary policy effects (Faia and Pezone, 2023); international trade effects (Helpman and Itskhoki, 2010); and product market competition effects (Griffith et al., 2007).

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bargaining, have been most often analyzed only through cross-country comparisons or highly aggregated data, particularly when the outcome of interest was employment.

This tendency is problematic, given that there are relevant differences between the government–legislated wage floors, which are typically lower, and those that are set by collective bargaining (Boeri, 2012). However, this gap in the literature is also not surprising. Indeed, pay determination, when not completely decentralized at the firm level, typically works through complex implementation mechanisms that may differ across and even within industries (Flanagan, 1999, OECD, 2017 and Bhuller et al., 2022). Therefore, the empirical evaluation of policies adopted through complex collective-bargaining systems often represents a challenging task.

In this study, we examined the employment effects of wage growth induced by the Italian sectoral wage bargaining system. This is an interesting institutional setting, where contractual pay schedules are bargained by trade unions and employers' associations at the level of the national sector. In the context of a standard minimum wage, selected workers earning more than this pay floor sometimes have wages that are linked to its level. The Italian institutional setting can be described as a system in which a similar indexation to various minimum wages exists for the entire private-sector workforce. Thus, we could analyze the relationship between wage growth and employment using policy shocks affecting virtually the entire pay distribution.

Our analysis was based on monthly data covering the population of private-sector workers' social security contribution records.[2] We matched these records with precise information on contractual wage levels bargained by trade unions and employers' associations in nearly 160 national sector-wide agreements periodically renewed between 2006 and 2016.[3] This dataset represents the most detailed available source of information on the population of interest. Using the variations in contractual wages, we have estimated the own-price labor demand elasticity for the entire economy and its heterogeneity across several dimensions.

The characteristics of the Italian institutional setting have allowed us to build a solid and innovative research design for several reasons. First, collective-bargaining provisions regarding wages apply to all private-sector employees, irrespective of their union membership. Therefore, we have avoided complications related to self-selection of firms into more or less centralized bargaining levels, which characterize systems, such as that of Germany, where firm-level exemption clauses are allowed (Baumann and Brändle, 2017).

Second, in Italy several contracts usually coexist within an industry, since the activities defined and regulated by each collective agreement do not map to a standard sector classification. Moreover, the timing and the size of wage adjustments is not coordinated across collective contracts. These features have allowed us to identify treatment effects exploiting only employment and wage variation within granular sectors and geographic locations when adopting the most restrictive specifications.

Third, collectively bargained pay floors tend to be binding even for workers higher up in the wage distribution. Indeed, Italian legislation considers these wage floors to be also a *fixed pay component*. That is, an increase in contractual wages typically shifts up by the same amount the wages of all workers involved, including those who already earn more than the new minimum.

The empirical analysis was based on data covering a period of 11 years from January 2006 to December 2016. Employment and actual average pay levels, which represent the two main outcomes of interest, were computed in each month within groups of observations. These groups were defined by the interaction between collective contracts and either firms, or detailed geographical areas and economic activities. The estimation strategy was based on a generalized difference-in-differences regression approach. Through this model, we measured how much the variation in contractual pay levels across time and collective agreements affected wages and employment levels.

The fixed effects approach that we adopted identifies the parameters of interest exploiting only employment (wage) variation within groups, with respect to each group's average employment level across periods. Time fixed effects further restrict the identifying variation to account for employment fluctuations that are common across groups within a given period. In the most saturated specification, time fixed effects were interacted by an ISIC 38-sectors classification (1.5-digit sector) and a 107-provinces classification to control for a rich set of nonparametric effects accounting for business-cycle fluctuations.

The results reveal that the growth of contractual wages had positive effects on actual average pay levels. The salient role of Italian collective bargaining in shaping wage dynamics is consistent with existing evidence for other countries with similar systems of industrial relations (Cardoso and Portugal, 2005, Dahl et al., 2013, Card and Cardoso, 2022, Bhuller et al., 2022). The results also demonstrate substantial negative employment effects. Ignoring general equilibrium considerations, our estimates show that the Italian private-sector workforce was reduced by approximately 0.8% per year, relative to its true potential, owing to the statutory growth in compensations set by collective bargaining. This evidence contributes to the relatively less developed literature that aims to provide nation-specific micro-based evidence on the employment effects of collective wage bargaining.[4]

As mentioned, wage shocks induced by Italian collective bargaining typically affect not only marginal workers at the bottom of the earnings distribution, but also virtually the entire workforce within a collective contract. Given this context, our results are consistent with several findings documenting larger disemployment effects associated with minimum wage increases that bite more deeply into the wage distribution (Clemens and Strain, 2019 and Gregory and Zierahn, 2022).

Italy's macroeconomic performance during the study period was characterized by low or negative economic growth, along with low or negative inflation rates. Therefore, our results are consistent with evidence suggesting that statutory wage growth should lead to more negative employment effects during an economic downturn, and to lower employment losses during expansionary periods (Jardim et al., 2022, Clemens and Wither, 2019). Moreover, our results suggest that

[2] The social security contribution data are property of the Italian National Institute for Social Security (INPS) and are accessible at the INPS premises through the VisitInps program. The data on collective agreements was collected for this project using disaggregated information on each contract's pay levels and the dates of their validity over an 11-year period. To access the data for replication purposes researchers should contact INPS's central research unit (https://dcstudiricerche@inps.it).

[3] Italian collective bargaining is characterized by an intermediate degree of centralization. The average size of collective agreements tends to be quite large, as the 150 largest sectoral contracts cover almost 15 million workers, representing more than 90% of all private-sector employees. See Calmfors and Driffill (1988) for a characterization of bargaining systems according to their degree of centralization.

[4] This literature includes (Card, 1990), who found negative employment effects related to contract wage shocks in the Canadian covered sector; (Dolado et al., 1997), who attributed large employment losses to collective bargaining using discontinuities in wages around the minima among Spanish workers; Magruder (2012), Martins (2021), and Hijzen and Martins (2020) who documented, for South Africa and Portugal, negative employment effects associated with the coverage extension of collective agreements; (Brändle and Goerke, 2018), who found negative, but rather small employment effects among German firms applying a collective or firm-level agreement; and Guimaraes et al. (2017), who found strong disemployment effects associated with the wage bill growth induced by collective bargaining in Portugal. More recently, Card and Cardoso (2022) document a nonstatistically significant association between contractual wage growth and employment changes among Portuguese firms covered by collective agreements.

statutory wage growth produces stronger employment effects when it is not rapidly eroded by inflation, as highlighted by Sorkin (2015).

We tested several theoretical hypotheses on the shape of employment adjustments to increased labor costs. First, we argue that the standard theories of labor demand are appropriate for analyzing wage-setting in the context of this study. In this respect, while contractual wage levels are often adjusted, in Italy, other rules set by collective bargaining are typically stable across time and often not compulsory for individual firms. Thus, this system is best characterized as a bargaining model where employment is set on the labor demand, rather than by theories where unions can implement efficient contracts (MaCurdy and Pencavel, 1986).

We show that price adjustment mechanisms within a standard Hicks–Marshall model can rationalize the large size of the estimated employment effects, given that contractual wage shocks were not symmetric across firms. Indeed, when only selected firms are hit by a shock, output levels are generally more sensitive to potential price increases for employers affected by wage growth. Consistent with the Hicks–Marshall predictions on the relative size of the labor demand elasticity (Hamermesh, 1993), we also show that employment responses to wage growth were stronger among firms with a higher share of contract-specific labor costs in total revenues.

The vintage model of firm creation with collective bargaining formalized by Moene and Wallerstein (1997) suggests that the negative employment effects of having a centralized trade union that bargains over wages should be concentrated mostly among the least-efficient employers. Indeed, the best performing companies could potentially benefit from pay moderation according to this theory.[5] Consistent with this hypothesis, we found that employment at companies with the lowest levels of value added per worker, compared with the average within the contract, was more responsive to statutory compensation growth. Moreover, employment effects were not significant among the relatively most efficient firms.

Importantly, we also found that the contractual wage growth led to lower job creation, rather than higher separation rates. This result is consistent with the *membership* theory (Blanchard and Summers, 1986), according to which wage-setting in unionized markets tends to be more favorable for incumbent workers. It is also consistent with recent evidence on the extensive-margin employment adjustments and on hiring practices provided by the minimum wage literature (Clemens et al., 2021, Gopalan et al., 2021 and Jardim et al., 2022).

Finally, we found that the timing of the employment effects was consistent with theoretical predictions from models with rational expectations about future wage hikes. Predictability is a characteristic of Italian contractual wage shocks, which are typically bargained and announced before their actual implementation. The distinction between anticipatory and delayed effects (and between rational expectations and uncertainty) is relevant in labor markets characterized by frictions.[6]

Sorkin (2015) showed that when frictions are determined by capital, as its level cannot be adjusted in the short run, unexpected minimum wage hikes can have ambiguous effects on employment owing to potential mistakes in investment decisions. Thus, according to this theory, rational expectations are a preferable context for studying the underlying structural relationship between the variables of interest. Moreover, labor market frictions driven by job search costs should give rise to employment adjustments carried out over a longer period of time and should start before the actual wage hike in the presence of rational expectations (Pinoli, 2010). Consistent with this hypothesis, employment adjustments to contractual wage growth were already significant some months before its occurrence. However, we did not find significant anticipatory effects in periods further away from when contractual wage growth occurred.

The study is structured as follows. Section 2 presents a general description of the institutional characteristics of Italian collective bargaining. Section 3 presents the data and the first evidence on the relationship between contractual wage growth and employment. Section 4 discusses the main empirical approach. Section 5 presents the main results. Section 6 discusses the relationship between the shape of employment adjustments and several theories of wage-setting in unionized labor markets. Section 7 discusses the dynamics in the employment effects of contractual wage growth. Section 8 provides the concluding remarks.

2. Institutional context

Italy has numerous national sector-wide collective contracts negotiated by trade unions and employers' associations, which are typically renewed every two years on dates that are not coordinated across different agreements.[7] The activities regulated by collective agreements are defined by bargaining parties and laid down in each contract. Generally, employers must apply the contract that is most representative given the activities performed by each employee.

A peculiarity of Italian collective bargaining is that several collective agreements typically coexist within a given industry, for several reasons. Different collective contracts are often applied within a sector depending on the size of the firm: for example, depending on the size of the enterprise, three collective contracts exist for metal-manufacturing firms (and for most manufacturing firms in general). Similarly, the application of a collective contract to a worker depends on the tasks that he/she performs within a business.[8] Moreover, in many sectors, the wages of some types of workers, such as managers, are negotiated through separate nation-wide collective agreements.[9] Finally, the presence of multiple collective contracts within a sector can also be in part the result of classification inconsistencies. Indeed, the industry classification of collective contracts does not precisely follow official industry definitions, as it rather depends on the historical organizational structure of trade unions, employers' associations, and firms.[10]

One of the main purposes of collective bargaining is to set minimum pay levels (contractual wages) in the private-sector at the national, industry-wide level. Such pay levels are negotiated by different bargaining parties (trade unions and employer associations' representatives) for each collective contract. Thus, the dynamics of such pay levels typically differ for each contract, even if there is some degree of informal coordination.

[5] This argument has often been used to rationalize the Scandinavian model, that is, a system characterized by compressed wage dispersion and high productivity (Edin and Topel, 1997, Agell, 1999, Hibbs and Locking, 2000 and Barth et al., 2014). More evidence on this hypothesis for the case of Italy has been provided by Devicienti and Fanfani (2021).

[6] In frictionless labor markets, employers can instantly adjust to the optimal employment levels for a given wage schedule. Thus, no differences should be observed between the effects of announced and unexpected wage hikes.

[7] The 2017 classification of the National Social Security Institute includes approximately 300 collective agreements. However, there are also several other contracts (typically those with an extremely small coverage) that are not included in this classification. The proportion of workers covered by a contract excluded in the official classification was always below 2% during the study period.

[8] For example, larger metal-manufacturing firms may employ workers in charge of sales, recruiting, and human resource management under the trade collective contract.

[9] There are even some extreme cases, such as that of the water transport industry, where almost all occupations' wages are negotiated through separate collective contracts (*e.g.*, captains, cooks, cleaning personnel, on-board doctors, etc.).

[10] A second source of inconsistency may also depend on the fact that in most available databases each firm reports only one industry as its main activity, even if larger firms could potentially operate in more than one sector, employing workers under the respective collective contracts.

Wage determination follows some particular rules that are worth noting in this context. First, wage growth is often implemented through gradual increases that are set to occur at future dates. Moreover, the amount by which contractual wages grow is typically added to the pay level of all workers employed in the relevant job title, irrespective of whether they already earn above the minimum. That is, contractual wages represent both a minimum floor and a fixed component of workers' pay.[11] Finally, employees cannot be downgraded to less remunerative job titles as they can only move up in the firms' hierarchy. Therefore, the amount of wage rigidity imposed by collective bargaining tends to be sizable.

Collective bargaining is also used to regulate several other aspects of labor contracts besides wage levels. However, negotiations on additional regulatory components of labor contracts are typically conducted only once every four years, and many of these rules are seldom changed.[12] Moreover, according to the Italian legislation, individual firms can amend or opt out of most of the rules set by collective bargaining if they do not directly involve pay floors. Instead, contractual wages are of a statutory nature for all private-sector firms and employees, regardless of their trade union membership.[13] This implies that the provisions of collective contracts that do not directly involve wage levels can be more easily side-stepped by individual companies.

Minimum contractual pay levels are enforced through two main channels. First, the National Social Security Institute routinely sends officers to firms. They are asked to check, among other infractions, whether wages adhere to the relevant collective contract. Second, employees can sue employers either directly or through the local trade union, in which case judges must verify whether wages adhere to the sector-wide minimum contractual standards. In case of a violation, employers are not only asked to cover any difference in social security contributions between what they have paid and what they should have paid according to the correct contractual wage level, but they also incur the potential loss of several fiscal benefits and incentives. Indeed, most tax exemptions typically include firms' adherence to collective bargaining standards as an eligibility rule.[14]

3. Data and first evidence

This paper is based on three main sources of information. First, the social security records of private-sector employees collected by the Italian Social Security Institute (INPS). These are monthly data and contain information about wages, days worked, and other individual characteristics. The employers are obligated to provide the details so that each employee is always matched to their respective firm. The data do not cover self-employed and public-sector employees. Importantly, employers also indicate the collective agreement to which each worker is covered by, specifying one of the nearly 300 contract codes provided by the INPS.

The second data source is a database on contractual wages stipulated by collective agreements, gathered using the pay scales listed with such contracts. In particular, for each job title within a sector-wide agreement, we recovered the relevant pay level for each month between January 2006 and December 2016 and could match 159 contracts to the INPS data, although some agreements did not have information on pay scales covering all years between 2006 and 2016. The contracts considered in the analysis tend to be the larger ones. Overall, we could match information on contractual wages for approximately 78% of all person–month observations in the INPS archives between 2006 and 2016 (approximately 1.26 billion of 1.62 billion records). The full list of contracts considered in the analyzed samples is provided by Tables C.1 and C.2 in Appendix.

Finally, for a subsample of around 200,000 incorporated companies with at least one employee registered in the INPS archives we matched the financial information on value added, revenues, and physical capital derived from the AIDA-Bureau van Dijck data. These financial variables were available for the period between 2007 and 2015. To avoid potential problems related to the representativeness of this sample and selection across years, we considered only a strongly balanced panel of these businesses in our analyses, for which a positive level of revenues and value added were observed in all years between 2007 and 2015.[15]

3.1. Evolution of contractual wages within the largest collective contracts

We illustrate two important cases to explain how minimum wages set by collective agreements work. Fig. 1 plots the evolution of contractual wages from 2006 to 2016 within the metal-manufacturing and trade collective contracts, which are by far the two largest contracts in Italy.[16] In these graphs, the lines connect the level of contractual wages at each renewal for different job titles within the same contract.

Collective agreements do not simply set a single overall pay floor but define a series of floors applied according to each worker's occupation. When these pay floors increase, in principle the wages of all workers in the relevant job title should increase by the same amount, regardless of whether wages already complied with the higher minimum.

Contractual wages were renewed at different dates in the metal-manufacturing and trade contracts. These pay floors changed more frequently in the metal-manufacturing contract (with 12 renewals during the period of observation) than in the trade contract (which had 9 renewals within the same 11-year window). The size of wage increments differs between contracts and renewal dates. However, *within each contract*, pay floors followed relatively similar dynamics across all job titles. Our main empirical specifications used variation in both the size and the timing of wage adjustments *between contracts* to identify the treatment effects of interest.

3.2. Descriptive statistics on contractual wages and treatment definition

As shown in Fig. 1, each collective agreement usually sets more than one contractual wage. Such contracts typically define a series of job titles for which specific pay levels apply. The INPS archives indicate the collective contract under which any employee is hired, but not his/her specific job title. Therefore, only collective agreements could be matched deterministically to individual employees.

[11] This general rule can be sidestepped only in the presence of a specific agreement between a worker earning more than the minimum and his/her employer. This agreement is called *superminimo assorbibile* in Italian.

[12] For example, the rules governing the relationship between workers' tasks and pay levels in the metal-manufacturing contract (which is one of the largest ones) had not been changed since the 1970s. Such rules were rewritten only in the latest contract renewal of 2021.

[13] The statutory nature of contractual wages derives from the Italian Constitution, which states that all workers must be paid *fairly*. The Italian Supreme Court has traditionally interpreted this *fair pay* to be the level that collective bargaining sets through contractual wages. However, in the Italian legal system, other regulatory elements of collective agreements do not have a similar level of protection derived directly from the Constitution.

[14] Noncompliance rates with Italian contractual wages have been investigated by Garnero (2018), Adamopoulou and Villanueva (2022) and Garnero and Lucifora (2022), although precise estimates tend to be difficult to recover.

[15] The AIDA-Bureau van Dijck data are not collected based on a random sampling procedure, as the objective of this archive is rather to cover the largest feasible number of incorporated businesses. This procedure potentially leads to problems of sample selection across years, motivating our choice of considering only a strongly balanced panel of these firms.

[16] The trade collective contract covers about 24% of the workforce in our analysis sample, which includes nearly 80% of all Italian private-sector employees. The coverage of the metal-manufacturing contract is around 13% in the same sample.

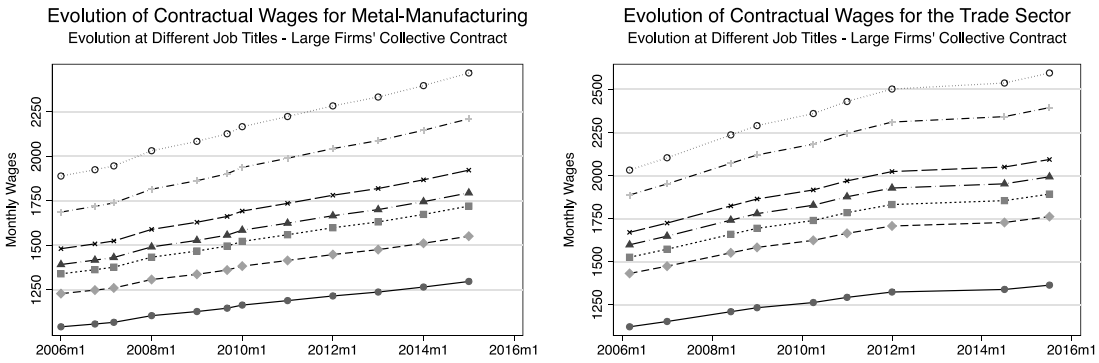

Fig. 1. Evolution of contractual wages in the Metal-Manufacturing and the trade agreements.
The left panel shows the monthly contractual wages in Euros within the metal-manufacturing collective contract. The right panel shows the contractual wages in the trade collective contract. Each line describes the evolution across time of a job title pay floor within the same collective contract. Each dot represents a new level for these pay floors. The slope of the lines is steeper for larger and more frequent contractual wage increases.

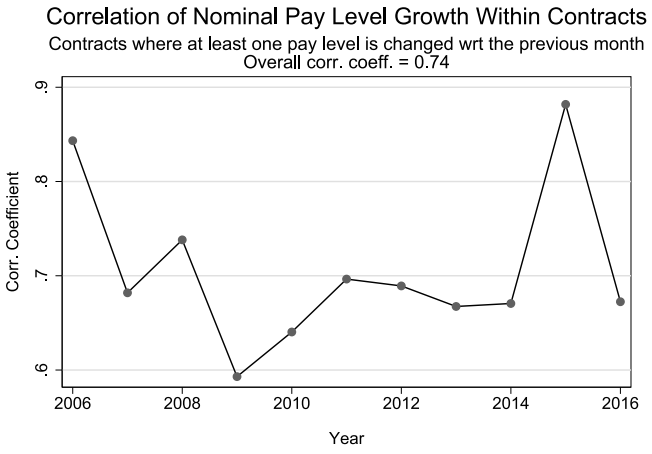

Fig. 2. Correlation of nominal contractual Wages' growth within collective agreements. The graph shows the correlation coefficient by year between the monthly percentage growth of nominal job title pay floors, and the average percentage growth of other nominal job title pay floors within the same collective contract. These correlation coefficients were computed using only months and agreements where at least one job title pay floor within the same collective contract changed. The sample included only collective contracts and dates that could be matched with the final sample of analysis (see Table C.1 for the full list).

Table 1
Descriptive statistics on the main treatment variable.

Statistic (Median Log Nominal Pay Scale)	*Level*	*St. dev.*
Average	4.041	0.144
Average log growth	0.002	0.007
Average log real growth	0.001	0.007
Average log growth, given positive growth	0.020	0.012
10th perc. of log growth, given positive growth	0.009	
25th perc. of log growth, given positive growth	0.016	
50th perc. of log growth, given positive growth	0.018	
75th perc. of log growth, given positive growth	0.025	
90th perc. of log growth, given positive growth	0.035	
99th perc. of log growth, given positive growth	0.055	
Observations (contract-location-sector-month cells)	17,384,546	
Number of collective contracts	159	
Number of contractual wage changes	1,414	
% of obs. with positive growth in contractual wages	7.26%	
Avg. n. of contracts within 38 sectors-107 provinces cells	12.39 (*11.04*)	

Statistics computed on contractual wage data matched with grouped monthly data derived from the INPS archives on private-sector workers. All means, proportions, and standard deviations are weighted by the number of workers in the group-month cell.

Given this data limitation, we have defined the median contractual wage across job titles within each collective agreement as a proxy for the actual pay floor. Thus, the changes in the median pay level within each collective agreement represent the policy treatment of interest in our analyses.[17] This choice should not represent a major source of bias, particularly if we consider how contractual wages within the same collective agreement have evolved during the study period.

Fig. 2 plots the correlation coefficient between the nominal growth rate of a given pay level, and the average growth observed for other job titles within the same collective contract and month. To avoid overestimating this parameter, such correlation was computed only in the months during which at least one of the nominal pay levels within a contract had changed. The overall correlation coefficient in pay floors' growth rates within collective contracts was 0.74. Moreover, this correlation was close to or more than 0.6 in all of the years considered in the analysis. This strong correlation is consistent with the evolution of the pay levels observed within the trade and metal-manufacturing agreements, which can be inferred from Fig. 1. Given these considerations, the growth in the median pay scale within a contract can be considered a good proxy for the evolution of other contractual wages within the same collective agreement.[18]

Table 1 provides several descriptive statistics on our main treatment variable, defined as the median log nominal pay scale of the contract in each month. These statistics are computed on the main study sample. This sample is derived from the archives of social security records aggregated by contract, sector, geographic location, and month.[19]

Table 1 shows that the monthly growth in collective agreements' median nominal pay scales was 0.2% on average, which implies a yearly growth rate of approximately 2.4%. Interestingly, contractual wages increased, on average, by 0.1% per month when also considering their price-adjusted level. That is, on average, contractual wages have been increasing faster than inflation throughout the study period.

During this time there were 1,414 contractual wage changes. The size of each nominal wage adjustment was of approximately 2% on average, with adjustments of about 3.5% at the 90th percentile. The probability of wage adjustments in each month was more than 7%, implying a frequency of nearly one contractual floor change every 14 months.

[17] For robustness, we have also tested the main results using the average contractual wage across job titles within the same collective agreement.

[18] This approximation is also likely to provide a bias toward zero in our estimates, assuming a classical errors-in-variable structure.

[19] More details on the construction of this sample are provided in the next section.

Table 2
Weighted Descriptive Statistics on the Grouped Samples.

Variables	*Entire INPS Sample*		*INPS-AIDA Sample*	
	Mean	St.dev.	Mean	St.dev.
Log FTE employment rate in the group	−2.128	1.713	−4.166	2.384
Log real wage in the group	−4.314	0.369	−4.419	0.394
Contracts' log median nominal pay scale	−4.041	0.144	−4.062	0.130
Contracts' log mean nominal pay scale	−4.073	0.144	−4.093	0.125
Contracts' log growth in median pay scale	−0.002	0.007	−0.002	0.007
Number of workers in the group	−5,717	14,670	−1,711	6,138
Workers in group/LLM workforce	−0.015	0.025	−0.008	0.040
LLM Activity Rate	−50.73	5.699	−51.65	5.067
LLM Unemployment	−8.468	4.811	−7.880	4.160
Northern Regions	58.3%		64.3%	
Tertiary Sect.	56%		52.4%	
Secondary and Construction Sect.	40.5%		47.5%	
Number of Groups	320,546		263,564	
Number of Group-Month Observations	17,384,258		19,941,103	
Number of Worker-Month Observations	1.257 Bill.		0.447 Bill.	

Statistics computed on grouped monthly data derived from the INPS archives matched to collective contracts. In the *entire INPS sample,* groups are defined by the interaction of two-digit sectors, local labor markets, and contracts. In the *INPS-AIDA sample,* groups are defined by the interaction of firms and collective contracts. All means, proportions, and standard deviations are weighted by the number of workers in the group-month cell.

3.3. Grouping of the data, definition of the outcomes, and descriptive statistics

To study the effects of contractual wages on pay levels and employment, we have constructed the outcomes of interest by dividing the INPS social security records data into mutually exclusive *groups*. Such groups were formed by combining two-digit International Standard Industrial Classification (ISIC rev. 4) sectors, 611 ISTAT local labor markets (LLM), and 159 collective contracts for which information on pay scales was available.[20] Within these groups, we have constructed our main measures of employment (number of workers and number of full-time equivalent workers) and wage levels (average daily wages) for each month from January 2006 through December 2016.

Furthermore, we replicated the analyses on the matched INPS-AIDA sample, a balanced panel of incorporated businesses covering the years 2007–2015, for which financial information was available and the value added was positive. In this case, we have grouped the data using combinations of firms and the collective contracts applied within them as the unit of analysis, thereby adopting a more granular aggregation level.

Table 2 provides descriptive statistics for the grouped INPS and INPS-AIDA data, computed by weighting observations by the number of workers in each group. The first two rows summarize the main outcomes considered in this empirical analysis. The full-time equivalent (FTE) employment rate of the group was defined as the total number of days worked in a month divided by 26 (the standard duration of monthly full-time contracts in the Italian labor market) over the yearly number of active individuals in the local labor market. The third, fourth, and fifth rows summarize the policy treatment variables expressed in nominal terms.

In the INPS–AIDA sample (as reported in Row 6, weighted average workers in each group), the groups were consistently smaller than in the entire INPS sample because in this case, the data were grouped using finer firm – contract cells, rather than sector – LLM contract interactions. Generally, the INPS–AIDA sample overrepresents firms located in northern regions of Italy, where unemployment rates are lower and activity rates higher. In both samples the industry composition was highly influenced by the exclusion of self-employed and public employees, both of which tend to be concentrated in service sectors. Moreover, in the INPS-AIDA sample, the industry composition was further influenced by the unavailability of income statement data for financial institutions.

3.4. Employment evolution in contracts affected by a large growth in bargained wages

Before illustrating the main identification strategy of this study, we provide preliminary evidence on the relationship between contractual wage growth and employment relying on a case study. In particular, we have selected 155 contractual wage growth events greater than 3%.[21] For each of these events, we measured employment growth during a 14-month window that included 7 months before and after this shock. We built a control sample that included all observations belonging to collective agreements that were unaffected by contractual wage growth during the same periods when wage growth events were selected. To limit the influence of contractual wage changes occurring shortly before or after the periods covered in the data, we have also excluded treated and control groups for which a pay floor change occurred within 3 months before or after the window of observation.

The sample selection choices described above allow the building of a stacked dataset of contractual wage growth events. A more comprehensive regression analysis that exploits a similar setup is provided in Section 7, which presents results derived from a stacked event study estimator (Cengiz et al., 2019) for different choices of the observation window around the events.

Preliminary evidence on the employment effects of contractual wage growth is provided in Fig. 3. The right panel shows the unconditional evolution of employment among contract-sector-location groups affected by a growth in contractual wages at time zero larger than 3%. The second series shows the employment evolution among groups that were always unaffected by wage growth during the same periods considered for the sample of treated groups. The left panel of Fig. 3 shows descriptive statistics on the evolution of contractual wages among treated groups.

[20] ISTAT local labor markets are defined by the Italian National Institute of Statistics using census data on commuting behavior and applying an algorithm that maximizes the number of local jobs held by residents and the number of residents working within small geographical areas. The two-digit ISIC classification is formed by around 80 industries defined based on their product characteristics.

[21] The total number of contractual wage growth events observed in the sample of analysis amounted to 1,414.

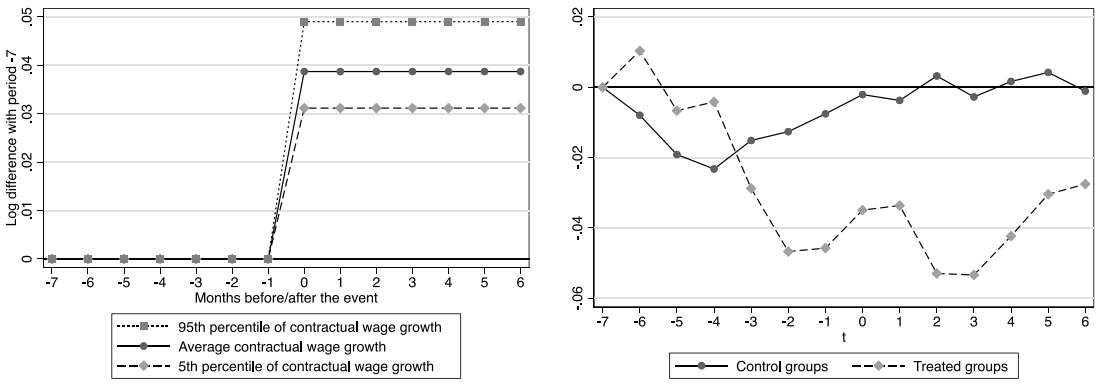

Fig. 3. Unconditional employment evolution for groups affected by large contractual wage shocks.
The left panel shows the log difference (with respect to period $t = -7$) in the median nominal pay scale of the collective contract among groups affected by contractual wage growth at time $t = 0$. The average, 5th percentile, and 95th percentile of this difference are shown. There were 13,169 treated groups affected by 155 contractual wage changes in the sample, for a total of 184,366 treated group-month observations.
The right panel shows the log difference in the employment level for each period before and after the contractual wage increase, separately among groups affected by wage growth and unaffected groups. The period $t = -7$ was used as the reference period. The sample size was 1,373,960 group-month observations, of which 1,189,594 (corresponding to 84,971 groups) were always unaffected by contractual wage growth.

As can be seen, employment fluctuates around a relatively negative trend among both treated and control groups up to period -4. From period −3 onward, the employment evolution among groups that were not affected by wage growth becomes flat or slightly positive. In contrast, among groups affected by wage growth the evolution remains negative. These processes generate a substantial difference in the employment evolution of the two groups, which reaches its peak around period 2. Even 6 months after the contractual wage growth event, employment growth is on average nearly 2.6 percentage points lower in the treated groups than in the control groups, which would imply an employment elasticity with respect to contractual wage growth of around 0.68.

All things considered, Fig. 3 provides descriptive evidence that is consistent with potential employment losses associated with contractual wage growth. However, this result has two main shortcomings. First, it is based on a limited sample of contractual wage growth events. Second, there could be trends that were correlated with contractual wage growth and were not accounted for when considering employment's unconditional evolution. The following section presents the main identification strategy of this study, which relies on the full sample and on a rich set of controls to account for the potential endogeneity in employment trends. Section 7 provides regression-based evidence on the dynamics of employment effects relying on a stacked event study sample similar to the one analyzed in this section.

4. Identification strategy

4.1. Empirical model specification

Our main identification strategy is based on the estimation of a generalized difference-in-differences model with continuous treatment. We have specified this model as follows. Let t represent the index time periods (months), c represent the index industry-wide collective contracts, m the index LLM, l the index less granular geographical units, and s the index sectors. Furthermore, we denote groups defined by the interaction of collective agreements, LLM, and two-digit sectors with g. When the model is estimated on the sample of incorporated businesses, groups g are instead defined by the interaction of firms with collective agreements. Using this notation, the regression equation of interest can be written as

$$y_{gt} = \beta \mathrm{PS}_{ct} + \gamma x_{mt} + \alpha_g + \phi_{slt} + \epsilon_{gt} \tag{1}$$

where PS_{ct} is the median log pay scale of collective contract c at time t; x_{mt} is a set of time-varying local labor market characteristics (activity and unemployment rates), which control for shifts in the labor supply and the business cycle; α_g is a group fixed effect; ϕ_{slt} is a sector- and region-specific time fixed effect; and ϵ_{gt} is a residual term. In this model, the contractual wages' nominal level is the relevant policy, as the effect of variations in their real level is fully absorbed by the monthly time fixed effects.

We have considered two main outcomes. First, we have defined y_{gt} as the log average wage in month t within group g. In this case, β gives the elasticity of the actual pay levels to the contractual wages set by collective bargaining. Second, we have defined y_{gt} as the log FTE number of workers in group g and month t divided by the workforce of the local labor market m in the respective year.[22] With this specification, β gives the percentage growth in the employment rate for a 1% growth in contractual wages.

As a robustness test, we have also defined employment (y_{gt}) as the number of workers in group g divided by the workforce of the local labor market. In this case, only employment adjustments on the extensive margin can influence the outcome; however, this dependent variable is less vulnerable to potential misreporting of actual days worked. In another related specification, we have separately considered the hiring and separation rates as outcomes, in order to analyze policy effects separately for incumbent workers and outsiders.

Finding a negative effect associated with contractual wage growth does not imply that employment dynamics are generally negative after this shock. Rather, a similar result implies that employment growth is less positive (or more negative) with respect to the trend observed in a counterfactual control group that was unaffected by the wage shock. That is, the size of the estimated employment elasticity to wage growth depends on a comparison with a counterfactual employment trend. This point is related to a well-known limitation of specifications exploiting cross-sectional variation in the treatment. These models cannot identify whether the aggregate levels of the outcome actually grow or fall after the treatment, unless all general equilibrium effects of the policy affecting both control and treatment groups are taken into account (see *e.g.*, Wolf (2023)).

To recover a measure of the reduced-form labor demand elasticity to wages, as well as a confidence interval for this parameter, we have also directly estimated the following employment equation

$$emp_{gt} = \eta w_{gt} + \gamma x_{mt} + \alpha_g + \phi_{slt} + \epsilon_{gt} \tag{2}$$

[22] Dividing employment measures by the size of the workforce allows us to better control for shifts in the labor supply. Specifications with unadjusted employment levels as an outcome provided similar results.

where emp_{gt} is the (formal) employment rate measured in FTE equivalent units, w_{gt} is the average log wage in group g and month t, while all other elements have the same interpretation as in Eq. (1). We have estimated the model of Eq. (2) by two-stage least squares (2SLS), using median contractual log pay scales (PS_{ct}) as an instrument for w_{gt}.

The labor demand elasticity (η) is a function of the parameters given by Eq. (1), *i.e.*, it is the ratio of $\beta(y_{gt} = emp_{gt})$ to $\beta(y_{gt} = w_{gt})$. However, the interpretation of this parameter as a labor demand elasticity comes with nontrivial caveats. First, the estimated wage effect depends on employment composition as well, which may change across time. Second, reductions in costly fringe benefits (which are typically unobserved) could, in part, hide the actual wage adjustments (see *e.g.*, Clemens et al., 2018).

For all regression models, we have addressed heteroskedasticity by clustering the standard errors at the group level and by weighting all of the regressions by the number of workers constituting each group g. This latter adjustment also has the advantage of providing parameter estimates closer to the population average. Thus, the clustering choice allows us to correct for any correlation pattern of the outcome within groups across time. Given the large number of available groups, this choice is appropriate in the present context (Bertrand et al., 2004).

4.2. Contractual wages and the exclusion restriction

Unobserved factors correlated with changes in collective bargaining pay scales, and also influencing the outcomes of interest, represent the main threat to a correct identification of the parameters of our model. For example, bargaining parties could consider business cycle fluctuations when setting pay scales, as they may possess information on future labor demand (Card, 1990).[23] In this respect, using data that cover our study period, Fanfani et al. (2023) found a strong correlation between contractual wages and the consumer price index, but no significant correlations with several measures of Italian firms' performance.[24] However, the presence of correlations between contractual wage dynamics and the business cycle cannot be excluded *ex-ante*.

To address this concern, we relied on the granularity of the available data and on institutional features that have allowed us to construct a solid research design. In particular, given that Italian collective bargaining is characterized by an intermediate degree of centralization, more than one contract is commonly applied within a sector, while, conversely, some large contracts cover heterogeneous activities that can take place in more than one industry. Table 1 shows that on average 12 collective contracts were observed within a 38 sectors — 107 provinces cell. Therefore, we could include nonparametric controls for aggregate trends in the outcomes at the level of the local industry. This approach would not be possible when studying more centralized wage policies, which typically have a much more limited variability within regions and sectors.

In our context, the policy effect was identified by comparing outcomes among groups whose contractual wages had changed with respect to those within the same geographical area and sector that were not subject to a similar shock. In particular, we controlled for the following confounders: constant effects for each two-digit sector, local labor market, and collective agreement interaction (firm and collective agreement interaction in the sample of incorporated businesses); monthly time fixed effects interacted with geographical areas (20 regions or 107 provinces) and industries (ISIC 21 or ISIC 38 classifications); specific time-varying regressors for nearly 600 LLM, controlling for business cycle fluctuations and labor supply effects (yearly activity and unemployment rates).[25]

Given the specification adopted, concerns related to the presence of endogenous unobservable trends in wages or employment across space are not particularly relevant. Those related to the correlation between contractual wages and business cycle fluctuations are addressed by conditioning on a very rich monthly set of industry space-specific unobservable effects.

4.3. Other identification concerns

Other potential concerns related to the empirical model adopted have to be addressed. First, treated and control firms may not be stable if firms select into collective contracts depending on wage levels. In this respect, Italian employers typically cannot avoid compliance with the pay legislation, nor can they choose to apply the most convenient contract in a given period.[26] There are also strict regulations prohibiting the downgrading of existing employees toward less remunerative job titles or contracts.

These features also emerge from the data when analyzing changes across time in the application of collective agreements by firms in our estimation sample. The percentage of workers continuously employed for 2 years in the same firm who switched contract was approximately or less than 3% in all of the sample years. The percentage of companies applying a new type of contract was always less than 5%.[27] Moreover, neither percentage seemed systematically higher during or after the years when previously applied contractual wages had increased.

Potential labor supply shifts toward firms operating under contracts that did not change their pay levels whenever a given agreement increased its wages would also be a cause for concern. While this possibility cannot be ruled out, its relevance should not be overstated. Year-to-year transitions of workers across contracts (considering both stayers and movers across employers) show that this probability was always around 5%, irrespective of the changes in pay levels in the collective agreement of origin. All workers in our data were bound by a collective contract with downward rigid wages; a feature that, in principle, should limit the extent of the potential employment effects of positive supply shocks. In this regard, the inclusion in the regression equation of a measure of labor market tightness at the local level (*i.e.*, the local unemployment rate) appeared to have no detectable influence on our main results.

We also emphasize that the employment measures considered in this study depend on firms' reliance on formal employment relationships, given the administrative nature of our data. Therefore, we could not cover workers hired off the books nor civil servants and the self-employed. In principle, firms may react to policy changes by outsourcing some of their activities to either of these groups, but this possibility is often unlawful. Moreover, this process would still have negative externalities, given that higher reliance on nonstandard work arrangements typically entails lower compensation, fewer social security contributions, and reduced employment protection levels (Goldschmidt and Schmieder, 2017).

Finally, the presence of treatment effect heterogeneity could be problematic given the specification adopted, which is characterized by variation in treatment timing. The recent methodological literature has mainly focused on the case of a binary treatment, showing that in this

[23] The related problem of correlations between contractual wage growth and other rules set by collective bargaining is discussed in Section 6.1.

[24] Matano et al. (2023) show that import competition shocks led some sectors to negotiate relatively lower contractual wages in Italy between the late 1990s and early 2000s. Evidence from other countries includes Avouyi-Dovi et al. (2013) and Christofides and Oswald (1992), who find that negotiated industry-level wage agreements are negatively correlated with the unemployment rate in France and Canada, respectively.

[25] Given that saturated specifications reduce the amount of variation used to identify the parameter of interest, we also tested the main results in a specification that accounts only for group- and time fixed effects.

[26] See Lucifora and Vigani (2021) for more specific analyses on similar tendencies in the Italian labor market.

[27] Both percentages were computed considering switches to any type of collective agreement, not only those matched to the contractual wage data.

context the ATT cannot be generally recovered through standard OLS approaches with time and unit fixed effects (Goodman-Bacon, 2021).[28] Most of the methodological innovations and diagnostic tests proposed in this recent literature are restricted to the case of event studies and binary treatments. Nevertheless, we have conducted several robustness tests that were feasible in our context.

In this regard, results of our main regression model were consistent across several specifications, such as when using unweighted regressions, when considering substantially less saturated regressions, or when using leads of the independent variable of interest. Since each of these models potentially alters the weights aggregating each treatment effect, consistency across these results did not support the hypothesis of a strong bias in the estimator related to heterogeneity problems. Importantly, Section 7 discusses evidence from a stacked event study model estimated on a subset of contractual wage changes that could be analyzed using this approach. This method is generally considered robust for weighting problems (Baker et al., 2022). Results from these dynamic models were broadly consistent with our main findings and the theoretical predictions on the shape of employment effects across time.

5. Effects of contractual wages on pay levels and employment

5.1. Main estimates of the effect on wages and employment

In this section, we present evidence on the wage and employment effects of collective bargaining, as obtained by estimating Eq. (1). We consider the results derived from both the entire social security records archives (*entire INPS sample*) and the balanced panel of incorporated businesses matched to financial information (*INPS-AIDA sample*). Table 3 summarizes the results obtained using the former sample, while Table 4 provides the corresponding evidence for the latter database.

In each table, columns on the left part refer to the model in which the outcome was the average log wage of the aggregation group. Columns on the right panel refer to the case in which employment (number of FTE workers in the group divided by the local labor market workforce) was the dependent variable. In all tables, the number of observations was computed omitting singletons, that is, clusters of fixed effects where only one observation is available, which were also dropped from all computations.[29]

Results show that contractual pay levels set by collective bargaining strongly impact wages. The elasticity of within-group average wages to the median statutory compensations set by collective agreements, depending on the models' specification and on the choice of the sample, was approximately 0.5 and always highly significant,[30] which is relatively stronger than the magnitude of similar elasticities estimated in studies on the minimum wage.[31]

The considerably strong influence exerted by wage-setting institutions on Italian pay levels may be rationalized through several mechanisms. First, statutory compensations are occupation-specific; thus, they are typically relevant for all types of workers. Second, as contractual wages are typically interpreted as a fixed pay component to be added to every employee's salary, their growth also tends to affect wages that are already higher than the contractual minimum levels. Measurement issues could also potentially be relevant. In particular, average wages are influenced by composition, and selection mechanisms across time could potentially influence the estimated parameter.

When looking at the employment effects of collective bargaining, results demonstrate a negative elasticity of FTE employment to contractual wages. The point estimate was approximately or less than −0.35 in the entire INPS sample, whereas it was even stronger (nearly −0.5) in the panel of incorporated businesses. The effect of the inclusion of time-varying controls at the local labor market level (activity and unemployment rates) on these coefficients was negligible. Moreover, these coefficients remained quite stable when choosing more saturated definitions of the fixed effects.

Contractual wage growth was at an average level of around 2.4% per year during the study period within the full INPS sample (Table 1). Considering this, an employment elasticity to contractual wage growth close to −0.35 would imply that the Italian private-sector workforce was reduced by approximately 0.8% per year, relative to its true potential, owing to the statutory growth in compensations set by collective bargaining. However, this conclusion holds only if we ignore any general equilibrium effect potentially affecting both the treated and control groups within our sample.[32]

Table B.1 (in the Appendix) further shows that our main results on employment effects were also robust for several alternative specifications. In particular, we found consistent results when using an unweighted regression, when including only unit- and time fixed effects with no interactions and time-varying controls, and when the outcome was defined without dividing employment by a time-varying measure of local workforce size. The stability across these specifications suggests that our main results were unlikely to be driven by bias in the estimator.[33]

Appendix A summarizes the heterogeneities in the policy effect found across several dimensions, namely, economic activities, population groups, and business cycle fluctuations. Generally, these results show that although the wage effects of collective bargaining were sizable and significant across all sectors and population groups, negative employment effects were not relevant among older workers and those under open-ended contracts, which are characterized by high levels of employment protection legislation. Fixed-term contracts and young workers were the most negatively affected in terms of employment.

The employment effects of higher contractual wages were not significant in some large tertiary industries, namely, the trade, transport,

[28] Wooldridge (2005) discusses a continuous treatment case, providing the conditions under which OLS with unit and time fixed effects identifies the ATT. However, this analysis is restricted to unit-specific treatment effect heterogeneity. The consequences of more general forms of heterogeneity for the case of a difference in differences with continuous treatment are discussed in Callaway et al. (2021). This is a quite recent study, and improved methodological approaches for this case have not yet been established.

[29] The omission of singleton groups reduces the risk of underestimating the standard errors, and it is a procedure available by default when using the program *reghdfe* in STATA.

[30] Notice that the median pay level of the contract is only highly correlated with the actual growth in effective contractual wages, thus the estimated coefficients, assuming a classical errors-in-variables setting, were probably biased toward zero due to measurement error.

[31] For example, Neumark et al. (2004), examining the minimum wage effects across the US wage distribution, found elasticities approximately or above 0.5 only for a relatively small fraction of workers with earnings that were close to the pay floor.

[32] This is a classical *missing intercept problem* (Wolf, 2023). General equilibrium effects could in principle reduce the estimated aggregate employment losses, if, *e.g.*, surviving firms are positively selected, or if the wage shock reduces competition across firms in the labor or credit market. Instead, other general equilibrium effects could reinforce aggregate employment losses if, *e.g.*, prices for intermediate goods are increased, or if other negative feedback effects are generated within value chains by the wage shock. The shape of these general equilibrium effects may also depend on the level of efficiency and coordination of bargaining parties across collective contracts (see Barth et al., 2023 for a model along this dimension).

[33] In Table B.2, we show that the results of the employment effects of collective bargaining also held when using an alternative definition of the treatment and outcome variables. We found similar elasticities when using the average (instead of median) contractual wage of the collective agreement. Moreover, employment effects of contractual wage growth were strong and negative even when the outcome was defined using the number of workers employed within each group, instead of its FTE level.

Table 3
Effect of pay scales on wages and employment — Entire INPS sample.

Dependent variable:	*Group's Avg. Log Wages*				*Group's Log FTE Empl. Rate*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Coefficients								
PS_{ct}	**0.450****	**0.450****	**0.435****	**0.430****	**−0.361****	**−0.363****	**−0.346****	**−0.357****
S.e.	*0.019*	*0.019*	*0.020*	*0.020*	*0.083*	*0.083*	*0.082*	*0.077*
Activity rate		0.001**	0.001**	0.000		−0.016**	−0.016**	−0.014**
S.e.		*0.000*	*0.000*	*0.000*		*0.001*	*0.001*	*0.001*
Unemployment		−0.001*	−0.001*	−0.000		−0.003	−0.003*	−0.006**
S.e.		*0.000*	*0.000*	*0.000*		*0.001*	*0.001*	*0.002*
Fixed Effects								
Group	✓	✓	✓	✓	✓	✓	✓	✓
Time∗ISIC 22∗region	✓	✓			✓	✓		
Time∗ISIC 38∗region			✓				✓	
Time∗ISIC 38∗province				✓				✓
Adjusted R^2	0.895	0.895	0.901	0.908	0.976	0.976	0.977	0.979
RMSE	0.119	0.119	0.116	0.112	0.264	0.263	0.258	0.251
N. of observations	17.363M.	17.363M.	17.363M.	17.347M.	17.366M.	17.366M.	17.365M.	17.350M.

**: 1%; *: 5% significance levels. Groups are defined by the interaction of collective contracts, local labor markets, and two-digit sectors. All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available).

Table 4
Effect of pay scales on wages and employment - INPS-AIDA sample.

Dependent variable:	*Group's Avg. Log Wages*				*Group's Log FTE Empl. Rate*			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Coefficients								
PS_{ct}	**0.523****	**0.523****	**0.507****	**0.489****	**−0.595****	**−0.587****	**−0.470****	**−0.490****
S.e.	*0.030*	*0.030*	*0.032*	*0.034*	*0.148*	*0.148*	*0.157*	*0.160*
Activity rate		−0.000	0.000	−0.000		−0.015**	−0.015**	−0.012*
S.e.		*0.000*	*0.000*	*0.000*		*0.001*	*0.001*	*0.002*
Unemployment		−0.000	−0.000	−0.000		−0.015**	−0.017*	−0.011**
S.e.		*0.000*	*0.000*	*0.001*		*0.003*	*0.003*	*0.005*
Fixed Effects								
Group	✓	✓	✓	✓	✓	✓	✓	✓
Time∗ISIC 22∗region	✓	✓			✓	✓		
Time∗ISIC 38∗region			✓				✓	
Time∗ISIC 38∗province				✓				✓
Adjusted R^2	0.826	0.826	0.833	0.844	0.985	0.985	0.985	0.987
RMSE	0.164	0.164	0.161	0.156	0.294	0.293	0.290	0.263
N. of observations	19.935M.	19.935M.	19.934M.	19.909M.	19.936M.	19.936M.	19.935M.	19.910M.

**: 1%; *: 5% significance levels. Groups are defined by the interaction of firms with the collective agreements that they apply. All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available).

and tourism industries. Moreover, not every association was consistent with a simple categorization of activities according to their degree of tradeability, given that, for example, significant disemployment effects were found in the construction sector, which tends to be insulated from international competition. Section 6.3 discusses some interpretative issues related to price dynamics and employment effects within collective bargaining systems.

5.2. Implied own-price labor demand elasticity and relationships with other studies

Table B.3 reports the labor demand elasticity to wages implied by our results, estimated using the 2SLS method. This parameter is given by the ratio of the elasticities of employment and wages to contractual pay levels, and its confidence interval was recovered by estimating these two equations simultaneously. The value of this elasticity was estimated to be nearly −0.8 when using the entire INPS sample, while it exceeded −1 in the baseline specification when using the sample of incorporated businesses.

Interpreting the size of the own-price labor demand elasticity can be difficult. Its magnitude may depend on, among other factors, how the wage effect is estimated, the frequency of the data, whether wage shocks are rapidly eroded by inflation, and the variation used in its estimation. Overall, our results suggest that the employment effects of the pay floors set through centralized collective bargaining are significant and quite strong.

The size of the parameter implied by our estimates can be rationalized through several underlying mechanisms. First, Italy's relatively slow economic performance throughout the years under study could be a relevant factor. In this regard, evidence from minimum wage studies suggest that negative employment effects could be larger during downturns (*e.g.*, Clemens and Wither, 2019).

A second peculiarity of collective bargaining is its comprehensive influence across the entire pay distribution. In this regard, the employment effects of wage floors are generally larger when they bite more deeply into the wage distribution (Clemens and Strain, 2019 and Gregory and Zierahn, 2022). A related point concerns the role of inflation. Sorkin (2015) showed that the employment effects of a higher wage floor should be larger when this shock is not rapidly eroded by inflation. This seems a relevant consideration in our context, given the close-to-zero inflation rates observed throughout the period of analysis. Finally the magnitude of the elasticity could also depend on the variation used for its estimation, which was based on comparisons among firms that potentially shared the same product market. This last point is more formally developed in Section 6.3.

The elasticity estimates derived from our results tend to be fairly negative when compared with evidence available from the minimum wage literature (see *e.g.*, Harasztosi and Lindner, 2019). In fact, our estimates are quite similar to the elasticity of employment with respect to the labor cost derived by Cahuc et al. (2018) for France, which was estimated in the context of a hiring subsidy.

A comparison of our results with those available for other studies on collective bargaining is less straightforward, given the limited number of applications and the underlying heterogeneity in institutional settings and estimation approaches. Card (1990) found an own-price labor demand elasticity of about −0.5, which was estimated using surprises in real wages in the nominally rigid Canadian union sector. Magruder (2012) found that collective bargaining extensions reduced employment in South Africa, with an implied demand elasticity to wages of around −0.7 in a fairly saturated model; however the effects of this policy on pay levels were not significantly different from zero in more saturated specifications.

Martins (2021), analyzing the effect of agreements' extensions in Portugal, documented negative employment effects; however, in this case the elasticity of average wages to this policy was not significantly different from zero.[34] Guimaraes et al. (2017) found a nearly −0.3 elasticity of net employment growth to the growth in labor costs attributed to collective bargaining in Portugal. In a recent contribution based on Portuguese data, Card and Cardoso (2022) did not find a significant relationship between contractual wage growth and employment dynamics.[35] Finally, Díez-Catalán and Villanueva (2015), found that Spanish workers with earnings close to pay floors negotiated before the 2008 recession had, on average, 2% higher wages, and their risk of being unemployed increased by five percentage points in subsequent years.

6. Wage-setting theories and employment effects

6.1. Conceptual framework for wage setting in unionized labor markets

This study has used the variation in wages set by Italian collective bargaining to estimate its effects on employment and actual pay levels. We now clarify potential interpretative issues related to this exercise, relying on standard theories of wage setting in unionized labor markets.

Seminal work on wage determination in unionized labor markets has been typically grounded on two alternative hypotheses (MaCurdy and Pencavel, 1986). According to the first theory, unions set wages to satisfy their objective function, whereas firms choose employment on their demand function (this is referred to as the *labor demand curve equilibrium model* in MaCurdy and Pencavel (1986)). As this outcome leads to inefficiencies,[36] a classical alternative hypothesis states that unions prefer to bargain for wage and employment combinations. This alternative theory (the so-called *contract curve equilibrium model*) can potentially lead to solutions with improved gains from trade that lie outside of the labor demand function.

If right-to-manage contracts cannot be enforced by unions (*i.e.*, if unions can set only pay levels), firms choose employment to maximize profits, given the wage level. In such a setting, a shock on wages bargained within a unionized labor market can be used to identify the firms' labor demand elasticity. Thus, under the hypotheses of the *labor demand curve equilibrium model*, the employment effects of contractual wage growth can be interpreted using the standard theoretical approaches typically used in the analysis of a minimum wage shock, such as the Hicks–Marshall model of labor demand (*e.g.*, Hamermesh, 1993).[37]

Some studies in the literature on unions have argued that the *labor demand curve equilibrium model* should be considered more realistic (*e.g.*, Oswald, 1993). Several considerations suggest that this conclusion is particularly appropriate in the Italian context. First, bargaining occurs at a quite centralized level, which makes it difficult to implement right-to-manage contracts, given the underlying firm heterogeneity and the related enforcement problems. Moreover, while pay floors are statutory for all private-sector firms and workers, other rules stipulated by collective contracts can often be amended by individual companies. Finally, contractual wages are frequently negotiated (usually every 2 years), and they are often changed even more frequently through gradual increments planned in advance. In contrast, other rules contained in collective contracts are usually negotiated only once every 4 years. Anecdotal evidence suggests that such rules are seldom subject to major revisions.[38]

Given these considerations, in our context the effects of contractual wage growth on employment and wages should provide an estimate of the own-price labor demand elasticity. The remainder of this section illustrates in more detail several relationships between existing theories on the effects of centralized wage setting and our empirical evidence.

6.2. Estimation of the labor demand elasticity across firm-level characteristics

The empirical approach of this study allows us to investigate several hypotheses on the effects of centralized wage bargaining. One strategy to test such theoretical links is based on the estimation of heterogeneities in the labor demand elasticity across firm-level characteristics. Thus, we have relied on the INPS-AIDA panel of incorporated businesses, which included information on value added per worker, revenues, the share of the wage bill of the collective contract in total revenues, and the capital–labor ratio.[39]

A simple comparison of labor demand elasticities separately estimated for different levels of these firms' characteristics would not be optimal. The financial variables considered in this study could be affected by collective agreements and pay scales could be set differently, depending on the average level of these financial indicators within a contract. To overcome these problems, a time-constant measure of distance from the collective agreement average was constructed for each firm-level characteristic.

In particular, we estimated the following regression model

$$f_g = \psi_c + r_g$$

where f_g denotes firm-level characteristics considered, measured as an overall average over the 2007–2015 period. As the underlying panel of firms was balanced, the years used to compute these averages were the same for all firms within a collective agreement. This choice allowed

[34] In a related study, Hijzen and Martins (2020) found negative employment effects associated with collective bargaining extensions through an RDD research design and positive effects of extensions on wages at the bottom of the earnings' distribution.

[35] One important difference between the Portuguese and Italian contexts is that the former is characterized by voluntary (rather than statutory) participation in collective bargaining. Its dispositions can sometimes be applied to the entire economy but only if the government rules for such extension. See Villanueva and Adamopoulou (2022) for comparative evidence on collective contracts' extension mechanisms.

[36] Inefficiencies depend on the fact that the union sets the wage as a monopolist, whereas firms read quantities on their profit-maximizing labor demand. Another way of characterizing this inefficiency is by considering the union as a principal that can set the wage but which cannot prevent the firm (the agent) from choosing employment according to its own interests (see MaCurdy and Pencavel, 1986).

[37] Section 6.3 provides a more specific discussion on some conceptual differences between the standard minimum wage setting and contractual wages, with reference to the Hicks–Marshall theory of labor demand.

[38] Section 2 provides more details on these institutional characteristics.

[39] These variables provide broad measures of a firm's efficiency (value added per worker), size (revenues), labor cost shares, and capital intensity. Table B.4 (in the Appendix) reports descriptive statistics on these outcomes.

us to abstract from year-specific fluctuations in financial indicators and to characterize firms along more persistent dimensions.

The aforementioned equation, in which ψ_c is a collective contract fixed effect and r_g is the residual, was estimated using one observation per firm-collective contract group (as in previous sections, such groups are denoted by g and collective agreements by c). We then constructed five quintiles for the distribution of the estimated residual $\hat{r}_g$ and computed the labor demand elasticity within each. Through this approach, we could characterize the size of the labor demand elasticity along several firm-level dimensions, controlling for differences in composition across collective agreements.

Fig. 4 reports the labor demand elasticity (as estimated through 2SLS) by quintiles of the difference between given firms' outcomes and the mean of collective contracts. All elasticities were estimated controlling for time fixed effects interacted by regions and ISIC 21 industries controls, that is, adopting an equivalent specification to model (2) in Table 4. Table B.5 (in the Appendix) provides the full list of the treatment effect coefficients on wage and employment levels for each quintile of the difference between a firm's characteristics and its collective contract average.

6.3. Hicks-Marshall theory of labor demand

In light of the discussion of Section 6.1, the classical Hicks–Marshall theory of labor demand represents an important framework to consider. In particular, this approach can be helpful in rationalizing the finding of a quite negative own-price employment elasticity. In its simplest version, the Hicks–Marshall model is based on a standard profit-maximization problem with two inputs (labor and capital) and constant returns to scale. Following (Hamermesh, 1993) notation, the own-price elasticity of labor can be defined as

$$\eta_{LL} = -(1-s)\sigma - \eta s$$

where s is the labor share in revenues, σ is the technical rate of substitution between labor and capital, and η is the product-demand elasticity. The first addend in the above elasticity is the substitution effect, which is related to the fact that if the cost of labor increases, more capital is used in the production process. The last term is the scale effect, which captures the fact that the output price increases as the wage increases, also determining a negative effect on output levels, which depends on the size of the product-demand elasticity η.

The characteristics of the product market structure may help rationalize the relatively large size of employment effects associated with wage growth documented in this study. In a perfectly competitive environment, if only a single firm is hit by a wage shock, its own-price labor demand elasticity tends to infinity. If, instead, the wage shock affects all firms, then the usual scale effect occurs (Hamermesh, 1993).

This model can be easily extended to the case of monopolistic competition. In this setting, each firm supplies a product variety. Firms face a demand function, given the constant prices of other products, and a demand function for a general change in prices of all varieties.[40] A conventional condition of similar models is that the former demand curve is generally more elastic than the latter (*e.g.*, Dixit and Stiglitz, 1977). It follows from this consideration that when only a few firms within a market are hit by wage growth, the reduction in demand they face tends to be larger with respect to the case where all producers are affected by the same factor price shock.

Our empirical approach was based on a comparison between firms that were hit by the factor price shock and a counterfactual group of similar firms not affected by wage growth. This choice reflects the characteristics of collective wage bargaining. This institution asymmetrically affects firms within a given sector and region. In this setting, affected firms and their comparison groups potentially share the same product market. Given the theoretical considerations for the case of monopolistic competition, a rather negative own-price labor demand elasticity can be expected from such asymmetric shocks. By contrast, minimum wage policies may affect all firms symmetrically, provided that they all hire affected (low-wage) workers. Therefore, pass-through mechanisms on consumers could lead to lower reductions in output levels in response to such policies.

Furthermore, the Hicks–Marshall model of labor demand predicts that the larger the labor share in total costs, the larger the employment adjustments to wage growth, provided that the product-demand is sufficiently elastic.[41] In this regard, the top panel of Fig. 4 shows that the estimated labor demand was not statistically different from zero among firms whose labor costs specific to the collective agreement represented a smaller share of total revenues. This evidence is consistent with the theory.

Fig. 4 also shows that the elasticity of labor demand had an inverse U-shape when considering its heterogeneity across firms' capital intensity. The fact that more labor-intensive firms had more negative employment responses to wage growth is broadly consistent with the standard predictions. Indeed, employment effects should be significantly negative whenever the technology more easily allows for labor–capital substitution.[42] The observation of more negative elasticities at most capital-intensive establishments is less straightforward to rationalize. In part, this result could be related to cash constraints. That is, firms with excess capacity could be less likely to hoard labor when hit by a factor cost shock owing to lack of resources (see *e.g.*, Giroud and Mueller, 2017).

6.4. Vintage model of firms' creation with centralized wage setting

Moene and Wallerstein (1997) proposed an influential hypothesis on the effects of centralized wage bargaining, which is based on a vintage model of firms' creation with heterogeneous efficiency.[43] This model assumes that collectively bargained centralized wage standards are typically adopted by firms in which pay levels would be higher under a decentralized equilibrium.[44] In such a setting, the most efficient employers can potentially benefit from excess profits as wages are not directly linked to workers' usefulness to firms or to their outside options.

An implication derived from this theory is that centralized wage setting may be conducive to more innovation, as efficient firms are advantaged within this system (Barth et al., 2014). However, if the process of destruction of inefficient companies is achieved at the cost of lower employment, then it would be less attractive in contexts of high unemployment rates (Boeri et al., 2021 provide a similar argument).

The hypothesis that most employment losses should be concentrated among marginal and less efficient firms appears consistent with the results reported in the bottom panels of Fig. 4. The labor demand elasticity was more negative when estimated among relatively smaller firms within a collective contract. Moreover, it was significantly negative among firms with relatively low value added per worker levels. These two patterns may reflect similar underlying mechanisms, as size and productivity tend to be positively correlated.

[40] These functions are usually referred to as the *dd* and *DD* Chamberlinian curves.

[41] Stated differently, η_{LL} is decreasing in s as long as $\sigma < \eta$, a result known as one of the Hicks–Marshall laws of derived demand.

[42] This conclusion holds if companies that adopt a relatively less capital-intensive production process with respect to the collective contract average can more easily substitute away from labor.

[43] A similar version of this hypothesis was formalized by Agell and Lommerud (1993) as well.

[44] In this regard, Wallerstein (1999) provides a cross-country evaluation of the link between wage equality and pay-setting institutions and a critical discussion of a variety of evidence that fits well with this modeling choice of Moene and Wallerstein (1997).

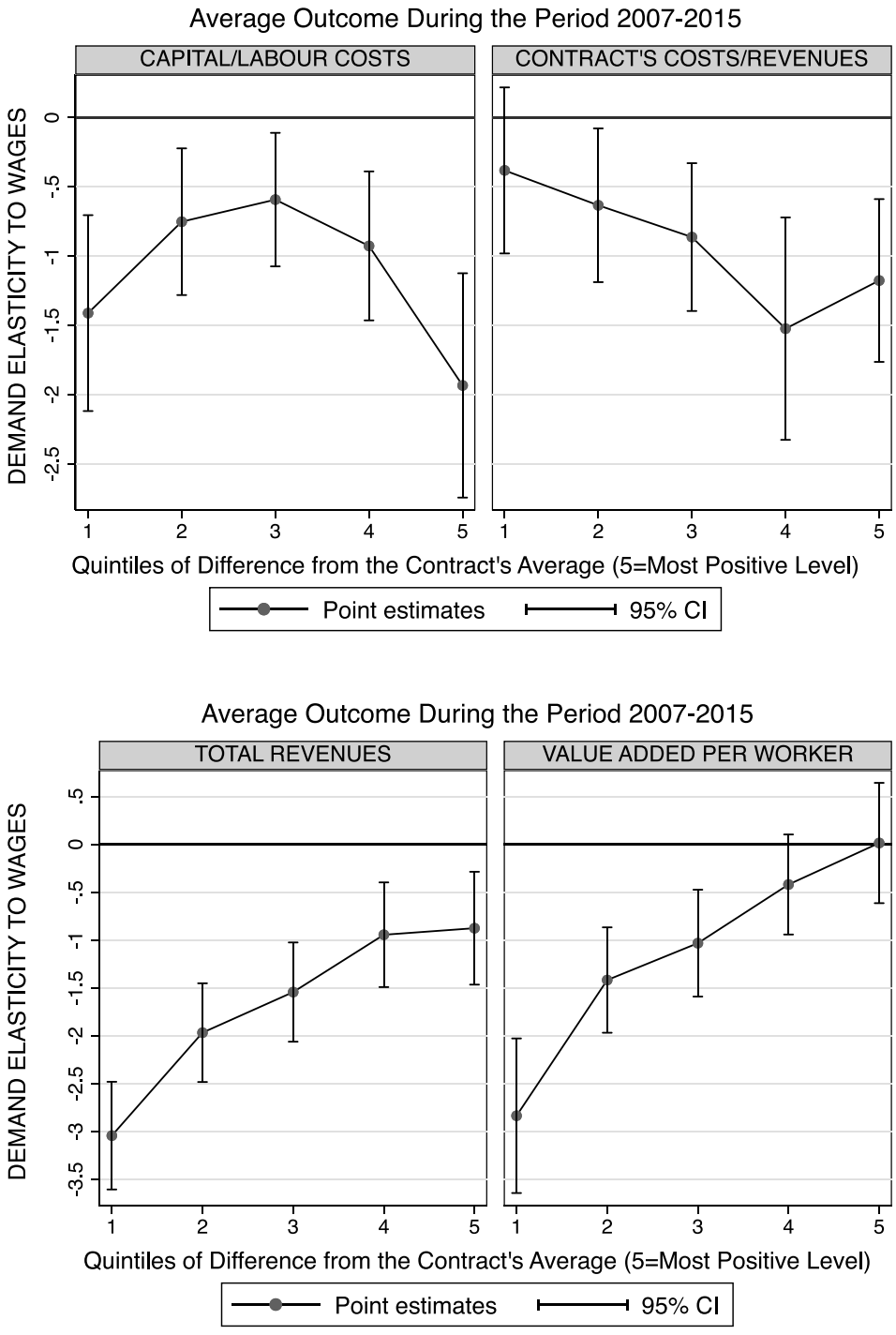

Fig. 4. Labor demand elasticity across quintiles of average firm level characteristics.
Each graph shows the labor demand elasticity by levels of a firm characteristic estimated using 2SLS. The model specification interacted contractual wages with indicators for quintiles of the difference from the average firm-level characteristic of the collective contract, controlling for time fixed effects interacted by regions and ISIC 21 industries controls. These quintiles were time-constant and defined using the procedure described in Section 6.2. Table B.4 (in the Appendix) reports descriptive statistics on each firm-level characteristic. Table B.5 (in the Appendix) provides the full list of the treatment effect coefficients on wage and employment levels for each quintile of the difference between a firm's characteristic and its collective contract average.

Interestingly, firms with high value added per worker did not experience employment losses for a given growth in contractual wages. This indicates the presence of rents among best-performing companies, which would be consistent with the theory proposed by Moene and Wallerstein (1997). Such rents could be linked *e.g.*, to higher monopsony power or to the ability to limit employment losses through labor hoarding (*i.e.*, draining other firms' resources, such as liquidity, see Giroud and Mueller, 2017).

Another potentially relevant mechanism is that relatively small and less efficient companies may have less influence on the wage-setting

Table 5
Effects of pay scales on hiring and separation rates.

Dependent variable:	*Hiring rate*	*Separation rate*
Outcome average	0.040	0.039
Outcome st. dev.	0.072	0.073
Coefficients		
PS_{ct}	**−0.020****	**−0.001**
S.e.	*0.004*	*0.003*
Activity rate	−0.000**	0.000
S.e.	*0.000*	*0.000*
Unemployment	−0.000	0.000
S.e.	*0.000*	*0.000*
Fixed Effects		
Group	✓	✓
Time∗ISIC 22∗region	✓	✓
Adjusted R^2	0.374	0.385
RMSE	0.057	0.057
N. of observations	17.336M.	17.336M.

∗∗: 1%; ∗: 5% significance levels. Results computed on the entire INPS sample. Groups are defined by the interaction of collective contracts, local labor markets, and two-digit sectors. Outcomes defined as number of new hires or separations over the current group size. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available).

process within their collective contract. That is, such firms may not be able to negotiate a wage growth tailored to their needs. This mechanism would still be coherent with the Moene and Wallerstein (1997) hypotheses. Moreover, it would also rationalize the active support for centralized wage-setting procedures often expressed by the largest Italian employers' associations, which tend to be more representative among relatively efficient companies (Fanfani et al., 2023).

A formal analysis of differences in adjustment mechanisms adopted by firms across the productivity distribution was provided by Devicienti and Fanfani (2021), whose results were broadly consistent with the evidence of this study, adopting a similar identification approach. Moreover, the hypothesis of a general adherence to centralized standards and of the pervasiveness of "wage moderation" is consistent with available evidence on the Italian wage structure, particularly the relatively limited size of geographical pay differences (Boeri et al., 2021, Belloc et al., 2023) and the limited contribution of employers' pay heterogeneity in shaping the evolution of Italian inequality (Devicienti et al., 2019).

6.5. Employment adjustments among outsiders and incumbent workers and the membership theory

Table B.2 shows that the employment effects of contractual wage growth were strong and negative even when the outcome was defined using the number of workers employed within each group, instead of its FTE amount. This evidence suggests that firms adjusted to this policy on the extensive margin. Moreover, it rules out the hypothesis that misreporting of days worked, or similar mechanisms potentially used to avoid compliance, had a major influence on our results.

A further mechanism to explore is the difference between employment effects on new hires and effects on separations. This approach allows us to test whether union contracts are designed to benefit incumbents, which is a classical implication of the *membership* theory of unionized labor markets (Lindbeck and Snower, 1986). According to this model, the unions' objective function is skewed toward the welfare of *insiders*, who are considered to be workers currently employed. In this setting, union contracts could be designed to prevent the involuntarily unemployed from underbidding in order to find a job. This theory has relevant implications. For example, it has been considered important in explaining the hysteresis in European employment trends (see in particular Blanchard and Summers, 1986).

Table 5 reports on an analysis of the effects of contractual wage growth on two outcomes: hiring rates and separation rates of incumbent workers.[45] These outcomes were computed, respectively, by taking the ratio between new workers and total workers within groups, and between workers in their last month of employment within the group and total workers. These monthly hiring and separation rates were similar, on average, with a level of about 4%.

An estimation of our standard regression model on these outcomes found a significant negative effect of contractual wage growth on hiring rates. Hires typically decreased by 0.5% with respect to their average for a 1% growth in contractual pay levels. On the other hand, no significant effects could be found on separations.[46] Given the absence of information on the nature of separations, we were not able to test more nuanced mechanisms on this latter result. For example, testing whether this process was driven by a combination of lower quits and higher layoffs was not possible.

Overall, the evidence of Table 5 is consistent with *membership* theories of unions. Extensive margin negative employment effects adversely affected *outsiders*, that is, those currently unemployed and potentially available to work for the jobs that were affected by the wage shock. In principle, outsiders could underbid by proposing to work at the pay floor level, which could be lower than incumbents' rigid wages. However, the results in this section suggest that the amount of underbidding allowed by collective contracts was probably not enough to compensate for search and replacement costs incurred by firms.

7. Stacked event study evidence on the dynamics in the employment effects

In this section, we provide further evidence on the dynamics in the employment effects of contractual wage growth. For this purpose, we rely on a stacked event study estimator (Cengiz et al., 2019). This approach requires the creation of separate datasets around each contractual wage increase, also called an *event*. As the pay floors considered in this study have been changing quite frequently (on average once every 14 months), we could build these datasets for only 87 contracts and 437 contractual wage changes, out of the 1,414 changes observed throughout the study period.

For each event, defined as a contractual wage increase greater than 1% and smaller than 5%, we have measured employment levels within grouped observations during a 14-month window that included 7 months before and 7 months after this shock. The control sample included all observations belonging to collective agreements that were unaffected by contractual wage growth during the same period.[47] To limit the influence of contractual wage changes occurring shortly before or after the dataset periods, we have also excluded treated and control groups for which a pay floor change occurred within 3 months before or after the window of observation.

We have estimated the average employment effect of the contractual wage increase across events using the following model. Let g represent the index groups, defined by the interaction of collective agreements, LLM, and two-digit sectors; e represent the index contractual wage growth events; t the index time periods (months), where $t = 0$ when the contractual wage increases in the treated groups; and l and s the

[45] Owing to data limitations, it was not possible to distinguish separations generated by voluntary quits and those that were a result of layoffs.

[46] This evidence is consistent with several minimum wage studies highlighting that the negative employment effects of such policies tend to be driven by reduced hires and more restrictive hiring practices rather than higher quits or layoffs (Portugal and Cardoso, 2006, Clemens et al., 2021, Gopalan et al., 2021, and Jardim et al., 2022). Similar results have been documented by Martins (2021) while analyzing the employment effects of collective bargaining contract extensions in Portugal.

[47] We have eliminated all events where an unaffected control group could not be identified.

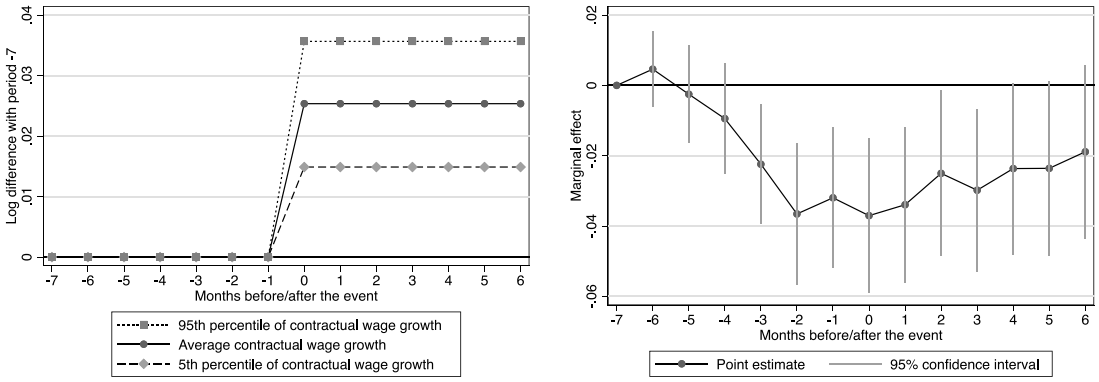

Fig. 5. Stacked event study employment effects of contractual wage growth.
The left panel shows the log difference (with respect to period $t = -7$) in the median nominal pay scale of the collective contract among groups affected by contractual wage growth at time $t = 0$. The average 5th percentile and 95th percentile of this difference are shown. There were 99,232 treated groups affected by 437 contractual wage changes of 87 contracts in the sample, for a total of 1,389,148 treated group-month observations.
The right panel shows the estimated parameters β_a of Eq. (3) and their 95% confidence intervals for each period before and after the contractual wage increase. The period $t = -7$ was used as the reference period. The sample size was 2,673,720 group-month observations, of which 1,284,472 (corresponding to 91,232 groups) were always unaffected by contractual wage growth. The regression was weighted by the group size and standard errors were clustered at the group level.

index less detailed geographical units and sectors, respectively. The regression equation of interest can be written as

$$y_{get} = \sum_{a=-7}^{6} \beta_a 1[\mathrm{PS}_{ge(t-a)} > \mathrm{PS}_{ge(t-a-1)}] + \alpha_{ge} + \phi_{slet} + \epsilon_{get} \quad (3)$$

where y_{get} is the log FTE number of workers in group g during event e at time t divided by the size of the local labor market workforce; PS_{get} is the median nominal pay scale of the collective contract; $1[\mathrm{PS}_{ge(t-a)} > \mathrm{PS}_{ge(t-a-1)}]$ is an indicator variable for positive changes occurring in contractual wages between $t + a - 1$ and $t + a$; α_{ge} is a group by event fixed effect; ϕ_{slet} is a time by sector, location, and event fixed effect; and ϵ_{get} is the residual.

In this model β_a measures the percentage difference in employment growth between treated and control groups each month before the contractual wage increase, if $a < 0$, and after this shock, if $a \geq 0$. This parameter is estimated based on local- and sector-specific employment shocks that are common across treated and control groups.

Results obtained from difference in differences models with variation in treatment timing may provide a biased estimate of the ATT in the presence of heterogeneity (Goodman-Bacon, 2021). While most methodological innovations in this recent literature do not extend to the continuous treatment case, the stacked event study setup presented above can also be considered a feasible robustness test in our application. The advantage of similar event study specifications is that they allow us to estimate (and control for) the dynamics of treatment effects, which could potentially represent the most severe source of bias in static difference in differences models (Baker et al., 2022).

The left panel of Fig. 5 shows the growth of contractual wages across time in the treated groups at the 5th percentile, mean, and 95th percentile. On average, treated groups were subject to a 2.5% growth in contractual wages at $t = 0$, while wage growth was always zero in the control groups. The right panel of Fig. 5 shows the estimated parameters β_a. Employment growth was not statistically different in the treated and control groups up to four months before the wage shock. From three months before the event onward, employment growth decreased by a magnitude of up to 3.7% in the period contemporaneous with the shock, corresponding to an implied elasticity to contractual wage growth close to −1.5. In the sixth month after the event the estimated employment effect was still negative, but not statistically significant.

The evidence provided by Fig. 5 suggests that contractual wage growth had a significant negative impact on employment dynamics in the treated group, a result consistent with our main findings. The size of the employment effect was considerable, but its timing was partly

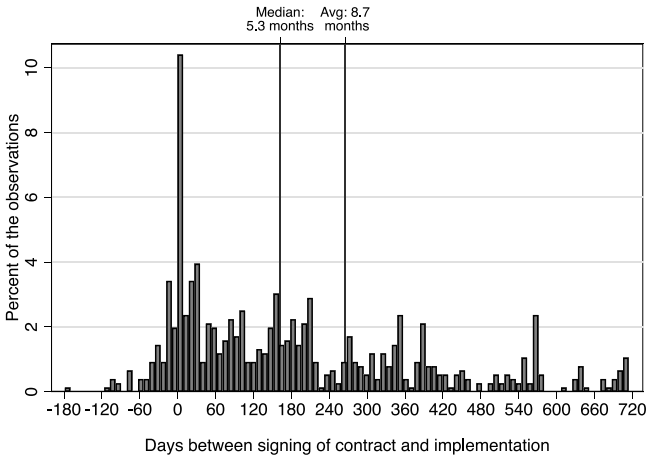

Fig. 6. Distribution of the distance between the signing of contracts and their implementation.
Histogram of the difference in days between the signing of a collective contract and the implementation of its dispositions. The graph is derived from the CNEL archives and includes all renewals that were registered in the period between January 2006 and December 2016, and which were signed by the largest Italian trade union (CGIL). The sample consists of 849 contract renewals. For graphical convenience, the sample used to draw the histogram is trimmed at the 1st and 90th percentiles. The average and median refer to the full sample statistic.

anticipated with respect to the timing of the policy. In this regard, we now discuss in more detail, also using theoretical arguments, what the expected shape of employment adjustments across time should be, given the characteristics of the policy under study.

First, it should be noted that contractual wage growth is typically announced before it is actually implemented. Thus, it tends to be perfectly predictable before it comes into effect with some, potentially long, anticipation. There is some indirect evidence supporting this claim. For example, contractual wages change on average once every 14 months in our sample, while negotiations on wage levels typically occur only once every two years. This discrepancy can be explained by the fact that, at each renewal, bargaining parties usually agree on a series of planned and gradual wage increments that come into effect at future dates.[48]

[48] A relevant example, given the large size of the collective contract in question, was the renewal of the metal-manufacturing contract signed on

More direct evidence on the presence of announcements regarding bargained wage growth can be derived from the CNEL archive of collective contracts.[49] This archive reports the dates when each collective contract renewal was signed and when it came into effect. The latter is not necessarily the date when the first contractual wage increase after the renewal is implemented, but it can be considered a good proxy for it. Fig. 6 shows a histogram of the distance (in days) between the signing of a collective contract renewal and its actual implementation.[50] As can be noted, the average distance between a renewal and its implementation was of 8.7 months in the years covered by our study, and the median was of around 5.3 months. Moreover, only in around 10% of the renewals the implementation was contemporaneous with the signing of contracts.[51] Thus, Fig. 6 provides direct evidence showing that bargained wage increments are typically announced before their implementation.

Given the above considerations, the consequences of studying the effects of announced wage shocks should be discussed. In a frictionless economy there should be no differences between expected and unexpected wage shocks, as firms can immediately adjust along their profit-maximizing demand curve once the new pay schedule is implemented. However, in a standard job search model with frictions, the employment effects of expected wage increases tend to be in part anticipated, as it takes time for firms to adjust their workforce composition to the new pay schedule (Pinoli, 2010). This does not imply that employment effects do not occur when wage shocks are expected, but rather that adjustments could potentially be less sharp at each point in time around the policy change, and that they could start to take place even before the shock.

The expected dynamics of adjustments to pay floors were also analyzed by Sorkin (2015), who argues that employment effects may not fully emerge in the short run. This is because capital and technologies are not easy for firms to change once they are installed. In the (Sorkin, 2015) model, rational expectations about wage shocks are always assumed, while uncertainty can potentially lead to short-run adjustments that depart from the true structural relationship between labor costs and employment, owing to wrong investments resulting in excess or reduced capacity with respect to the new wage schedule. Thus, adopting this theoretical perspective, analyzing announced wage shocks should be preferable.

All things considered, the presence of anticipatory effects in Fig. 5, which start from around three months before the policy shock, and which have the same sign as post-policy effects, should not be surprising. Indeed, the shape of these dynamics is consistent with predictions from labor market models where firms have rational expectations about the policy and face frictions in adjusting their employment levels. Nevertheless, there are some remaining issues to be discussed.

First, given that employment adjustments were partly anticipated, the static specification of Section 5, where employment is regressed on contemporaneous contractual wage levels, might not be the most appropriate choice. In this regard, the last row in Table B.1 shows that including the three-month lead of the contractual wage as the independent variable of interest, instead of the contemporaneous term, had hardly any effect on the main estimates. This specification would be the most natural if most of the employment losses took place around three months before the contractual wage shock.

The fact that results from the generalized difference in differences model were not very sensitive to the choice of the timing in the measurement of the independent variable can be linked to the shape of contractual wage dynamics. Contractual wages are a highly persistent autocorrelated process. This means that lags or leads that are potentially relevant whenever there are dynamic effects, are also positively correlated with the contemporaneous level of the contractual wage, with a correlation coefficient that tends to decrease with the distance in time between them. Thus, if the effects of contractual wages span over more than one period, omitted relevant leads and lags affect the estimated parameter of the static model according to the standard omitted variable bias formula.[52] These considerations suggest that the two-way fixed effects specification that forms the main analysis of the paper tends to be biased toward the cumulative effect of the policy, and they rationalize why its estimates are not particularly sensitive to the choice of the exact timing at which contractual wages are measured with respect to the outcome of interest.

A second issue that emerges from Fig. 5 concerns the test for parallel trends. Given that employment adjustments were partly anticipated, the number of periods on which the parallel trend assumption was tested in the specification of Fig. 5 is quite limited. To test whether parallel trends hold even in periods that are further away from the policy shock, we have built a second stacked dataset. In this case, we have selected all observations where a contractual wage increase greater than 1% and smaller than 5% occurred in the 10th month of a 14-month window. Thus, we were able to observe treated groups for nine periods before the wage shock, and up to three months after this shock occurred. As in the specification reported in Fig. 5, we have selected as controls all observations for which no contractual wage growth occurred during the same period, and we excluded all cases where a wage shock occurred three months before or after the observation window. The resulting datasets consisted of 375 wage changes affecting 81 collective contracts among the treated groups.

Fig. 7 shows the results obtained by estimating the model of Eq. (3) on a 14-month window between 10 months before and three months after the policy shock. The left panel shows descriptive statistics for the dynamics of contractual wages in the treated groups. The right panel reports the estimated regression coefficients and the 95% confidence intervals. As can be noted, the parallel trend between treated and control groups was never rejected in more distant periods prior to the policy shock. Significant negative employment effects start to emerge around two months before the growth in contractual wages. The largest negative employment effect occurred in period -1, with a marginal effect of −0.037, which corresponds to an elasticity to contractual wage growth of around −1.4. The employment effects were still negative in the last three periods, but they were smaller in absolute value and only marginally significant at the 10% level.

All things considered, the evidence in Fig. 7 supports the identifying assumptions of our empirical models, which controlled for business cycle dynamics through sector- and location-specific time effects. Indeed,

December 5th 2012, which set planned wage growth events to take place on January 1st 2013, 2014, and 2015.

[49] The *Consiglio Nazionale dell'Economia e del Lavoro* is a public research center that is in charge of keeping the register of collective contracts in Italy.

[50] The CNEL archive contains many more collective contracts with respect to the main representative ones, which are those typically registered in the social security contribution records of INPS. Indeed, CNEL has an obligation to register a collective contract even in cases where it is signed by just a few firms and workers. Thus, there could be a limited overlap between the collective contracts registered by CNEL and those considered in our sample of analysis. Moreover, CNEL registers collective contracts using a different identification code with respect to INPS, which makes a deterministic matching between the two datasets difficult. For these reasons, in constructing Fig. 6 we have selected only collective contracts in the CNEL archives for which the largest Italian trade union (CGIL) was among the bargaining parties that signed it. This choice is likely to greatly increase the overlap between collective contracts considered in the CNEL and INPS archives.

[51] There are cases where renewals are retroactive. Apart from potential errors in the data, this may actually occur if there were delays in the renewals. In this case, the implementation of contracts could be backdated to the time at which the last collective contract expired. In terms of wages, delays are usually compensated with a higher contractual pay growth that takes into account the flat growth that occurred during the period of absence of renewals.

[52] Discussions related to this point can be found in Neumark and Wascher (1992), Baker et al. (1999), and, more recently, in Meer and West (2016).

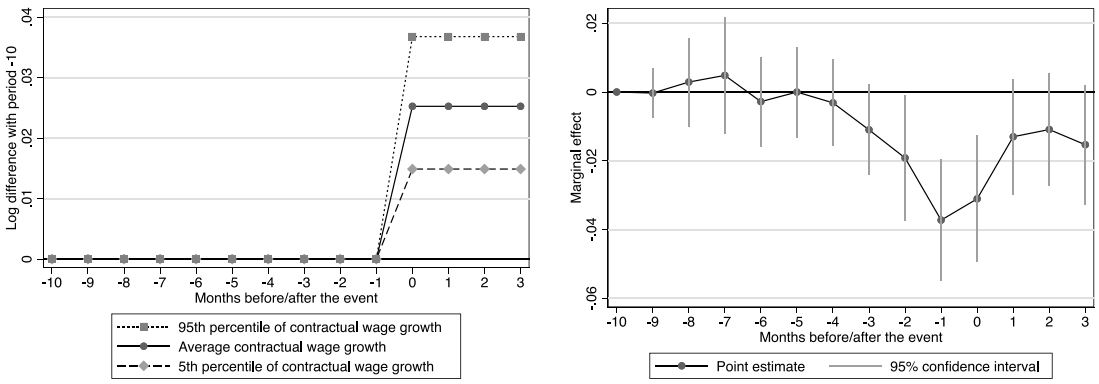

Fig. 7. Stacked event study employment effects of contractual wage growth using longer Pre-Trends.
The left panel shows the log difference (with respect to period $t = -10$) in the median nominal pay scale of the collective contract among groups affected by contractual wage growth at time $t = 0$. The average, 5th percentile, and 95th percentile of this difference are shown. There were 87,677 treated groups, affected by 375 contractual wage changes of 81 collective contracts in the sample, for a total of 1,227,478 treated group-month observations.
The right panel shows the estimated parameters β_a of Eq. (3) and their 95% confidence intervals for each period before and after the contractual wage increase. The period $t = -10$ was used as the reference period. The sample size was 2,265,032 group-month observations, of which 1,037,554 (corresponding to 74,111 groups) were always unaffected by contractual wage growth. The regression was weighted by the group size and standard errors were clustered at the group level.

the presence of parallel trends between treated and control groups was confirmed in this specification. There were significant anticipatory effects in this specification, too, which we interpret as announcement effects that can be rationalized by frictions faced by firms in adjusting their employment levels. Employment effects were still negative and significant in this specification, but they were not long-lasting, even if still marginally significant three months after the wage shock.

8. Conclusions

This study shows that Italian collective bargaining has a positive influence on wages and a considerable negative effect on employment. Strong negative employment adjustments among firms affected by bargained wage growth could be explained through several mechanisms.

First, the Italian economy was characterized by low economic growth throughout the study period. This trend was also accompanied by low inflation rates, with resulting positive real growth rates of contractual pay levels between 2006 and 2016. Second, the wage shocks considered in this study were affecting virtually the entire distribution, rather than a small portion of the workforce. Finally, wage shocks were affecting firms asymmetrically within similar markets. In this context, consumers could be more responsive to price changes by individual companies, and, consequently, negative employment effects could be more pronounced.

This study shows that negative employment effects were prevalent among young workers, fixed-term contracts, those currently unemployed, and relatively less efficient firms. Moreover, these effects were stronger at firms where the share of the wage bill in revenues was higher. The shape of these adjustments was broadly consistent with several theories of wage setting in unionized labor markets.

Italian collective bargaining seems well characterized by models where firms set employment according to their labor demand, rather than on an efficient contract curve. Moreover, the standard Hicks–Marshall theory provides several predictions consistent with our results. The *membership* theory, according to which unions tend to maximize the welfare of currently employed workers, seems also well grounded in the Italian context, and it may have more general macroeconomic implications (Blanchard and Summers, 1986). Finally, some important implications of the *vintage* model of firm entry (Moene and Wallerstein, 1997), which suggests that efficient firms could benefit from centralization in wage bargaining, were also supported by our results.

The welfare implications of centralized collective wage bargaining could be further explored focusing on general equilibrium effects. This study has adopted a *cross-sectional* approach, comparing outcomes between firms affected and unaffected by wage growth. This approach did not consider more general implications mediated by shocks potentially affecting both groups, such as competition in the credit and labor market, or feedback across firms within value chains. Better accounting for similar factors could substantially improve our understanding of the aggregate employment effects of collective wage bargaining.

Declaration of competing interest

I hereby declare to have no interest to declare.

Data availability

The authors do not have permission to share data

Appendix A. Effects of contractual wages across activities, population groups, and the business cycle

We have investigated how the effects of pay levels set by collective bargaining varied across industries, population groups, and the business cycle. Notice that each sector and population group was typically subject to different collective agreements, which could have set more or less binding provisions with respect to a market-clearing wage. However, the comparison of wage and employment effects of pay scales still allows to recover an implied labor demand elasticity.

Table A.1 provides the sector-specific elasticities of average wages and employment to contractual pay levels. We have defined industries using the ISIC rev. 4 eleven-group (or *high-level*) classification. Results in the left coefficients' column of Table A.1 show that there was a significant underlying variability in the effectiveness of collective bargaining, given that the same growth in contractual wages had always significant, but also heterogeneous effects on pay levels across sectors. The highest sensitivity of wages to statutory compensations was observed in finance and insurance activities (with an elasticity of 1.49); the lowest among human care, public services, and social work activities (0.13); but, with regard to other relatively large sectors, all of the estimates fell in a narrower range between 0.3 and 0.6.

Several reasons could drive this variability. In part, it can be attributed to differences in the diffusion and application of firm-level and even individual-level labor contracts, through which employers can provide performance-related and additional pay components on top of contractual wages. These flexible top-up components could make the growth in actual wages different from the rate set by collective

Table A.1
Wage and Employment Effects of Pay Scales across Industries.

Linear combinations of:	*Dep. Variable*		Weighted
PS_{ct} *and its industry interactions*	Groups' avg. wages	Groups' FTE empl.	industries' frequency
Agriculture	**0.221****	**−0.346**	0.5%
S.e.	*0.051*	*0.268*	
Quarrying and industrial act.	**0.566****	**0.387**	1.2%
S.e.	*0.061*	*0.259*	
Manufacturing	**0.578****	**−0.255***	33%
S.e.	*0.023*	*0.103*	
Construction	**0.306****	**−1.107****	9.6%
S.e.	*0.033*	*0.226*	
Trade, transport, & accommodation	**0.352****	**0.203**	29.1%
S.e.	*0.038*	*0.110*	
IT & communications	**0.306****	**−2.506****	3.4%
S.e.	*0.071*	*0.557*	
Finance & insurance	**1.494****	**−0.574****	2.8%
S.e.	*0.117*	*0.222*	
Real estate	**0.695****	**1.716****	0.4%
S.e.	*0.133*	*0.505*	
Professional, technical, & support services	**0.466****	**−0.292**	11.4%
S.e.	*0.051*	*0.232*	
Human care, public services, & social work	**0.133***	**−0.415***	4.5%
S.e.	*0.062*	*0.197*	
Other services	**0.416****	**−1.267****	4.1%
S.e.	*0.063*	*0.259*	
Controls			
Unemployment	✓	✓	
Activity rate	✓	✓	
Fixed Effects			
Group	✓	✓	
Time∗ISIC 22∗region	✓	✓	
Adjusted R^2	0.895	0.976	
RMSE	0.119	0.253	
N. of observations	17.363M.	17.366M.	

∗∗: 1%; ∗: 5% significance levels. Estimates performed on the entire INPS sample. Groups are defined by the interaction of collective contracts, local labor markets and two-digit sectors. All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available). Sectors are defined according to the ISIC rev. 4 high-level industries classification.

bargaining. However, part of the heterogeneity in the elasticity of wages to contractual pay levels across sectors could also reflect lower measurement precision, since in this interacted model the number of policy effects to be estimated was higher -and the number of available contrasts for each parameter lower- than in the baseline specification (a similar consideration holds for employment effects).

The right coefficients' column in Table A.1 provides estimates of the elasticity of employment to contractual wages. The classical theory of labor demand suggests that this parameter should be smaller, the less price-elastic the product demand faced by firms (*e.g.*, Hamermesh, 1993). Our results are not completely consistent with this mechanism. For example, the sensitivity of employment to contractual wages was higher in the construction sector, which is considered the classical example of a non-tradeable industry, with respect to manufacturing, which is a typically tradeable sector. However, some of the other relationships along this line followed a more expected pattern (*e.g.*, the null effect in the tourism-transport-trade industry). This suggests that pass-through mechanisms on consumer prices were limited even in some relatively insulated domestic markets. Notice that tradeability is usually taken as a proxy for the presence of competitors not affected by higher costs related to contractual wages. In our context this element could also vary depending on the market share of self-employed, or on the degree of homogeneity and coordination among collective agreements applied within a given sector.[53]

Table A.2 presents the wage and employment elasticities to contractual pay levels computed across population groups (manual/clerical occupations, prime-aged, young, old, open-ended, and fixed term contract workers). To obtain these estimates, we have constructed separate grouped samples for each age, occupation, and type of contract, using a procedure equivalent to that applied in constructing the entire INPS sample. From the top part of the table, it can be noted that the effects of collective bargaining on wages were strong among each type of worker, and more stable than those documented across sectors. However, there was a tendency for pay levels of young workers and fixed-term contracts to be more sensitive to changes in contractual wages, which is likely to be driven by a lower incidence of top-up components of remuneration among these type of employees.

The lower part of Table A.2 shows that the employment effects of collective bargaining across population groups were quite heterogeneous. Significant negative elasticities were found among all occupations, but were stronger among non-manual ones. Interestingly, only prime-aged, young, and fixed-term contract workers' employment levels were influenced by this institution. In contrast, older employees and those with an open-ended contract characterized by high levels of employment protection -two characteristics that often overlap in the Italian context- were not affected, consistent with cross-country evidence on the effects of minimum wages, which appear to be stronger where employment protection legislation standards are lower (see in particular Neumark and Wascher, 2004). However, this heterogeneity may also be driven in part by the self-selection of marginal, less trained, and less productive workers into temporary contracts (see *e.g.*, Berton and Garibaldi, 2012).

Table A.3 summarizes the results obtained from an analysis on the heterogeneity in the effects of contractual wages across local business cycle conditions. In particular, we have divided local labor markets into groups where the unemployment rate was higher than in the previous

[53] Section 6.3 provides a more formal discussion of this point.

Table A.2
Wage and Employment Effects of Pay Scales across Population Groups.

Population group	Clerical Occ.	Manual Occ.	16–29	30–49	50–70	Open-Ended	Fixed-Term
Dependent variable	*Group's Avg. Log Wages*						
Coefficient							
PS_{ct}	**0.435****	**0.421****	**0.512****	**0.447****	**0.472****	**0.449****	**0.602****
S.e.	*0.024*	*0.024*	*0.030*	*0.019*	*0.023*	*0.017*	*0.050*
Controls							
Unemployment	✓	✓	✓	✓	✓	✓	✓
Activity rate	✓	✓	✓	✓	✓	✓	✓
Fixed Effects							
Group	✓	✓	✓	✓	✓	✓	✓
Time∗ISIC 22∗region	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.903	0.846	0.804	0.881	0.885	0.903	0.733
RMSE	0.118	0.135	0.148	0.123	0.147	0.115	0.205
N. of observations	12,4M	12,8M	11,4M	15,3M	10,8M	16,5M	8,2M
Dependent variable	*Group's Log FTE Employment Rate*						
Coefficient							
PS_{ct}	**−0.518****	**−0.197***	**−0.812****	**−0.311****	**0.104**	**−0.048**	**−1.495****
S.e.	*0.123*	*0.092*	*0.120*	*0.092*	*0.089*	*0.076*	*0.250*
Controls							
Unemployment	✓	✓	✓	✓	✓	✓	✓
Activity rate	✓	✓	✓	✓	✓	✓	✓
Fixed Effects							
Group	✓	✓	✓	✓	✓	✓	✓
Time∗ISIC 22∗region	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.983	0.968	0.967	0.976	0.972	0.979	0.941
RMSE	0.237	0.298	0.319	0.267	0.280	0.244	0.479
N. of observations	12,4M	12,8M	11,4M	15,3M	10,8M	16,5M	8,2M

∗∗: 1%; ∗: 5% significance levels. Estimates performed on specific subsamples derived from the entire INPS archives for each population segment. Groups are defined by the interaction of collective contracts, local labor markets, and two-digit sectors. All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available).

Table A.3
Wage and employment effects of pay scales across local business cycle fluctuations.

Linear combinations of:	*Dep. Variable*		Weighted
PS_{ct} *and its interactions with LLM unemployment growth indicators*	Groups' avg. wages	Groups' FTE empl.	frequency
Negative yearly LLM unemployment growth	**0.482****	**−0.270****	63.7%
S.e.	*0.019*	*0.082*	
Positive yearly LLM unemployment growth	**0.483****	**−0.267****	36.3%
S.e.	*0.019*	*0.082*	
Controls			
Unemployment	✓	✓	
Activity rate	✓	✓	
Fixed Effects			
Group	✓	✓	
Time∗ISIC 22∗region	✓	✓	
Adjusted R^2	0.897	0.979	
RMSE	0.119	0.250	
N. of observations	15.881M.	15.883M.	

∗∗: 1%; ∗: 5% significance levels. Estimates performed on the entire INPS sample. Groups are defined by the interaction of collective contracts, local labor markets, and two-digit sectors. All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available). Unemployment growth indicators denote whether the current year's unemployment rate of the local labor market was higher or lower than in the previous year.

year – which was the case for around one third of the local labor markets in each month – and groups where the local unemployment was instead lower. We have interacted the policy variable by this indicator for business cycle conditions and estimated our main regression model on the entire INPS sample, excluding the first available year (2006).

As can be noted, differences in the results across local labor market conditions were negligible regarding the influence of contractual wages on pay levels and on employment. However, local unemployment measures often could not represent an accurate approximation for the heterogeneity in business cycle conditions faced by individual firms.

Appendix B. Other tables

See Tables B.1–B.5

Appendix C. Further data documentation

In this section, we present the full list of collective contracts that we have included in our analyses, together with the period during which each of these agreements was covered by our sample. The list of contracts is presented separately for the *entire INPS* and the *INPS-AIDA*

Table B.1
Employment effects of pay scales from alternative specifications.

Only time and group FE as controls				
Coefficient	St. err.	Adj. R^2	RMSE	Obs.
−0.686**	0.068	0.973	0.277	17.366M.
Employment not divided by time-varying local workforce size				
Coefficient	St. err.	Adj. R^2	RMSE	Obs.
−0.362**	0.083	0.976	0.263	17.366M.
Regression not weighted by group size				
Coefficient	St. err.	Adj. R^2	RMSE	Obs.
−0.376**	0.040	0.924	0.570	17.366M.
Regression using the 3-month lead of contractual wages				
Coefficient	St. err.	Adj. R^2	RMSE	Obs.
−0.340**	0.079	0.976	0.263	16.462M.

**: 1%; *: 5% significance levels. Results computed on the entire INPS sample. Groups are defined by the interaction of collective contracts, local labor markets, and two-digit sectors. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available).

Table B.2
Effect of pay scales on employment — Alternative definitions of the main variables.

Sample	*Entire INPS*		*INPS-AIDA*	
Dependent Variable				
Group's Log FTE Empl. Rate	✓		✓	
Group's Log Empl. Rate		✓		✓
Coefficients				
Median PS_{ct}		**−0.455****		**−0.580****
S.e.		*0.083*		*0.149*
Average PS_{ct}	**−0.302****		**−0.490****	
S.e.	*0.086*		*0.156*	
Activity rate	−0.016**	−0.015**	−0.016**	−0.015**
S.e.	*0.000*	*0.001*	*0.001*	*0.001*
Unemployment	−0.003	−0.002	−0.015*	−0.015**
S.e.	*0.001*	*0.001*	*0.003*	*0.003*
Fixed Effects				
Group	✓	✓	✓	✓
Time∗ISIC 22∗region	✓	✓	✓	✓
Adjusted R^2	0.976	0.979	0.985	0.987
RMSE	0.263	0.246	0.293	0.273
N. of observations	17.366M.	17.366M.	19.936M.	19.936M.

**: 1%; *: 5% significance levels. Groups are defined by the interaction of collective contracts, local labor markets, and two-digit sectors (*entire INPS sample*) or firms with the collective agreements that they apply (*INPS-AIDA sample*). All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available).

Table B.3
2SLS estimates of the employment elasticity to wages.

Sample:	*Entire INPS*			*INPS-AIDA*		
	(1)	(2)	(3)	(1)	(2)	(3)
Coefficient						
w_{gt}	**−0.806****	**−0.795****	**−0.829****	**−1.107****	**−0.916****	**−0.976****
S.e.	*0.188*	*0.195*	*0.186*	*0.283*	*0.307*	*0.323*
Controls						
Activity rate	✓	✓	✓	✓	✓	✓
Unemployment	✓	✓	✓	✓	✓	✓
Fixed Effects						
Group	✓	✓	✓	✓	✓	✓
Time∗ISIC 22∗region	✓			✓		
Time∗ISIC 38∗region		✓			✓	
Time∗ISIC 38∗province			✓			✓
First-stage F statistic	580	482	436	312	245	208
Centered R^2	0.974	0.975	0.977	0.981	0.983	0.986
RMSE	0.277	0.271	0.264	0.330	0.312	0.290
N. of observations	17.363M.	17.363M.	17.347M.	19.935M.	19.934M.	19.909M.

**: 1%; *: 5% significance levels. Groups are defined by the interaction of collective contracts, local labor markets, and two-digit sectors (*entire INPS sample*) or firms with the collective agreements that they apply (*INPS-AIDA sample*). All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available).

Table B.4
Descriptive Statistics on Selected Firms' Outcomes.

Variables	*Firms' averages over the years 2007–2015*		
	Mean	St.dev.	N. groups
Log contract's costs/revenues	−7.212	1.372	260,241
Log phys. capital/labor costs	4.326	1.874	259,019
Log revenues	14.358	1.625	260,292
Log value added p.w.	10.902	0.563	260,292

Statistics computed using one observation per group in the INPS-AIDA sample. Groups are defined by the interaction of firms and collective contracts. All variables are averaged over the period 2007–2015, considering only a strongly balanced sample.

Table B.5
Wage and employment effects of pay scales across quintiles of average firm-level outcomes.

Firms' Outcomes	Total Revenues	Value Added per Worker	Capital/ Labor Costs	Contract's Costs/ Revenues
Dependent variable	*Group's Avg. Log Wages*			
Coefficients:				
$PS_{ct} * q_{\hat{r}_g}(1)$	**0.438****	**0.381****	**0.464****	**0.540****
S.e.	*0.034*	*0.054*	*0.057*	*0.048*
$PS_{ct} * q_{\hat{r}_g}(2)$	**0.501****	**0.507****	**0.535****	**0.612****
S.e.	*0.029*	*0.043*	*0.037*	*0.047*
$PS_{ct} * q_{\hat{r}_g}(3)$	**0.484****	**0.440****	**0.583****	**0.554****
S.e.	*0.028*	*0.034*	*0.034*	*0.034*
$PS_{ct} * q_{\hat{r}_g}(4)$	**0.462****	**0.544****	**0.542****	**0.483****
S.e.	*0.028*	*0.034*	*0.031*	*0.032*
$PS_{ct} * q_{\hat{r}_g}(5)$	**0.547****	**0.652****	**0.482****	**0.494****
S.e.	*0.033*	*0.035*	*0.041*	*0.040*
Adjusted R^2	0.826	0.826	0.826	0.826
RMSE	0.164	0.164	0.164	0.164
N. of observations	19.9M.	19.9M.	19.8M.	19.9M.
Dependent variable	*Group's Log FTE Employment Rate*			
Coefficients:				
$PS_{ct} * q_{\hat{r}_g}(1)$	**−2.019****	**−1.955****	**−0.784****	**−0.023**
S.e.	*0.133*	*0.247*	*0.221*	*0.205*
$PS_{ct} * q_{\hat{r}_g}(2)$	**−1.335****	**−1.018****	**−0.287**	**−0.294**
S.e.	*0.130*	*0.169*	*0.162*	*0.189*
$PS_{ct} * q_{\hat{r}_g}(3)$	**−0.947****	**−0.615****	**−0.172**	**−0.462****
S.e.	*0.126*	*0.157*	*0.151*	*0.156*
$PS_{ct} * q_{\hat{r}_g}(4)$	**−0.411****	**−0.205**	**−0.467****	**−0.897****
S.e.	*0.133*	*0.157*	*0.151*	*0.229*
$PS_{ct} * q_{\hat{r}_g}(5)$	**−0.448****	**0.143**	**−1.233****	**−0.627****
S.e.	*0.168*	*0.228*	*0.258*	*0.162*
Adjusted R^2	0.985	0.985	0.985	0.985
RMSE	0.293	0.293	0.293	0.293
N. of observations	19.9M.	19.9M.	19.8M.	19.9M.
Controls				
Unemployment	✓	✓	✓	✓
Activity rate	✓	✓	✓	✓
Fixed Effects				
Group	✓	✓	✓	✓
Time∗ISIC 22∗region	✓	✓	✓	✓

∗∗: 1%; ∗: 5% significance levels. Estimates performed on specific subsamples derived from the entire INPS archives for each population segment. Groups are defined by the interaction of collective contracts and firms. All regressions are weighted by number of workers in each group-month cell and standard errors are computed clustering at the group level. The number of observations is computed omitting singletons (*i.e.*, fixed effects clusters for which only one observation is available). $q_{\hat{r}_g}(n)$ is an indicator for the nth quintile of the distance from the contract-specific outcome's average.

Table C.1
Collective agreements included in the entire INPS sample.

INPS contract code	Included from	Included until	% of total worker-month observations
001	2006m1	2016m12	0.80
002	2006m1	2016m12	0.40
003	2006m1	2016m12	1.34
005	2006m1	2016m12	0.15
006	2006m2	2007m4	0.00
007	2006m1	2016m12	0.05
010	2006m1	2016m12	0.18
011	2006m8	2016m10	0.01
012	2006m7	2016m12	0.06
013	2006m1	2016m12	2.08
014	2006m1	2016m12	0.31
015	2006m1	2016m12	0.11
017	2006m1	2010m8	0.01
018	2006m2	2016m12	0.32
019	2006m1	2016m12	0.17
020	2006m1	2016m11	0.11
021	2006m1	2016m12	1.07
023	2006m1	2016m12	0.15
025	2008m1	2012m2	0.00
026	2006m1	2016m12	0.46
027	2006m1	2016m12	0.12
028	2006m1	2016m12	0.52
029	2006m1	2016m12	0.08
030	2006m1	2008m12	0.03
031	2006m1	2008m12	0.46
032	2006m10	2016m12	0.08
033	2006m2	2016m12	0.18
034	2006m1	2016m12	0.05
035	2006m3	2016m11	1.34
037	2006m1	2016m12	0.17
038	2006m2	2016m11	0.01
039	2006m1	2016m12	0.01
042	2006m1	2016m12	24.26
043	2006m1	2016m12	0.97
044	2006m1	2016m11	0.01
045	2006m1	2012m11	0.27
047	2006m1	2016m12	0.12
048	2006m1	2016m12	0.04
049	2006m4	2016m12	0.03
050	2006m1	2011m11	0.00
051	2006m1	2016m11	1.79
053	2006m1	2016m12	0.19
054	2006m1	2016m12	0.00
055	2006m1	2016m12	0.00
057	2006m1	2016m12	0.04
058	2006m1	2013m6	0.14
059	2006m1	2010m11	0.05
062	2006m1	2012m12	0.02
063	2006m8	2016m12	0.12
064	2006m1	2010m12	0.01
065	2006m1	2016m12	0.00
067	2006m1	2012m12	0.01
068	2006m1	2016m12	3.83
069	2006m1	2016m12	0.97
070	2006m1	2016m12	0.21
071	2006m1	2016m12	2.06
072	2006m1	2016m12	0.02
075	2006m1	2016m12	0.06
078	2006m1	2013m8	0.09
079	2006m1	2016m12	0.02
081	2006m1	2016m12	0.03
084	2006m1	2016m12	0.39
085	2006m1	2009m11	0.03
086	2006m1	2016m11	0.00
088	2006m1	2016m12	1.63
089	2006m1	2016m12	0.32
090	2006m1	2016m5	0.43
091	2006m1	2016m12	0.20
092	2006m2	2016m10	0.30

(*continued on next page*)

Table C.1 (*continued*).

INPS contract code	Included from	Included until	% of total worker-month observations
093	2006m1	2016m12	1.45
094	2006m4	2016m10	0.01
095	2006m1	2016m12	0.32
096	2006m1	2016m12	0.15
097	2006m1	2016m12	0.27
098	2006m1	2016m12	0.06
099	2006m1	2016m12	0.15
100	2006m1	2016m11	0.68
101	2006m1	2016m12	0.80
102	2006m1	2016m8	0.05
110	2007m6	2016m12	0.01
111	2007m6	2016m12	0.03
112	2006m1	2016m12	0.03
113	2006m1	2016m12	12.95
115	2006m1	2016m12	4.29
116	2006m1	2016m12	5.30
117	2006m1	2016m11	0.02
118	2006m2	2016m12	0.61
119	2006m1	2013m3	1.19
120	2006m1	2016m12	1.61
121	2006m1	2016m12	0.08
122	2006m1	2016m12	0.00
123	2006m1	2016m12	0.13
124	2006m1	2016m12	0.11
125	2006m1	2016m12	0.08
126	2006m1	2016m12	0.06
127	2006m1	2016m12	0.53
128	2006m1	2016m12	0.16
129	2006m1	2016m12	0.06
131	2006m1	2016m12	0.08
134	2006m1	2016m12	0.09
135	2006m1	2016m12	0.13
136	2006m2	2016m12	0.32
137	2006m1	2016m12	0.04
138	2006m1	2016m12	0.00
140	2006m1	2009m8	0.00
141	2006m1	2008m4	0.00
142	2006m1	2007m8	0.00
143	2006m1	2016m10	0.30
144	2006m1	2016m12	0.39
145	2006m1	2016m12	0.40
146	2006m6	2006m7	0.00
148	2006m1	2016m12	0.03
151	2006m1	2016m12	2.57
152	2006m1	2016m12	2.50
153	2006m1	2016m12	0.32
154	2006m1	2016m12	0.00
156	2006m1	2009m8	0.01
158	2006m1	2009m12	0.02
159	2006m1	2016m12	1.39
160	2006m2	2016m12	0.92
161	2006m1	2016m12	0.06
162	2006m1	2016m12	0.44
167	2006m6	2016m12	5.51
168	2006m1	2016m12	0.33
172	2006m1	2016m12	0.01
175	2006m1	2016m12	0.50
176	2006m1	2016m12	0.13
178	2006m1	2013m2	0.03
180	2006m1	2016m12	0.18
182	2006m1	2016m12	0.16
184	2006m1	2016m12	0.01
189	2006m6	2016m9	0.01
191	2006m1	2016m12	0.09
192	2006m1	2016m12	0.00
193	2006m1	2016m12	0.08
194	2006m1	2016m12	0.01
196	2006m1	2016m12	0.06
198	2006m1	2016m12	0.01
201	2006m2	2013m2	0.46

(*continued on next page*)

Table C.1 (*continued*).

INPS contract code	Included from	Included until	% of total worker-month observations
204	2006m1	2016m12	0.14
206	2006m1	2016m12	0.00
207	2006m1	2008m12	0.01
208	2006m5	2016m12	0.02
209	2006m1	2016m12	1.29
211	2006m1	2016m10	0.01
212	2006m1	2016m12	0.04
214	2006m4	2016m11	0.03
218	2006m1	2016m12	0.02
219	2006m1	2006m8	0.00
222	2006m1	2009m1	0.00
224	2006m1	2016m12	0.01
229	2006m2	2016m12	0.11
231	2006m1	2016m12	0.01
271	2015m1	2016m12	0.00
272	2014m2	2016m12	0.00
290	2016m1	2016m12	0.00
291	2016m10	2016m12	0.00
300	2016m7	2016m12	0.01
304	2016m7	2016m12	0.00

Table C.2
Collective agreements included in the INPS-AIDA sample.

INPS contract code	Included from	Included until	% of total worker-month observations
001	2007m1	2015m12	0.80
002	2007m1	2015m12	0.36
003	2007m1	2015m12	0.29
005	2007m1	2015m12	0.23
006	2007m1	2007m4	0.00
007	2007m1	2015m12	0.08
010	2007m1	2015m12	0.13
011	2007m2	2015m11	0.01
012	2007m1	2015m12	0.10
013	2007m1	2015m12	3.20
014	2007m1	2015m12	0.26
015	2007m1	2015m12	0.13
017	2007m1	2010m8	0.00
018	2008m1	2015m12	0.07
019	2007m1	2015m12	0.14
020	2007m4	2015m12	0.23
021	2007m1	2015m12	0.08
023	2007m6	2015m11	0.16
025	2008m1	2012m2	0.00
026	2007m1	2015m12	0.65
027	2007m1	2015m12	0.12
028	2007m1	2015m12	0.90
029	2007m1	2015m12	0.12
030	2007m1	2008m12	0.02
031	2007m1	2008m12	0.38
032	2007m1	2015m11	0.14
033	2007m3	2014m12	0.33
034	2007m1	2015m12	0.02
035	2007m2	2015m11	2.29
037	2007m1	2015m12	0.25
038	2007m1	2015m11	0.01
039	2007m1	2015m12	0.01
042	2007m1	2015m12	26.35
043	2007m1	2015m12	1.22
044	2007m5	2015m11	0.02
045	2007m1	2012m11	0.01
047	2007m1	2015m12	0.15
048	2007m1	2015m12	0.06
049	2007m4	2015m10	0.00

(*continued on next page*)

Table C.2 (*continued*).

INPS contract code	Included from	Included until	% of total worker-month observations
050	2007m1	2011m11	0.00
051	2007m1	2015m9	0.11
053	2007m4	2015m9	0.02
054	2007m1	2015m12	0.01
055	2007m1	2015m12	0.00
057	2007m1	2015m12	0.05
058	2007m1	2013m6	0.21
059	2007m1	2010m11	0.00
062	2007m1	2012m12	0.03
063	2008m1	2015m12	0.19
064	2007m1	2010m12	0.01
065	2007m1	2015m12	0.00
067	2007m1	2012m12	0.00
068	2007m1	2015m12	3.15
069	2007m1	2015m12	0.57
070	2007m1	2015m12	0.18
071	2007m1	2015m12	0.36
072	2007m1	2015m12	0.02
075	2007m1	2015m12	0.06
078	2007m2	2013m8	0.02
079	2007m1	2015m12	0.02
081	2007m1	2015m12	0.05
084	2007m1	2015m12	0.70
085	2007m1	2009m11	0.03
086	2007m2	2015m12	0.00
088	2007m1	2015m12	2.75
089	2007m1	2015m12	0.48
090	2007m2	2015m7	0.59
091	2007m1	2015m12	0.21
092	2007m2	2015m12	0.39
093	2007m1	2015m12	1.54
094	2007m1	2015m11	0.01
095	2007m1	2015m12	0.36
096	2007m1	2015m12	0.14
097	2007m1	2015m12	0.30
098	2007m1	2015m12	0.06
099	2007m1	2015m12	0.04
100	2007m1	2015m11	0.86
101	2007m2	2015m12	0.20
102	2007m1	2015m12	0.07
110	2007m6	2015m12	0.02
111	2007m6	2015m12	0.03
112	2007m1	2015m12	0.03
113	2007m1	2015m12	19.53
115	2007m1	2015m12	5.65
116	2007m1	2015m12	1.74
117	2007m1	2015m12	0.02
118	2007m1	2015m11	0.82
119	2007m1	2013m3	1.55
120	2007m1	2015m12	1.45
121	2007m1	2015m12	0.02
122	2007m1	2015m12	0.01
123	2007m1	2015m12	0.18
124	2007m1	2015m12	0.04
125	2007m1	2015m12	0.09
126	2007m1	2015m12	0.02
127	2007m2	2015m12	0.13
128	2007m1	2015m12	0.26
129	2007m1	2015m12	0.04
131	2007m3	2015m12	0.07
134	2007m1	2015m12	0.06
135	2007m1	2015m9	0.23
136	2007m1	2015m12	0.11
137	2007m1	2015m12	0.03
138	2007m1	2015m12	0.00
140	2007m2	2009m8	0.00
141	2007m1	2008m4	0.00

(*continued on next page*)

Table C.2 (*continued*).

INPS contract code	Included from	Included until	% of total worker-month observations
142	2007m1	2007m8	0.00
143	2007m2	2015m12	0.20
144	2007m1	2015m12	0.08
145	2007m1	2015m12	0.04
148	2007m1	2015m12	0.03
151	2007m1	2015m12	2.15
152	2007m1	2015m12	0.51
153	2007m1	2015m12	0.07
154	2007m1	2015m12	0.00
156	2007m1	2009m8	0.01
158	2007m1	2009m12	0.02
159	2007m1	2015m12	1.83
160	2007m1	2015m12	1.24
161	2007m2	2015m12	0.08
162	2007m1	2015m12	0.41
167	2007m1	2015m12	3.81
168	2007m1	2015m12	0.49
172	2007m1	2015m12	0.01
175	2007m1	2015m12	0.10
176	2007m2	2015m12	0.01
178	2007m1	2013m2	0.01
180	2007m1	2015m12	0.06
182	2007m1	2015m12	0.07
184	2007m1	2015m12	0.01
189	2007m1	2015m12	0.01
191	2007m1	2015m11	0.09
192	2007m1	2015m12	0.00
193	2007m1	2015m12	0.03
194	2007m1	2015m12	0.01
196	2007m1	2015m12	0.06
198	2007m1	2015m12	0.00
201	2007m6	2013m2	0.89
204	2007m1	2015m12	0.07
206	2007m1	2015m12	0.00
207	2007m1	2008m12	0.00
208	2007m5	2015m10	0.04
209	2007m1	2015m12	2.20
211	2007m3	2015m12	0.00
212	2007m1	2015m12	0.02
214	2007m1	2015m12	0.05
218	2007m1	2015m12	0.02
222	2007m1	2009m1	0.00
224	2007m1	2015m12	0.01
229	2007m2	2015m12	0.04
231	2007m1	2010m9	0.00
271	2015m10	2015m10	0.00
272	2015m12	2015m12	0.00

samples. The INPS contract code refers to the official classification number of the contract provided by the National Institute for Social Security.[54] For each of these agreements, we have computed their relative size, measured as the proportion of total worker-month observations in the estimation sample that were covered by each contract.

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[54] The contracts' names associated with each of these codes is available at:https://www.inps.it/circolariZip/Circolare%20numero%20130%20del%207-9-2004_Allegato%20n%206.pdf.

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