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THE EMPLOYMENT EFFECTS OF COLLECTIVE BARGAINING

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The Employment Effects of Collective Bargaining*

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Abstract

This paper studies the wage and employment effects of Italian collective bargaining. For this purpose, it analyses monthly data derived from administrative archives on the population of private-sector employees, matched with extensive information on contractual pay levels settled in industry-wide agreements bargained by trade unions' and employers' representatives at the national level. The research design is based on a generalised differences-in-differences method, which exploits the numerous contrasts generated by the Italian wage setting rules and controls for space-specific sectoral unobserved time-varying disturbances in a fully non-parametric way. Results show that a growth in contractual wages produced sizeable increases in actual pay levels for all workers, determining at the same time strong and negative effects on employment. The resulting confidence interval of the implied own-price labour demand elasticity ranged between -0.4 and -1.2, and it was even slightly more negative among incorporated companies. Studying interactions of this parameter with firm-level outcomes –value added per worker, size, the labour share and capital intensity– we found associations broadly consistent with Hicks-Marshall laws and with traditional models of centralized wage bargaining. Further analyses carefully document the presence of dynamic employment adjustments to contractual wage levels and assess the overall robustness of the results.

Keywords: collective bargaining, labour demand, employment, industrial relations, minimum wage.

JEL codes: J01, J08, J21, J23, J38, J52

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1 Introduction

Wage setting institutions have often been regarded as important candidates in explaining differences across countries' economic performances (*e.g.* Nickell [1997]). Indeed, the provisions characterising collective or decentralized wage bargaining can potentially influence several economic variables. Important outcomes have been linked to the wage setting structure using either theoretical arguments or empirical evidences, 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]); wage rigidities (Agell and Lundborg [2003], Messina et al. [2010]); monetary policy effects (*e.g.* Faia and Pezone [2019]); 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]); international trade effects (*e.g.* Helpman and Itskhoki [2010]); product market competition effects (Griffith et al. [2007]).

Despite this interest, abundant micro-based evidences on the effects of similar institutions are available only for a limited number of policies (minimum wages and trade union presence). Other forms of national wage setting regimes have been most often studied using only either cross-country comparisons or highly aggregated data, in particular when the outcome of interest was employment. This tendency is quite problematic, given that, as shown *e.g.* by Boeri [2012], there are relevant differences between government-legislated wage floors and those that are set by collective bargaining. However, it is also not surprising considering that wage setting, when not completely decentralized at the firm level –which is the case for most western countries (see *e.g.* Flanagan [1999] and OECD [2017])– typically works through complex implementation mechanisms that may differ across industries and even within them. Thus, building a feasible research design for the purpose of evaluating complex wage setting systems often becomes a challenging task.

In this paper we study the employment effects of the Italian sectoral wage bargaining system. For this purpose, we have analysed high-frequency, comprehensive and updated information on employment and wages derived from restricted-access administrative data on social security contributions covering the population of private-sector employees, matched with precise information on the economic content of around 160 sector-wide agreements bargained by trade unions' and employers' representatives.[1] Through this rich amount of information, we have been able to conduct an in-depth within-country evaluation of a complex collective bargaining system characterized by an intermediate degree of centralization.[2] In this environment, wage-setting institutions impose occupation- and industry-specific minimum pay levels, which are bargained at the sectoral nation-wide level and apply to all private-sector employees. Exploiting the provisions of this system, we have identified the own-price labour demand elasticity for the whole economy, as well as across demographic and industry groups.[3] Furthermore, we have uncovered how heterogeneous responses to this policy interacted with firm-level outcomes, in particular size, value added per worker, labour cost shares and capital intensity, characterizing differences in employment elasticities to the wage observed within collective agreements along these dimensions.

Several institutional features have allowed us to build a solid research design. First, collective bargaining provisions regarding wages apply to all private sector employees, irrespective of a worker's union membership.[4] Thus, we have analysed a population where coverage is virtually full and mandatory, avoiding complications related to self-selection of firms into more or less centralized bargaining levels, which would arise in systems, such as the German one, where firm-level exemption clauses are allowed (see *e.g.* Baumann and Brändle [2017]). Second, many contracts usually coexist within an industry, and their renewals dates are not coordinated. This feature has allowed us to implement a solid identification strategy, as we

[1] The social security contribution data are property of the Italian Social Security Institute (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 eleven-years period. To access the data for replication purposes researchers should contact INPS' central research unit (`dcstudiricerche@inps.it`).

[2] See Calmfors and Driffill [1988] for a characterization of bargaining systems according to their degree of centralization.

[3] Given that we have based our analysis on administrative data, the private-sector labour demand elasticity was estimated considering only formal employment relationships, ignoring *e.g.* workers hired off the books. That is to say, the employment losses to the wage growth measured in this study could have been partly offset by a growth in informal or non-standard work relationships.

[4] In Italy, collective bargaining provisions regarding wages are not applied among self-employed, while they differ between the private and public sectors. However, our analysis has focused on the population of private-sector employees, the relevant group to which such provisions are always mandatory.

were able to control for a richer set of non-parametric industry- and geographic-specific time effects than that typically feasible in the evaluation of once-and-for-all policy interventions, such as government-legislated minimum wages. Finally, collectively bargained pay levels are occupation-specific, so that they tend to be binding for all skill levels and qualifications. On this respect, they are considered by the Italian legislation not only as a *wage floor*, but also as a *fixed pay component*, unless a worker and his/her employer agree otherwise. That is to say, an increase in such pay levels often shifts up the wage of all workers involved, typically also those that are already above the new minimum.[5] Thus, the policy interventions analysed here did not affect only employees with earnings close to the contractual pay floors, and this feature has allowed us to characterize firms' adjustment path to what can be considered a general shock in the cost of labour.

Our study covered an eleven-years period, from January 2006 to December 2016, and it was based on the construction of monthly panels of employment and pay levels. We have measured these outcomes within groups defined by interactions between collective contracts and either firms, or detailed geographical areas and economic activities. We have applied a classical generalised differences-in-differences method that exploits heterogeneities in minimum pay levels across time and wage-setting agreements, testing whether and how much collective bargaining affected wages and employment levels. In our setting, the identifying variation was given by comparing the outcomes of interest between units whose contractual wages had changed, with respect to units operating in the same sector and geographic location, but under a different collective agreement.[6]

Results show that the growth of contractual wages had strong effects on pay levels at the average level –with an elasticity always close to 0.5– and substantial negative employment effects, with an overall elasticity to full time equivalent formal employment rates of around -0.36 in the baseline model. This last parameter was found to be -0.59 when considering a balanced

[5]This institutional characteristic makes the use of bunching estimators (*e.g.* Cengiz et al. [2019]) a questionable choice in the present context.

[6]Firms are not free to choose which is the most convenient contract to apply, since coverage is defined by collective agreements themselves through a rich and detailed description of the activities and tasks regulated by their provisions. Moreover, the coverage of collective agreements does not map to a standard classification of industries. For example, the same activity is sometimes covered by two distinct contracts depending on the size of firms, while in other circumstances even co-workers could be covered by different contracts depending on the content of their job.

panel of incorporated businesses for which balance-sheet information was available (and value added was positive) in all years between 2007 and 2015. The implied labour demand elasticity was of -0.81 for the whole private sector (-1.11 among incorporated companies), with a 95% confidence interval between -0.44 and -1.17 (-0.55 and -1.66). These estimates lie in the lower (most negative) bound of the own-price elasticities typically reported in the minimum wage literature (see *e.g.* a recent survey of the results by Harasztosi and Lindner [2019]). This implies that the overall labour demand is more downward sloping than what could be inferred using as identifying variation wage shocks that affect only marginal workers at the bottom of the earnings distribution. Moreover, our evidence also suggests that wage setting policies specifically targeted to low-pay jobs tend to produce smaller disemployment effects than more pervasive institutions such as collective bargaining.

The above results are completely novel for Italy and they provide a contribution to the international literature on the impact of collective bargaining. They show that this institution has a salient role in shaping wage dynamics, which is consistent with existing evidences for other countries with similar systems of industrial relations (see *e.g.* Cardoso and Portugal [2005] and Dahl et al. [2013]). Our results also inform the relatively less developed literature that aims at providing nation-specific micro-based evidences on the employment effects of collective bargaining.[7] Some studies have focused on more specific features of this wage setting institution when evaluating employment outcomes, in particular its tendency to produce nominal wage rigidities (*e.g.* Card [1990]) or real ones (*e.g.* Díez-Catalán and Villanueva [2015]). In our context, the role of collective agreements was evaluated more directly, and both of the above mechanisms were potentially affecting our estimates, as long as the dynamics of contractual wages differed from those of prices and the business cycle at the local and sectoral level. Thus, our results also provide indirect evidences on the importance of wage rigidities, which calls for further research on their relationship with alternative collective

[7]This literature includes Dolado et al. [1997], who attributed large employment losses to collective bargaining using discontinuities in wages around the minima found in a small cross-section of Spanish workers; Magruder [2012], Martins [2014] and Hijzen and Martins [2016] who documented, for South Africa and Portugal, negative employment effects associated to 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; Guimaraes et al. [2017], who found strong disemployment effects associated to the wage bill growth induced by collective bargaining in Portugal.

bargaining regimes (*e.g.* Devicienti et al. [2007], Boeri et al. [2019]) and on the sensitivity of contractual wages to macro-economic and institutional factors (*e.g.* Christofides and Oswald [1992], Abowd and Lemieux [1993], Avouyi-Dovi et al. [2013], Fougère et al. [2018]).

We have taken numerous steps in order to further characterize our findings, testing several hypotheses and the robustness of the main results. We have estimated dynamic employment adjustments to contractual wages set by collective bargaining, studying the relevance of anticipatory and long-run elasticities. Following *e.g.* Dube et al. [2010], Meer and West [2016] and Cengiz et al. [2019], we have tested the relevance of long-run anticipatory effects to probe the robustness of our identification, finding no significant pre-existing trends far away from the dates of contractual wages implementation. However, we found significant policy effects (of the same sign of post-policy implementation adjustments) starting from around five months before contractual wages had changed, which we interpret as anticipatory announcement effects. In general, policy effects were significant for at least two years around the contractual wages' implementation. This result is consistent with the presence of rigidities in short-run employment responses to increased labour costs (see *e.g.* Sorkin [2015]), but the fact that negative employment effects were found also in a balanced panel of companies (*i.e.* excluding those that shut-down and new-entrants) is an evidence against more extreme versions of this hypothesis, according to which adjustments to wage floors occur only through the slow substitution of old labour-intensive firms with new capital-intensive ones (see *e.g.* Aaronson et al. [2018]).

We have looked at heterogeneities in the employment effects of collective bargaining across economic activities and demographic groups, finding differences in the results depending on the degree of employment protection legislation available to workers. In particular, employment levels among open-ended contracts were not significantly affected by changes in minimum pay levels, while the labour demand elasticity was strongly negative among fixed-term employees. Moreover, prime-aged and young individuals were the two groups suffering most of the employment losses, while no significant employment effects related to wage setting policies were detected among older workers. With respect to the main economic activities, we found significant and negative employment effects related to collective bargaining in the manufacturing, construction, IT and communication, finance, human care and social work sectors, while we did not find significant elasticities in the trade, transportation, accommodation and food service, professional and technical activities sectors. Not all of these associations were fully consistent with a stylized positive relationship between tradeability and the labour demand elasticity (*e.g.* Harasztosi and Lindner [2019]). For example, negative employment adjustments to contractual wages were strong also in a domestic market relatively insulated from international competition, such as the construction sector.[8]

We have also tested more nuanced hypotheses on the relationship between wage setting, the degree of employment adjustments to increased labour costs, and firm-level outcomes. Companies that had the lowest levels of value-added per worker, compared to the average within the contract, were found to be more employment-responsive to statutory compensation growth. This result is in line with traditional models of collective bargaining (*e.g.* Moene and Wallerstein [1997]), in which the negative employment effects of having a centralized trade union that bargains over wages are concentrated mostly among least-efficient firms, while best performing ones can benefit from pay moderation.[9] Consistently with Hicks-Marshall predictions (see *e.g.* Hamermesh [1993]), we found stronger employment responses to higher contractual wages among firms where the share of contract-specific labour costs in total revenues was higher. Moreover, demand elasticities were more negative among firms that, during a nine-years period, increased their capital/worker ratio more than the collective agreement average, which is also consistent with standard theory predicting that companies with better opportunities of substituting capital to labour tend to implement larger reductions in the workforce when facing higher wages.[10] Finally, we did not find significant associations

[8]On this respect, the shocks in wages analysed here were not nation- or industry-specific, but rather contract-specific. Thus other factors than international competition, such as the coordination among collective agreements applied within a sector, or the incidence of competition from self-employed in a given product market, were likely to be relevant as well in our context.

[9]A similar version of this hypothesis was formalized also in Agell and Lommerud [1993] and it has been used as an argument in support of more centralized wage setting policies compressing wage dispersion, since such systems would direct more resources toward most efficient companies. However, this argument was developed with reference to the experience of Scandinavian countries, which have been characterized by low unemployment throughout the last decades of the past century. Two interesting accounts of this debate are provided *e.g.* by Agell [1999] and Hibbs and Locking [2000].

[10]A similar hypothesis has been considered by Sorkin [2015] in a dynamic context where capital worker ratios are relatively fixed once that equipment is installed. On the other hand, our evidence could be consistent also with a tendency toward the creation of excess capacity among more labour demand elastic firms if capital adjusts more slowly than employment.

between higher or lower labour demand elasticities and faster-than-average growth in value added per worker, which hints that efficiency-enhancing adjustments to higher contractual wages were not a main driver of the underlying heterogeneity across firms.

Overall, the analysis of firm-level outcomes provided indirect evidences that high value-added per worker firms may benefit from rents in a centralized bargaining system characterized by wage moderation, as they were able to adjust for the growth in contractual wages on other margins than employment.[11] Instead, companies most responsive, in terms of employment, to the growth in statutory compensations did not appear to have a faster growth in value added per worker.[12] This finding is not completely inconsistent with other studies on the relationship between productivity and minimum wages. For example, Riley and Bondibene [2017] show that the potential efficiency-enhancing effects of higher pay floors are not mediated by cuts in employment and capital-labour substitution, but rather by better organizational practices. Yet, firms that adjusted less on the employment margin did not experience a faster-than-average growth in value added per worker either. Partly due to limitations in the data we were not able to provide conclusive evidences on other adjustment margins exploited by firms. However, the above results suggest that –as *e.g.* in Draca et al. [2011] and Giroud and Mueller [2017]– profit-reductions and labour hoarding, rather than efficiency growth, were likely to be an important channel driving the underlying heterogeneity in labour demand elasticities that we have documented.

The paper is structured as follows. Section 2 provides a short institutional framework, Section 3 presents the data, Section 4 describes the identification strategy, Section 5 presents estimates of the wage and employment effects of collective bargaining and of the related labour demand elasticity, Section 6 contains the analysis of interactions between employment responses to contractual wages and firm-level outcomes, Section 7 presents robustness tests and results from dynamic specifications of the model, Section 8 contains the concluding

[11]The adjustment margins other than employment typically discussed in the literature are profits (*e.g.* Draca et al. [2011]), productivity (*e.g.* Riley and Bondibene [2017], Mayneris et al. [2018]) or product market prices (*e.g.* Aaronson and French [2007], MaCurdy [2015]).

[12]The hypothesis that employers could be more likely to make investments that increase workers' productivity while reducing employment in the presence of binding wage floors is discussed *e.g.* by Acemoglu [2003], although using a framework that was designed to characterize differences across countries, rather than firms' heterogeneity.

remarks.

2 Institutional Context

In Italy there are hundreds of national sector-wide collective contracts negotiated by trade unions and employers' associations, which are typically renewed every two years at dates that are not coordinated across different agreements.[13] The number of collective agreements within an industry varies depending mostly on historical and organizational reasons, so that, in general, more than one collective contract can coexist within a sector and multi-sector contracts are also common. 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. These compensation floors are different across *job titles*, which are usually between five and ten occupations defined by each collective agreement on the basis of the tasks performed by workers and sometimes seniority levels.

Contractual wages are binding for all private-sector dependent workers (self-employed are excluded) regardless of their trade unions' membership. Moreover, the application of collective bargaining provisions follows peculiar rules that are worth noticing in the present context. In particular, a growth in a minimum pay level is typically added to the base wage of all workers employed in the relevant job title, also those who already earn above the minimum, and this general rule can be sidestepped only in the presence of a specific agreement between a worker and his/her employer.[14] Moreover, employees can not be downgraded to less remunerative job titles, as they can only move up in the firms' hierarchy. Thus, the amount of rigidity imposed by this system is substantial, as its provisions tend to be binding for all employers.

There are two main channels to enforce minimum contractual pay levels. First, the National Social Security Institute is in charge of sending officers to firms, which 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

[13]The 2017 classification of the National Social Security Institute includes around 300 collective agreements. However, there are also several other contracts (typically those with an extremely small coverage and often a dubious legal basis for their applicability) that are not included in this classification.

[14]This agreement is called *superminimo assorbibile* in Italian.

a judge is asked to check 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 applying the correct contractual wage level, but they also incur in the potential loss of several fiscal benefits and incentives, as these tax exemptions typically include firms' adherence to collective bargaining standards as an eligibility rule.

For what concerns wage setting, collective bargaining has not undergone major reforms in the recent years. There have been a few legislative interventions and agreements between the main actors of the industrial relations system, mostly aimed at broadening the subjects on which firm-level exemption clauses from industry-wide provisions can be introduced, but none of these reforms has involved minimum compensation levels, which are still settled at the national, sector-wide level and remain binding for all private-sector employees.[15]

Several recent works have shown that the rules set by collective bargaining tend to have a strong influence on wages. In particular, Devicienti et al. [2019] show that Italian wage inequality has been largely channelled in the tight tracks set by this institution, as wage differences have always grown between contractual pay levels, while they have been persistently constant within such job titles. Similarly, Belloc et al. [2018] and Boeri et al. [2019] show that geographical differences in pay are quite small among private sector employees and they both attribute this tendency to the presence of contractual wages that are uniformly set at the national level.

Garnero [2018] studies non-compliance rates of Italian wages to collective bargaining standards, finding mixed results. The share of workers paid below the minimum was found to be as high as 7% in a labour force survey where informal work arrangements were potentially included and wages were self-reported, while the same rate dropped to around 2.5% using a sample of administrative records. This issue is studied also by Adamopoulou and Villanueva [2018], who found negligible levels of non-compliance to contractual wages in the Italian metal-manufacturing sector. This last study documents that Italian pay levels in-

[15] Erickson and Ichino [1995] provide a detailed description of Italian collective bargaining in the mid-90s, which still represents quite well how this institution works today, at least for what concerns wage setting. More recent institutional frameworks on Italian collective bargaining are provided by Dell' Aringa and Pagani [2007] and Devicienti et al. [2019].

creased across the entire earning distribution in response to the growth of negotiated wages, with no evidences of higher "bunching" around higher pay floors, a finding consistent with the tendency to consider contractual wages as a fixed component of pay.[16]

3 Data

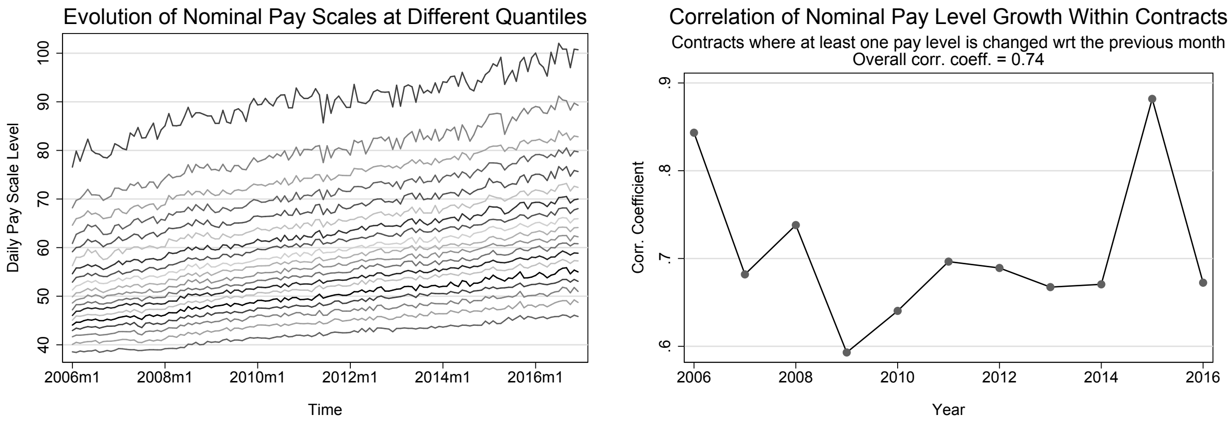
This paper is based on three main sources of information. First, we rely on the population of private-sector employees' social security records collected by the Italian Social Security Institute (INPS), which cover around four decades up to 2017. These data have a monthly frequency (but only since 2005) and contain information on wages, days worked and other individual characteristics. They are mandatorily filled by employers, so that each employee is always matched to his/her respective firm, but they do not cover self-employed and the public sector. Employers must also indicate the collective agreement to which each of their workers belong, indicating one of the around 284 contract codes provided by INPS.

The second data source that we have used is a database on contractual wages stipulated by collective agreements, which we have collected using the pay scales' tables attached to such contracts. In particular, for each job title within a sector-wide agreement we have recovered the relevant pay level in each month between January 2006 and December 2016. In general, we have been able to match 159 contracts to the INPS data, even if for some of these agreements we did not have information on pay scales covering all the years between 2006 and 2016. The contracts considered in the analysis tend to be the larger ones, as we were able to match information on contractual wages for around 78% of all person-month observations in the INPS archives between 2006 and 2016 (roughly 1.26 billions out of 1.62 billions of records). The full list of contracts considered in the samples of analysis is provided by Tables D1 and D2 in the Appendix.

Finally, we have conducted some of the analyses on a subsample of around 200,000 incorporated companies with at least one employee registered in the INPS archives. For these firms, we were able to match balance-sheet information on value added, revenues and physi-

[16]As mentioned, this is the main reason why using bunching estimators to detect employment losses (as suggested *e.g.* by Cengiz et al. [2019]) does not seem an appropriate choice in the present context.

Figure 1: **Evolution of Nominal Contractual Wages for Matched Contracts and Correlation in Their Growth Within Collective Agreements**



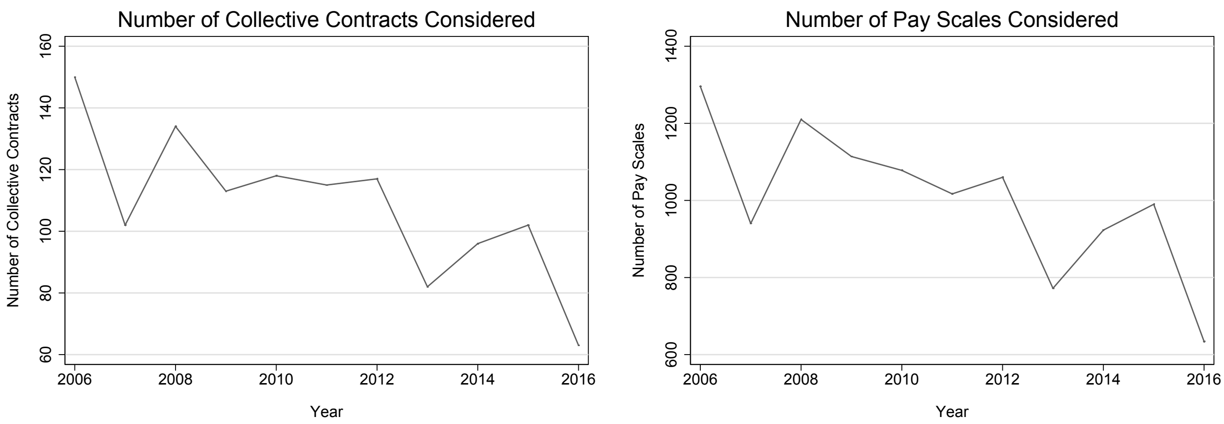
cal capital derived from AIDA-Bureau van Dijck data and covering the period between 2007 and 2015. To avoid potential problems related to the representativeness of this sample and selection across years, we have considered only a balanced panel of businesses for which a positive level of revenues and value added was observable in all years between 2007 and 2015, ignoring firms that shut down production, or new companies that had been created during this period of time.[17]

3.1 Matching Workers to Contractual Wages and Treatment Definition

As mentioned above, INPS archives indicate the collective contract under which any employee is hired. A collective agreement usually sets more than one contractual wage, as it typically defines a series of job titles for which specific pay levels apply. The left panel of Figure 1 shows the evolution of (nominal) contractual wages at twenty quantiles of their yearly distribution, considering only contracts that could be matched with INPS data. As can be noticed, such pay levels have been growing at a fairly steady rate throughout the period analysed here, following similar growth rates at all percentiles of their distribution. Unfortunately, only sector-wide agreements, and not job titles, could be matched determin-

[17] 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 has the potential of creating problems of sample selection across years, even if during the period considered in this analysis the sample size was relatively stable.

Figure 2: **Number of Collective Agreements and of Job Titles Considered in the INPS Data by Year**



istically to workers.[18] Thus, we have not been able to measure precisely this policy, using the actual contractual wage of each worker as a treatment in our analyses. Instead, we have adopted a fuzzy treatment definition, using the average and median pay level within each collective agreement as two policy variables. This choice does not represent a major weakness once that we consider how contractual wages within a collective agreement have evolved during the period under study. The right panel of Figure 1 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 months during which at least one of the nominal pay levels within a contract had changed. As can be noticed, the overall correlation coefficient was 0.74 and it was close to or above 0.6 in all of the years considered in the analysis. Thus, the growth in the median or average pay scale represent two good proxies for the evolution of other contractual wages within the same collective agreement.

In order to further limit inconsistencies across years, we have excluded from the analyses all renewals where the total number of pay levels defined by the collective contract was different than the one observed at the subsequent renewal. This choice has allowed us to compute medians and averages of contractual wages on a consistent number of pay levels across years, avoiding complications related to the introduction or removal of additional pay scales within

[18]This was not possible because pay levels within contracts are not reported in the INPS data after 2004.

a contract. The left panel of Figure 2 provides the number of collective agreements that have been matched to INPS data and considered in our analyses.[19] The right panel provides the same statistic computed on job titles. As can be noticed, the average number of pay levels within an agreement was between 5 and 10 in all years.[20]

3.2 Grouping of the Data, Outcomes' Definition and Descriptive Statistics

In order 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 formed by the combination of two-digit International Standard of Industries' Classification (Isic rev. 4) sectors, 611 ISTAT local labour markets (LLM) and 159 collective contracts for which information on pay scales was available.[21] Within these groups, we have constructed measures of employment (number of workers and number of full-time equivalent workers) and wage levels (average daily wages) in each month between January 2006 and December 2016. We have also replicated the analyses on the matched INPS-AIDA sample, a balanced panel of incorporated businesses covering the years 2007-2015, for which balance-sheet variables were available and value added was positive. In this case, we have grouped the data using combinations of firms and the collective contracts applied within them.

Table 1 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 that we have considered in the empirical analyses. 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 labour market) over the yearly number of active individuals in the local labour market.

[19]Figure 2 refers to the number of contracts matched to the whole INPS data. In some of the analyses we have considered a smaller number of contracts matched to a subsample of incorporated businesses.

[20]See Tables D1 and D2 (in the Appendix) for more details on the collective agreements included in the samples of analysis, together with the exact periods for which information on their pay levels was retrieved.

[21]ISTAT local labour markets are defined by the Italian National Statistical Office using census data on commuting behaviour 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 90 industries defined on the basis of their product characteristics.

Table 1: **Weighted Descriptive Statistics on the Grouped Samples**

	Whole INPS Sample		*INPS-AIDA Sample*	
Variables	**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	
Weighted Group-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 whole INPS sample groups are defined by the interaction of two-digit sectors, local labour markets and contracts. In the INPS-AIDA sample groups are defined by the interaction of firms and collective contracts. All statistics are weighted by the number of workers in the group-month cell.

The third and fourth rows summarize the policy treatment variables expressed in nominal terms, while the fourth row shows that the monthly growth in collective agreements' median nominal pay scales was of 0.2% on average in both samples. The size of groups in the INPS-AIDA sample was consistently smaller, due to the fact that in this case the data was grouped using finer firm-contract cells, rather than sector-LLM-contract interactions. In general, incorporated businesses were more often located in northern regions of Italy, where unemployment rates were lower and activity rates were 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 balance-sheet data for financial institutions.

4 Identification Strategy

This study aims at uncovering the effects of contractual wages set by collective bargaining on employment levels. From a theoretical perspective, we expect that changes in these provisions should affect firms' hiring decisions due to their influence on pay levels, and for this reason we have considered as a further outcome the impact of this policy on wages, which is a standard approach followed also in the extensive minimum wage literature. In an ideal setting where all the relevant parameters are correctly identified, changes in statutory compensations can be considered as instruments for wages, which allow to recover reduced-form local estimates of labour demand elasticities. In our context, this interpretation seems particularly appropriate, given that a growth in pay scales provides a close approximation to a general shock in prices, as it typically affects workers at all levels of the income distribution. Thus, estimates of labour demand elasticities recovered using this policy are arguably closer to those implied by a classical model of labour demand with homogeneous workers (*e.g.* Brown et al. [1982], Hamermesh [1993]).

Our identification strategy is based on the estimation of a generalised differences-in-differences model with continuous treatment, which is also referred to as a *fixed effect approach* (*e.g.* Neumark and Wascher [1992]) and *time-series* or *canonical model* (*e.g.* Card and Krueger [1995]) in the traditional minimum wage literature.[22] In our context, we have specified this model as follows. Let t index time periods (months), c index industry-wide collective contracts, m index local labour markets, l index less detailed geographical units and s index sectors. Moreover, denote groups defined by the interaction of collective agreements, local labour markets and two-digit sectors with g. When the model is estimated on the incorporated businesses' sample, 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

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

[22] Similar versions of this model are also estimated and extensively discussed in the more recent and voluminous minimum wage literature. See, *e.g.*, Dube et al. [2010], Neumark et al. [2014], Dube et al. [2016], Meer and West [2016] and Allegretto et al. [2017].

where PS_{ct} is either the median or average log pay scale of collective contract c at time t, x_{mt} is a set of time-varying local labour market characteristics (activity and unemployment rates), which control for shifts in the labour 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. Notice that the relevant policy variable of this model is the nominal level of contractual wages, since variations in the real level of pay scales are 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 actual pay levels to the contractual wages set by collective bargaining. In a second specification of the model, we have defined y_{gt} as the log full-time equivalent number of workers in group g and month t divided by the workforce of the local labour market m in the respective year.[23] With this specification, β gives the percentage growth in the employment rate for a one percent growth in contractual wages. As a robustness test, we have also estimated equation (1) defining y_{gt} as the number of workers in group g divided by the workforce of the local labour market. In this case, only employment adjustments on the extensive margin can influence the outcome, but the definition of the dependent variable is less vulnerable to potential misreporting of actual days worked.

We stress that when considering employment outcomes, we have looked only at firms' reliance on formal employment relationships. Given that INPS data are an administrative source of information, they do not cover workers hired off the books. Moreover, a reduction in the number of private-sector dependent workers does not necessarily imply lower employment rates in the economy, since civil servants and self-employed are not taken into account. Finally, firms could react to policy changes by outsourcing some of their activities to self-employed, but this possibility is often limited by the Italian employment legislation. Moreover, this process would still have negative externalities, given that higher reliance on non-standard work arrangements typically entails lower compensations, social security contributions and employment protection levels.

In order to recover a measure of the reduced-form labour demand elasticity to wages, as

[23] Dividing employment measures by the size of the workforce allows to better control for shifts in the labour supply.

well as a confidence interval for this parameter, we have also estimated directly the following employment equation

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

where emp_{gt} is the (formal) employment rate measured in full-time 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 equation (1). We have estimated the above model by Two Stages Least Squares (2SLS), using median contractual log pay scales (PS_{ct}) as an instrument for w_{gt}. As can be noticed, the labour demand elasticity (η) is a function of the parameters given by equation (1), *i.e.* it is the ratio of $\beta(y_{gt} = emp_{gt})$ to $\beta(y_{gt} = w_{gt})$.

For all regression models, we have dealt with heteroskedasticity by clustering standard errors at the group level and by weighting all the regressions by the number of workers forming each group g. This latter adjustment has also the advantage of providing parameter estimates that are closer to the population average. Instead, the clustering choice allows to correct for any correlation pattern of the outcome within groups across time. Given the large number of available groups (generally more than 250 thousands or even 300 thousands, depending on the sample), this choice can be considered appropriate in the present context (Bertrand et al. [2004]).

4.1 Threats to Identification and Solutions Adopted

The main threat to a correct identification of the parameters of our model is represented by the presence of unobserved factors, which could correlate with changes in collective bargaining pay scales while also influencing the outcomes of interest. In particular, Dube et al. [2010] argue that failing to control adequately for heterogeneity in employment growth across space has led to biased results in traditional panel studies of the US minimum wage. Moreover, it is reasonable to assume that bargaining parties consider business cycle fluctuations when setting pay scales and that they may posses information on future labour demand. On this respect, Avouyi-Dovi et al. [2013] show that negotiated industry-level wage agreements are negatively correlated with the unemployment rate in France.[24] In order to address these

[24] A similar finding was documented for Canada also by Christofides and Oswald [1992]. In a related study, Fougère et al. [2018] find that French wage agreements set a wage growth similar to that observed

concerns, we have relied on the granularity of the available data and we have exploited institutional features that make our application an almost ideal setting for the estimation of a generalized differences-in-differences model.

The main feature of Italian collective bargaining that has allowed us to build a solid research design is its intermediate degree of centralization. In particular, in Italy it is quite common to have more than one contract applied within a sector, while, conversely, some large contracts cover heterogeneous activities that can take place in more than one industry.[25] For this reason, and given also the relatively deterministic and uncoordinated timing of contract renewals across and within sectors, we were able to include non-parametric controls for aggregate trends in the outcomes that would be infeasible when studying more centralized wage policies, which typically have a much more limited variability.

In our context, the policy effect was identified by comparing outcomes between groups whose contractual wages had changed, with respect to groups within the same geographical area and sector who were not subject to this shock. In particular, we have controlled for the following confounders: constant effects for each two-digit sector, local labour market and collective agreement cell (firm and collective agreement cell in the incorporated businesses' sample); specific monthly time fixed effects for each interaction between geographical areas (20 regions or 107 provinces) and industries (ISIC 21 or ISIC 38 classifications); time-varying regressors controlling for business cycle fluctuations and labour supply effects in the local labour market (yearly activity and unemployment rates). In this setting, concerns related to the presence of endogenous unobservable trends in wages or employment across space are not particularly relevant. Moreover, concerns related to the correlation between contractual wages and business cycle fluctuations are addressed by the inclusion of detailed industry space-specific unobservable effects at the monthly level.

A different estimation strategy to deal with this latter problem was proposed, in a similar

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in other contracts and in the government-legislated minimum wage, while business cycle fluctuations have a significant, but smaller influence.

[25]For example, in many sectors there are at least three different collective contracts, depending on the size and sometimes even on the organizational structure of the firm. Moreover, it is quite common to find firms with some workers employed under the collective agreement of the trade sector, even in cases where the main activity of the business is not related to trade. Similarly, managers compensations are often regulated by separate collective contracts that typically cover several industries.

context, by Card [1990], who instrumented contractual wages at their end date using unexpected changes in real wages. However, through this approach only nominal wage rigidities can be studied, since other mechanisms through which contractual wages affect employment (*e.g.* real wage rigidities) would be filtered out by the instrument. Moreover, that study focused on relatively small Canadian agreements in the union sector and it analysed highly aggregated information on employment, while the contractual wages analysed here were uniformly set at the nation-wide level and the available data consisted of the population of private-sector employees. Thus, the amount of unobservable information on future labour demand embedded within collective agreements was obviously much coarser and the possibilities to control for unobserved disturbances much larger in our context.

Another identification problem is related to the timing of firms' adjustments to the policy. Equation (1) is static, as it includes only the contemporaneous level of contractual wages in a given month. In Section 7 we present and discuss dynamic specifications of the same model, in which leading and lagged values of PS_{gt} are also included. Here, we only stress that if the effects of contractual wages span over more than one period (as argued, from a theoretical perspective, by Sorkin [2015]), then, due to omitted variable bias, in the static model the coefficient β will be biased toward a weighted function of the cumulative effect of pay scales on the outcome, with weights decreasing in magnitude as the correlation between relevant lags or leads and current levels of PS_{ct} (conditional on all other independent variables of the model) decreases.[26]

Given the above considerations, assuming that anticipatory and long-run adjustments tend to be of the same sign than contemporaneous ones (or at least not larger and totally different from the contemporaneous effect), we can still consider estimates of the static coefficient $\hat{\beta}$ as interesting and relevant, given that in general they will tend be biased toward the cumulative effect of the policy. A mechanism that would induce differences in sign between short- and long-run elasticities could be a shock in contractual wages that is completely different from

[26]Given that contractual wages are a highly persistent autocorrelated process -as can be noticed from Table 1, the monthly growth rate in nominal pay scales is of around 0.2% with a standard deviation of only 0.7%- lags or leads that are relevant are also positively correlated with PS_{ct} and affect estimates of β according to the standard omitted variable bias formula. Discussions related to this point can be found in Neumark and Wascher [1992], Baker et al. [1999] and, more recently, by Meer and West [2016].

employers' expectations.[27] However, in our context the duration of collective agreements is known to firms, so that they can foresee the dates at which wages will be negotiated, while the growth rate followed by pay scales across time has been quite stable during the period under study (see Figure 1).

Finally, it should be noted that, in the Italian institutional context, an employer does not have the option to choose the most convenient agreement to apply. As mentioned, the coverage of collective agreements is determined at the national level by bargaining parties through a rich set of dispositions describing the activities and job tasks regulated by each contract. This feature emerges also in our data, by analysing the transitions of workers across contracts. Only around 1.5% of workers continuously employed for two years in the same firm switched contract, and this percentage was not higher during periods in which contractual wages had changed.

A related concern is given by the fact that there could be sizeable labour supply shifts toward firms operating under contracts that did not change their pay levels whenever a given agreement rises its wages. While this possibility can not be ruled out, its relevance should not be overstated. An analysis of the year-to-year transitions of workers across contracts showed that this probability was always around 5%, irrespective of whether there had been changes in pay levels in the collective agreement of origin. Notice also that all workers in our data were subject to a collective contract with downward rigid wages, a feature that, in principle, should limit the extent of the potential employment effects related to positive supply shocks. On this respect, the inclusion in the regression equation of a measure of labour market tightness at the local level (*i.e.* the local unemployment rate) appeared to have no detectable influence on our main results.[28]

[27]This hypothesis is discussed by Sorkin [2015], who argues that if firms decide their level of capital foreseeing a larger growth in the minimum wage then the actual one, the short-run employment elasticity to the policy change could even be positive.

[28]Even assuming that our results were completely driven by frictionless shifts of employees across firms operating under lower-wage contracts –an hypothesis that, in our opinion, is rather extreme and unrealistic given the above considerations– the finding of a negative elasticity of employment to contractual wages would still have policy relevance, as it would entail the presence of a systematic process of job-specific human capital destruction driven by collective bargaining provisions.

5 Contractual Wages' Effects on Pay Levels and Employment

In this section, we present evidences on the wage and employment effects of collective bargaining, as obtained by estimating equation (1) on the grouped samples derived from both, the entire social security records archives (*whole INPS sample*) and the balanced panel of incorporated businesses matched to balance-sheet information (*INPS-AIDA sample*). Table 2 summarizes the results obtained using the former sample, while Table 3 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 group, while columns in the right panel refer to the case in which the dependent variable was employment (number of full-time equivalent workers in the group divided by the local labour market workforce). In all tables, the number of observations was computed omitting singletons, *i.e.* 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 tend to have a strong influence on 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 generally close to 0.5 and always highly significant. This is a quite strong effect when compared to the magnitude of similar elasticities estimated in the context of the minimum wage literature. For example, Neumark et al. [2004], studying the minimum wage effects across the US wage distribution, found elasticities around or above 0.5 only for a relatively small fraction of workers with earnings that were close to the pay floor.[30] Instead, our results show that wage setting institutions exert a considerably stronger influence on Italian pay levels even at the mean level, but this is hardly surprising for several reasons. First, statutory compensations are occupation-specific, so that they are not relevant only for low-income workers. Second, as already mentioned, contractual wages are mostly interpreted as a fixed pay component to be added to the salary of all employees, so that this institution can potentially affect also wages that are already well above the contractual

[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]In a related study that considered US data covering several decades, Autor et al. [2016] found that the minimum wage affected the distance to median earnings only for the fifth and tenth lowest percentiles of the wage distribution, with point estimates of the associated elasticity that did not exceed 0.3.

Table 2: **Effect of Pay Scales on Wages and Employment - Whole 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 labour 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 3: **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).

minimum level.[31]

When looking at the employment effects of collective bargaining, results show a negative elasticity of the full-time-equivalent employment rate within the group to contractual wages. The point estimate was around or below -0.35 in the whole INPS sample, while it was even stronger (around -0.5) in the panel of incorporated businesses. These coefficients were hardly affected by the inclusion of time-varying controls at the local labour market level (activity and unemployment rates). Moreover, they remained quite stable when choosing more saturated definitions of the fixed effects. In specification (2), which we have adopted as the baseline model when performing heterogeneity analyses and robustness tests, we included constant effects for each interaction between time, 20 administrative regions, and the 24-sectors Isic rev. 4 classification. In specification (3) we used instead the 1.5-digits 38-sectors Isic classification, while model (4) included fixed effects for each interaction between these latter industry groups, 107 Italian administrative provinces and time. As can be noticed, the adjusted R^2 was already high in model (2), and increased only marginally in more saturated specifications. Instead, the point estimates of the coefficients were not statistically different across models.

In Table A1 (in the Appendix), we show that results on the employment effects of collective bargaining held also when using alternative definitions of the main variables of interest. In particular, we found similar elasticities when using the average (instead of median) contractual wage of the collective agreement. Moreover, the employment effect was strong and negative also when considering the number of workers employed within each group, instead of their full-time equivalent amount. Thus, we found evidences that firms adjusted to this policy also on the extensive margin, and that the results documented in Tables 2 and 3 were not simply driven by the potential misreporting of days worked.

Table A2 provides estimates of the labour demand elasticities to wages implied by our results obtained using the 2SLS method. As mentioned, 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. As can be noticed, the labour

[31] In general, this will always be true unless a worker and his/her employer agree otherwise through a clause called *superminimo assorbibile*.

demand elasticity to wages was of around -0.8 when using the whole INPS sample, while it exceeded -1 in the baseline specification when using the sample of incorporated businesses. The confidence interval associated to these estimates was also relatively narrow and always well below zero.

To put these results in perspective, notice that Harasztosi and Lindner [2019], reviewing the demand elasticity to wages found across 24 published studies of the minimum wage, found that only seven of them documented a point estimate lower or equal to -0.8. Moreover, only in four cases out of these seven the elasticity was also statistically different from zero, while only eleven studies had a point estimate at least as low as the lower bound implied by our baseline specification (-0.4).[32] A comparison of our results to those available for other studies on collective bargaining is instead less straightforward, given the limited number of applications and the underlying heterogeneity in institutional settings and estimation approaches. Card [1990] found an own-price labour demand elasticity of around -0.5, which was estimated exploiting surprises in real wages in the nominally rigid Canadian union sector, but the associated standard errors were fairly large. 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 not completely saturated model, but the effects of the policy on pay levels were not significantly different from zero in more saturated specifications. Martins [2014], analysing the effect of agreements' extensions in Portugal, documented negative employment effects, but also in this case the elasticity of average wages to this policy was not significantly different from zero.[33] Guimaraes et al. [2017] found an elasticity of net employment growth to the growth in labour costs attributed to collective bargaining of around -0.3 in Portugal. Díez-Catalán and Villanueva [2015], found that Spanish workers with earnings close to pay floors bargained before the 2008 recession had wages on average higher by 2% and their risk of being unemployed increased by five percentage points in subsequent years. Finally, and

[32] The demand elasticity estimated directly by Harasztosi and Lindner [2019] was also close to zero. Some of the standard errors reported for other studies were based on an approximation of the distribution of the ratio of random variables, and not on their actual estimation.

[33] In a related study, Hijzen and Martins [2016] found negative employment effects associated to collective bargaining extensions through a RDD research design and positive effects of extensions on wages at the bottom of the earnings' distribution. However, it is unclear what the labour demand elasticity implied by this study would be, given that the effect of the policy on average wages was not investigated.

quite reassuringly, the confidence interval of our estimates almost overlap with the elasticity of employment with respect to labour cost induced by a wage change derived by Cahuc et al. [2018] for France, a labour market relatively similar to the Italian one in terms of size and institutional characteristics. In this last case, the labour demand elasticity was recovered using the variation in employment induced by a hiring subsidy, rather than a change in collective bargaining provisions.

Overall, our results suggest that the employment effects of government-legislated pay floors tend to be smaller than those associated to centralized collective bargaining. Indeed, the magnitude of the labour demand elasticity that we have documented shows that employment adjustments to higher wages can be larger than what previous studies based on minimum wage hikes would imply. This discrepancy in the results can in principle be associated to several mechanisms and underlying factors. First, given the nature of that policy, minimum wage studies often implicitly refer to the employment elasticity to higher wages among young, less skilled workers and low-wage sectors, while collective bargaining affects labour costs for a wider range of employees and activities. Thus, government-legislated pay floors could have limited dis-employment effects due to a consistently smaller impact on a company's costs, or due to a lower degree of substitutability characterising workers at the bottom of the wage distribution. This last mechanism would be broadly consistent with the hypotheses set forth in the polarization literature, according to which capital-labour substitutability is high for median levels of the earning distribution, and relatively low at the top and bottom of it (see in particular Goos and Manning [2007]).

Rather than to the characteristics of wage setting policies, the discrepancy of our results to those documented in the minimum wage literature could be linked to the fact that Italian firms were more responsive to labour costs due to underlying compositional factors (*e.g.* due to a manufacturing- and export-oriented industry composition). Similarly, the parameters documented in this study could be influenced by the generally negative business cycle that characterized Italy during the period covered by our data. In order to gain more knowledge on the relevance of these and similar hypotheses, the Appendix B summarizes heterogeneities in the policy effect found across several dimensions, in particular: economic activities, population groups and business cycle fluctuations.

In general, results presented in the Appendix B show that while the wage effects of collective bargaining were seizable 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 characterised by high levels of employment protection legislation, as well as in some large tertiary industries, in particular the trade, transport and tourism one. On this last respect, not all of the associations found were consistent with a simple categorization of activities according to their degree of tradeability, given that, for example, significant disemployment effects were found also in the construction sector, which tends to be insulated from international competition. Finally, we did not find significant heterogeneities in the results depending on business cycle dynamics at the local labour market level, as proxied by the unemployment rate evolution. Overall, the fact that employment effects related to collective bargaining were significant for a fairly large portion of the Italian private sector, and that they were invariant to local business cycle fluctuations, suggests that our estimates of the own-price labour demand elasticity may have a more general external validity.

6 Labour Demand Elasticity and Firm-Level Outcomes

This section describes heterogeneities in the labour demand elasticity across firm-level outcomes. For this purpose, we have relied on the INPS-AIDA panel of incorporated businesses, for which we had information on revenues, value added and owned physical capital. Using these balance-sheet variables, we have analysed differences in the size of employment adjustments to higher wages across the distribution of the following outcomes: value added per worker and its evolution; total revenues; the share of the wage bill of each collective contract in total revenues; capital owned over total labour costs and its evolution.

These variables provide broad measures of a firm's efficiency (value added per worker), size (revenues), labour costs shares, capital intensity and capital-labour substitution. Table A3 (in the Appendix) reports descriptive statistics on these outcomes. As can be noticed, productivity, revenues and investments in physical capital followed a negative dynamic during the period of analysis. On average, revenues were more than seven time the size of contract-specific labour costs, while the value of owned physical capital was more than four times larger than the wage bill.

Simply comparing labour demand elasticities separately estimated for different levels of the above mentioned outcomes would not be optimal. The balance-sheet variables that we have considered could themselves be affected by collective agreements. Moreover, pay scales could be set differently depending on the average level of these balance-sheet indicators within a contract. In order to overcome these problems, we have avoided classifying firms along dimensions that were likely to interact with policy characteristics. Instead, for each firm-level outcome we have constructed a measure of distance from the collective agreement average, measuring the size of labour demand elasticities across the distribution of this distance. In this way, we have been able to compare results across firms' characteristics conditioning on differences in wage setting policies faced by them. In particular, we have estimated the following regression model

$$f_g = \psi_c + r_g$$

where f_g is a firm-level outcome, measured either as the yearly average between 2007 and 2015, or, for growth variables, as the difference between the average in 2013-2015 and the average in 2007-2009.[34] The above 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). Having obtained estimates of the residual $\hat{r}_g$, we have constructed five quintiles of its distribution and computed the labour demand elasticity within each of them. Through this approach, we were able to characterize associations between the intensity of adjustments to contractual wages on the employment margin and several firm-level dimensions, controlling for composition effects driven by reverse causality.[35]

[34]In general, the use of averages computed over more years allows to limit measurement error problems and to mitigate the effects of year-specific shocks in balance-sheet variables. Notice also that since the underlying panel of firms was balanced, the years used to compute these averages were the same for all firms within a collective agreement, unless for what concerns firms that completely stopped relying on workers hired under a given contract (but the influence of these observations on the overall results was limited by the use of weights).

[35]We did not control for the relationship between collective agreements and second moments of firm-level outcomes f_g. This introduces some compositional effect, given that contracts that were applied on a more heterogeneous population of companies were probably over-represented in the tails of the distribution of $\hat{r}_g$.

Figure 3 reports the labour demand elasticity (as estimated through 2SLS) by quintiles of distance in given firms' outcomes (averaged over the period 2007-2015) from the contracts' mean. Figure 4 presents similar elasticities for balance-sheet indicators f_g defined in terms of growth between the average in 2007-2009 and in 2013-2015. All elasticities were estimated through interactions with quintiles of $\hat{r}_g$ and controlling for time fixed effects interacted by regions and Isic 21 industries controls, *i.e.* adopting an equivalent specification to model (2) in Table 3. Tables A4 and A5 (in the Appendix) provide the full list of policy effect coefficients associated to wage and employment levels for each quintile of the distance between a firm's outcome and its collective contract average.

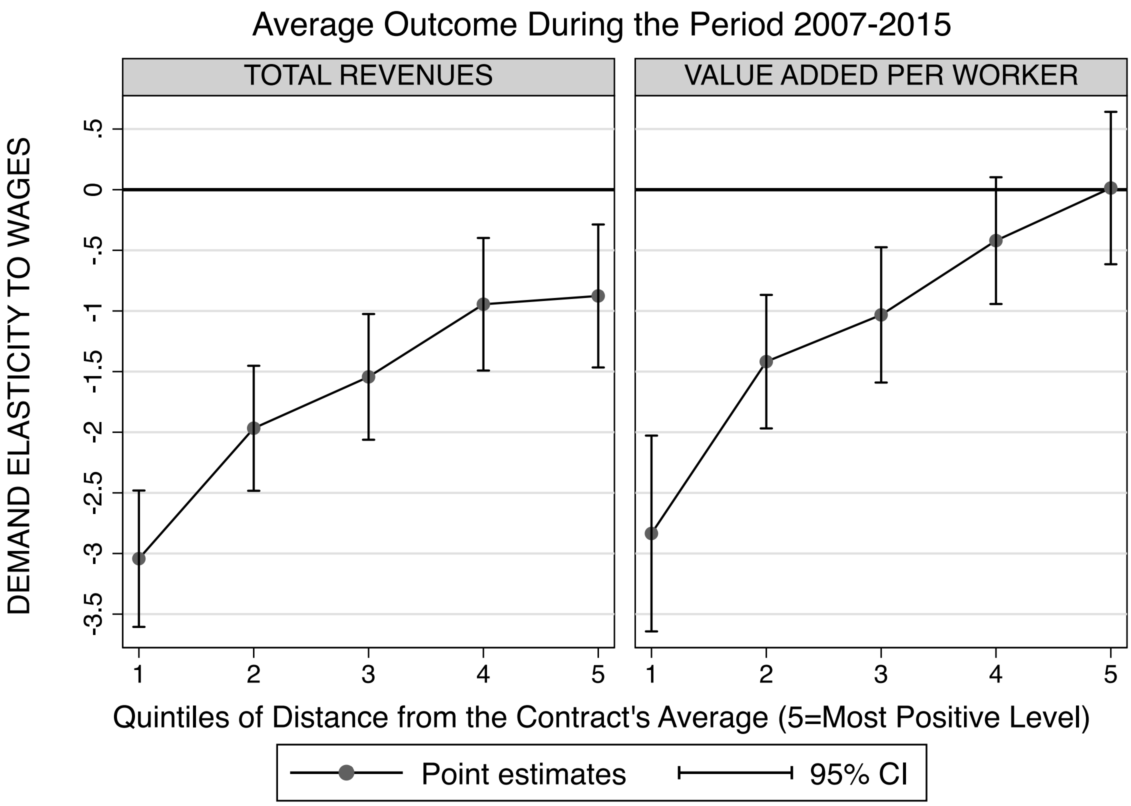
As shown in the top panel of Figure 3 there were clear patterns of more negative elasticities among smaller firms and among those with low value added per worker. These two tendencies may reflect similar underlying mechanisms as long as size and productivity were positively correlated. To some extent, the former could also suggest that relatively small companies had lower influence on the wage setting process and were not able to negotiate a wage growth more tailored to their needs. Interestingly, high value-added per worker firms did not experience employment losses for a given growth in pay scales. This hints at the presence of rents among best performing companies, which could be linked *e.g.* to higher monopsony power or to the ability to limit employment losses through labour hoarding (*i.e.* draining other firms' resources, such as liquidity, see *e.g.* Giroud and Mueller [2017]).

A more nuanced mechanism explaining lower employment losses among high value-added firms, which would be consistent with the collective bargaining theory of Moene and Wallerstein [1997], could be the tendency to adopt low collectively bargained centralized wage standards also at firms where pay levels would be higher under a decentralized equilibrium.[36] In such a setting, efficient employers can potentially benefit from excess profits, as wages are not directly linked to workers' usefulness to firms or to their outside options. This mechanism may in part rationalise the active support toward centralized wage setting procedures often

On the other hand, controlling also for this second-order effect –*e.g.* by defining quintiles on a contract-specific distribution of $\hat{r}_g$– would come at the cost of losing more information on the intensity of differences in firm-level outcomes.

[36] On this respect, Wallerstein [1999] provides a cross-country evaluation of the link between wage equality and pay setting institutions and a critical discussion of several evidences that fit well with this modelling choice of Moene and Wallerstein [1997].

Figure 3: **Labour Demand Elasticity across Quintiles of Average Firm Level Outcomes**



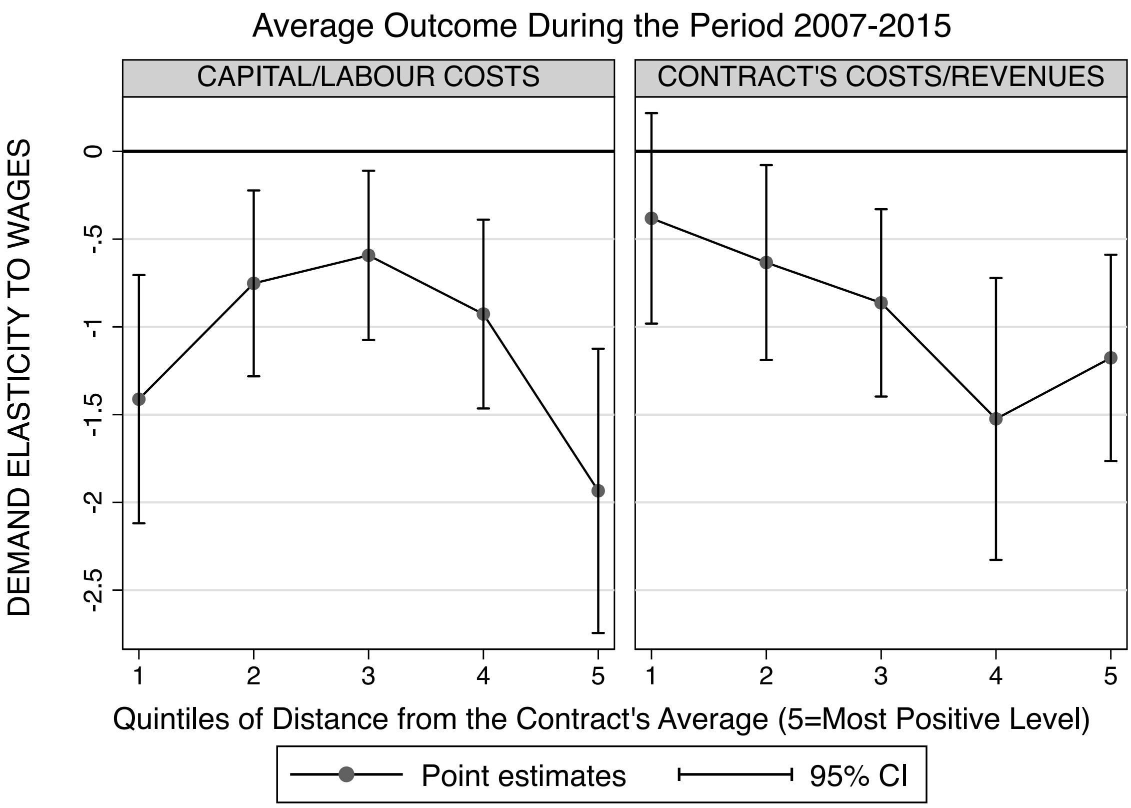
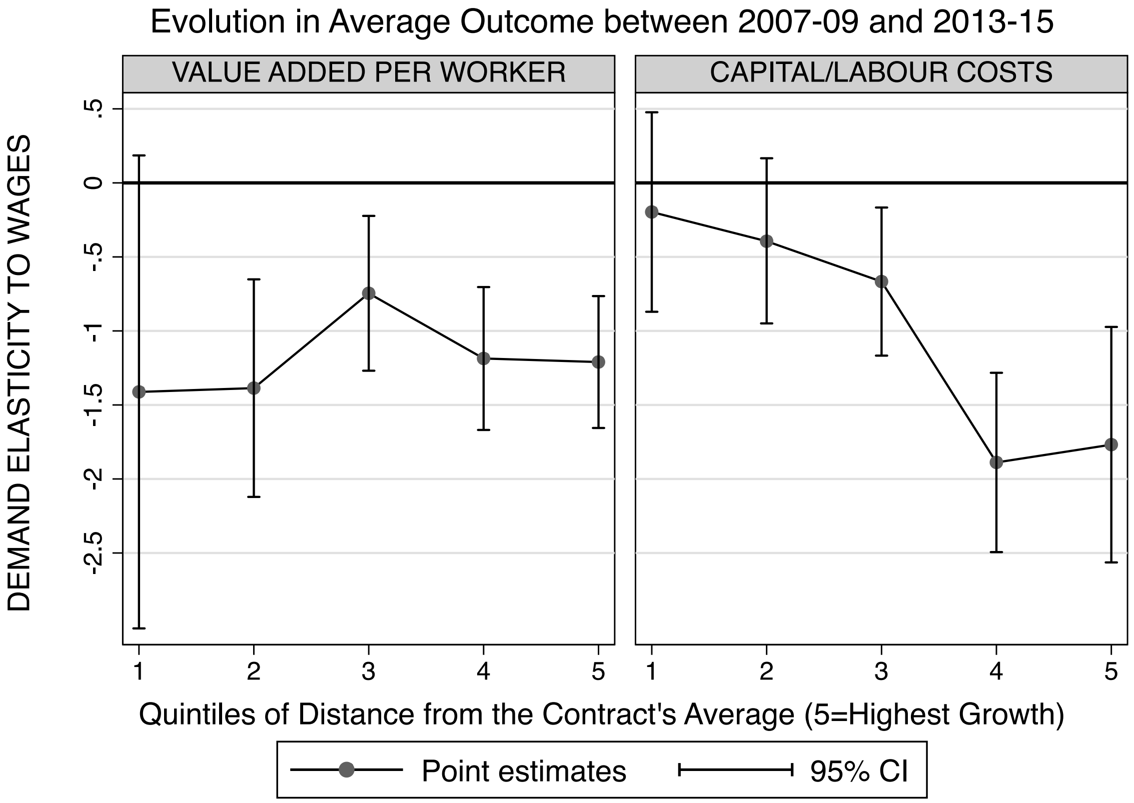


Figure 4: **Labour Demand Elasticity across Quintiles of Growth in Firm Level Outcomes**



expressed by the largest Italian employers' association. Moreover, a general adherence to centralized standards and the pervasiveness of "wage moderation" would be consistent with an almost irrelevant contribution of employers' pay heterogeneity in shaping the evolution of Italian inequality, an evidence that was recently documented by Devicienti et al. [2019].[37]
The lower part of Figure 3 shows that the relationship between capital intensity and the elasticity of labour demand had an inverse u-shape, with values closer to zero among firms more similar to the contract's average. The bottom-right panel shows instead that this elasticity was not statistically different from zero among firms whose labour costs specific of the collective agreement represented a smaller share of total revenues. This latter evidence (as well as the tendency toward more negative elasticities in companies with relatively low

[37]This study also shows that the evolution of wage differentials across several decades was closely linked to collective bargaining's provisions, and that inequality has been persistently flat in Italy during the years covered by our analysis, thus closely following the dynamics of contractual pay levels (see in particular Figure 1).

capital/labour costs shares) can be considered consistent with one of the Hicks-Marshall laws of derived demand, which predict larger employment adjustments to wage changes the larger the labour share in total costs, as long as the product demand is sufficiently elastic (*e.g.* Hamermesh [1993]).[38] The observation of more negative elasticities also at most capital-intensive establishments is instead less straightforward to rationalize using static theoretical arguments. In part, it may be related to excess capacity, which, in the dynamic framework of Sorkin [2015], where capital can be purchased instantly but is fixed once installed, may arise in the presence of binding wage floors exceeding employers' expectations and exacerbates negative employment adjustments.

Figure 4 shows that the labour demand elasticity was more negative at firms that increased the most their capital/labour share (right panel). Also this evidence can be considered consistent with standard theory, as the possibility of substituting workers with equipment in the production process is deemed to be a typical determinant of this elasticity. However, the left panel shows that there was no clear relationship between the size of employment adjustments to higher contractual wages and the growth in value added per worker. Thus, firms with the most negative labour demand elasticity did not experience a predominantly faster growth in efficiency, than those less sensitive to changes in contractual wages.

These last evidences do not seem consistent with a tendency toward efficient technology adoption among firms more likely to cut employment when facing higher wages. A related hypothesis is discussed by Acemoglu [2003], who models employers' decisions about whether to adopt technologies as dependent on the ability of workers and on wage floor levels. In that setting, equilibria where low-ability firms increase investments, but reduce employment, when facing higher binding wage floors can arise, since technology allows to align productivity to the higher pay levels. However, this argument was built to model differences in unemployment, wage dispersion and technology adoption across countries, rather than to characterize firms' heterogeneity in adjustment decisions, so that studying more specific implications related to this theory would not be straightforward in the present context.

[38] In Hamermesh [1993] notation, the own-price elasticity of labour (in the two-inputs case) reads as $\eta_{LL} = -(1-s)\sigma - \eta s$, where s is the labour share in revenues, σ is the technical rate of substitution and η is the product demand elasticity. Thus, η_{LL} is decreasing in s as long as $\sigma < \eta$

When considering empirical studies on the relationship between minimum wages and productivity, the associations documented in Figure 4 seem in part consistent with existing evidences. In particular, Riley and Bondibene [2017] found that the productivity-enhancing effects of the British minimum wage occurred through the adoption of better organizational practices, rather than through cuts in employment and capital-labour substitution.[39] Under this circumstance, we should not expect to find a faster growth in efficiency among firms that were more likely to cut employment. However, the fact that value-added per worker growth was generally unrelated to the size of firms' adjustments to changes in contractual wages on the employment margin suggest that, if any, the effects of collective bargaining on efficiency were relatively modest. Given that heterogeneities in product-market demand elasticities were likely to be limited across firms belonging to the same collective agreement, and that residual differences along this dimension were likely to be absorbed by the fixed effects of our regression model, it seems reasonable to conclude that, in our context, companies that adjusted less on the employment margin were probably reducing profits or liquidity more than increasing efficiency.[40]

7 Dynamic Effects and Robustness Tests

This section presents results on the dynamic employment effects of contractual wages, together with a series of robustness tests. The hypothesis that a shock in labour costs may take a long period of time to exert its full effects has to be taken into account due to several considerations. In particular, as argued by Sorkin [2015] firms usually cannot change their organizational structure and capital levels in the short run and this may cause delays in adjustment decisions, which, at the time of a minimum wage hike, could also be hidden by firms' expectations before the policy took place.

The issue of timing in the measurement of the treatment has been acknowledged also in the empirical minimum wage literature (*e.g.* Neumark and Wascher [1992], Dube et al. [2010],

[39] However, our findings are less consistent with the evidence of higher productivity achieved through capital-labour substitution among firms most hit by a minimum wage in China, which was documented by Mayneris et al. [2018].

[40] The relevance of the relationship between minimum pay floors and profits has been documented directly, but in a different context, by Draca et al. [2011].

Meer and West [2016] and Cengiz et al. [2019]). The most common regression model used to investigate the relevance of anticipatory or lagged policy effects is a dynamic specification of equation (1), which in our context reads as

$$y_{gt} = \sum_{i=\tau}^{T} \beta_i \mathrm{PS}_{c(t-i)} + \gamma x_{mt} + \alpha_g + \phi_{slt} + \epsilon_{gt} \tag{2}$$

where y_{gt} is the log full time equivalent employment rate of group g and all other variables are defined as in equation (1). Throughout this section, we have specified ϕ_{slt} as a 21 Isic sectors-20 regions time effect, avoiding the inclusion of more saturated sets of controls in order to limit the level of multicollinearity. The above regression model is usually called *distributed lags*, but notice that τ can be set lower than zero in order to include leading levels of PS_{ct}.[41] An equivalent formulation, which isolates the variability across the terms $PS_{c(t-i)}$ that contributes to the coefficients' estimation, is the following

$$y_{gt} = \sum_{i=\tau}^{T-1} \delta_i \Delta \mathrm{PS}_{c(t-i)} + \delta_T \mathrm{PS}_{c(t-T)} + \gamma x_{mt} + \alpha_g + \phi_{slt} + \epsilon_{gt}$$

where Δ is a one-period difference operator (from $t-i$ to $t-i-1$) and the corresponding estimates of β_i can be recovered as linear combinations of the coefficients δ_i.

The main difficulty in estimating equation (2) is given by the likely presence of correlation between leading and lagged values of contractual wages PS_{ct}. In our context, the growth in nominal pay scales across time was quite small (0.2% on average) and, more importantly, its standard deviation was of only 0.7% in both the whole INPS and INPS-AIDA samples. Thus, the policy variable of interest was highly persistent, which in turn implies that the regression model of equation (2) is likely to produce volatile results due to multicollinearity. Furthermore, the length of the time window $T-\tau$ in which policy effects are included is negatively correlated with the sample size, which further reduces the variability available for estimating long-run responses to contractual wages.

Near-perfect multicollinearity is a well-known classical problem associated to distributed lags

[41] In this model anticipatory policy effects are estimated by the coefficients β_i associated to leading levels of the policy, while long-run adjustments after the policy are estimated by the coefficients of the lagged levels.

(*e.g.* Alt [1942]), but it is also often overlooked or only implicitly mentioned in the most recent empirical literature. In Appendix C, we present Monte Carlo experiments on a dynamic process with similar levels of autocorrelation than the one observed for contractual wages, showing that results obtained using dynamic regression models similar to (2) can have indeed a volatile behaviour, which increases with the number of parameters to be estimated and decreases with the sample size. The most straightforward solution to near-perfect multicollinearity, typically adopted also in the recent literature studying the dynamic effects of the minimum wage on employment (*e.g.* Dube et al. [2010], Meer and West [2016] and Cengiz et al. [2019]), involves omitting relevant lags from equation (2), which is equivalent to restrict some of the parameters β_i as equal to zero. This approach, which in the limiting case corresponds to estimating the static specification of equation (1), introduces bias on the estimates $\hat{\beta}_i$. However, this bias is quite predictable through the usual formula for the effect of relevant omitted variables, given that the correlation between PS_{ct} and its lags is generally positive and decreasing in size for periods further away from t. Therefore, in the extreme case of the static model, $\hat{\beta}$ can be interpreted as a weighted sum of short- and long-run elasticities, with more importance given to those that are closer to t.

It follows from the above discussion that, under mild assumptions of relative stability in the sign and size of short- versus long-run elasticities, the coefficients associated to the static model are generally biased toward the cumulative effect of the policy. Sorkin [2015] explicitly models firms' dynamic behaviour around the introduction of a binding wage floor, showing that long- and short-run employment elasticities could diverge when the size of the policy change is unexpected or perceived as only temporary. In the Italian context, both of these concerns seem alleviated by the fact that dates of contract renewals, their duration and often also the new levels of contractual wages are known by employers before their implementation.[42] Thus, assuming that imposing restrictions of the form $\beta_i = 0$ for relevant lags $\mathrm{PS}_{c(t-i)}$ makes the unrestricted coefficient of a correlated regressor biased toward the cumulative pol-

[42]On this respect, it should be noticed that in Italy the wage growth mandated by collective agreements since 1993 has been quite stable and coordinated across sectors in order to reach low-inflation targets (see *e.g.* Dell' Aringa and Pagani [2007]).

icy effect seems reasonable in this setting.[43]

Estimates derived from versions of equation (2) have been also used in the literature as a test for the robustness of the differences-in-differences identification (*e.g.* Dube et al. [2010], Meer and West [2016] and Cengiz et al. [2019]). Indeed, a given contractual wage level should not have significant employment effects before its implementation, unless for periods fairly close to its implementation when the policy is announced and its content is well predictable. A placebo test for leading levels of PS_{ct} in equation (2), such as $\mathrm{H}_0 : \beta_i = 0 \; \forall \tau \geq i > 0$ can be performed using distributed lags. However, in performing this procedure the risk of committing type two errors (*i.e.* rejecting the null when it is true) should not be underestimated, as this possibility may arise due to the presence of near perfect multicollinearity among leads and lags of the policy.[44] Moreover, as shown by Cengiz et al. [2019], the distributed lags approach is generally more demanding than standard placebo tests available in the context of event-study analyses, which typically restrict the sample around the event window. Indeed, through distributed lags also long-run pre-existing differences in employment trends across treated and control groups can be measured, while an event-study approach simply ignores such differences if they arise far away from the policy.[45]

Given the existence of a bias-precision trade-off driven by multicollinearity, as illustrated in Appendix C through Monte-Carlo experiments, we have estimated two alternative restricted specifications of equation (2) using OLS. First, we have included one term $\mathrm{PS}_{c(t-i)}$ every five months, setting $\tau = -20$ and $T = 20$ and restricting all other parameters β_i as equal to zero.

[43] If the assumption of relative stability in the sign and magnitude of short- and long-run elasticities does not hold, then the coefficient estimated using a static model would still represent a weighted sum of short- and long-run effects, but it would be dubious to establish whether this sum was closer to the cumulative policy effect than the unbiased contemporaneous elasticity.

[44] Spanos and McGuirk [2002] show that there is no monotonic relationship between the size of confidence intervals and the degree of multicollinearity, so that the estimation of spurious but significant parameters is always a possibility whenever regressors are highly correlated. In Appendix C we show that the probability of wrongly rejecting $\beta_i = 0$ for regressors correlated with relevant policy levels is not negligible also in the presence of a large sample size.

[45] Cengiz et al. [2019] argue that if the distributed lags model detects pre-existing differences in employment trends across units far away from the policy, the event-study estimation approach should be preferred (provided its respective short-run placebo falsification test holds). While this might hold true in the specific US case study analysed there, in our opinion this argument is not generalizable. Indeed, the distributed lags and event-study approaches are built on the exact same assumption (absence of differences in employment trends across units apart from those generated by the treatment), while the placebo is only a diagnostic tool to evaluate this assumption, not a test for it. Thus, a rejection of this falsification test in the more demanding model equally harms the credibility of both approaches.

Thus, we have studied the employment effects of collective bargaining up to twenty months before and twenty months after the introduction of a new contractual wage level, estimating one employment elasticity every five months within this window. In a second specification, we have estimated a model designed to better test the presence of anticipatory policy effects. Namely, we have included only four leading terms of PS_{ct} for every five months up to twenty months before the policy implementation, together with the contemporaneous contractual wage level. By reducing the number of parameters, this latter approach provides more robust estimates of potential anticipatory effects.

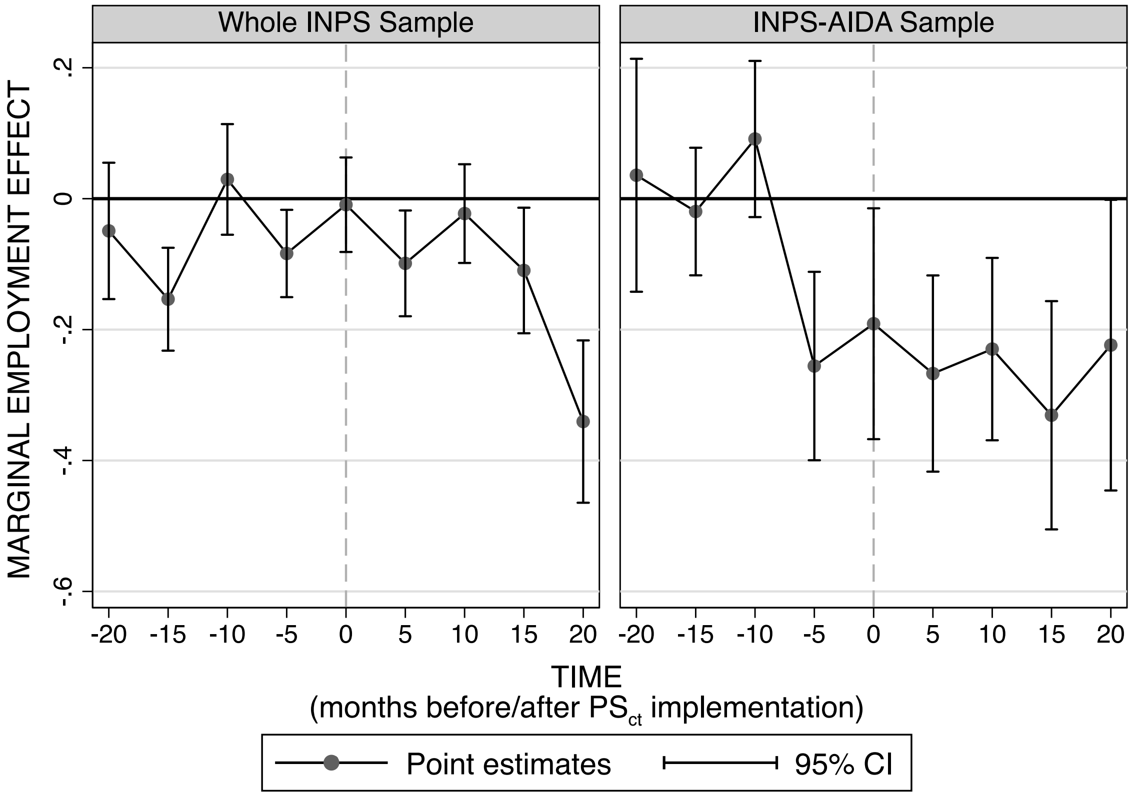
Figure 5 shows results for the OLS estimates computed on both, the whole INPS and INPS-AIDA samples. The top-left panel shows that, in the more comprehensive panel derived from the social security archives, the strongest employment effects of contractual wages took place with some delay with respect to their implementation. Indeed, the only coefficient below -0.2 was the one associated to the twenty months lagged term. Instead, we found smoother adjustments in the balanced panel of incorporated businesses (top-right panel), given that no significant differences could be detected among lagged and contemporaneous effects.

For what concerns anticipatory effects, policy changes induced significant employment adjustments already five months before their implementation. Given that the content of new collective agreements is typically known by employers in advance, the presence of such anticipatory adjustments starting from around five months before the policy does not seem too problematic. The fact that they are of the same (negative) sign of post-policy adjustments further corroborates this point, ruling out hypotheses such as the possibility that higher contractual wages are introduced just after positive business cycle fluctuations.

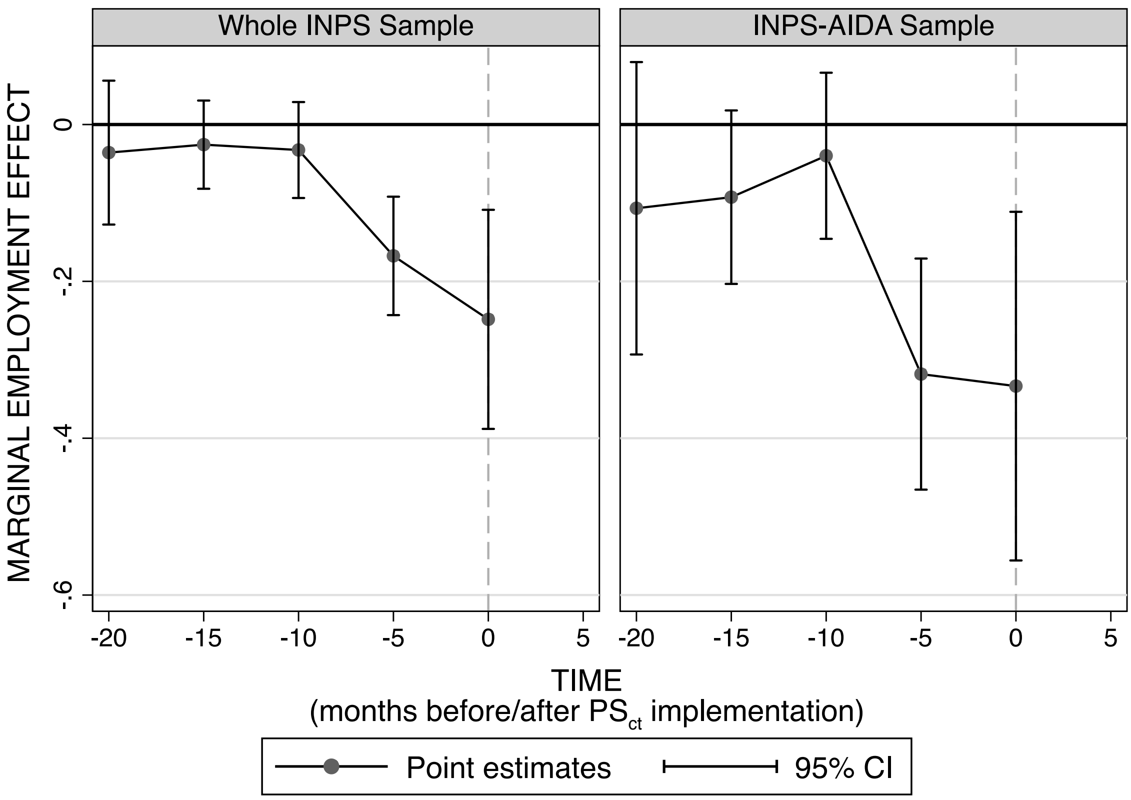
It should be noticed however that, in the whole INPS sample (top-left panel), also the coefficient associated to the fifteen months leading term was negative and significantly different from zero, which seems a result more difficult to interpret as an announcement effect. To gain more knowledge on the robustness of the estimated anticipatory effects, the lower panels of Figure 5 show the results obtained by including only the contemporaneous and leading levels of PS_{ct}. In this specification all parameters are estimated with less volatility, given that the sample size is larger and the number of almost collinear variables is lower. As can

Figure 5: **OLS-Distributed Lags Specifications**

9 Parameters, 41 Months Window with 32 $\beta_i = 0$ Restrictions



5 Parameters, 21 Months Window with 16 $\beta_i = 0$ Restrictions



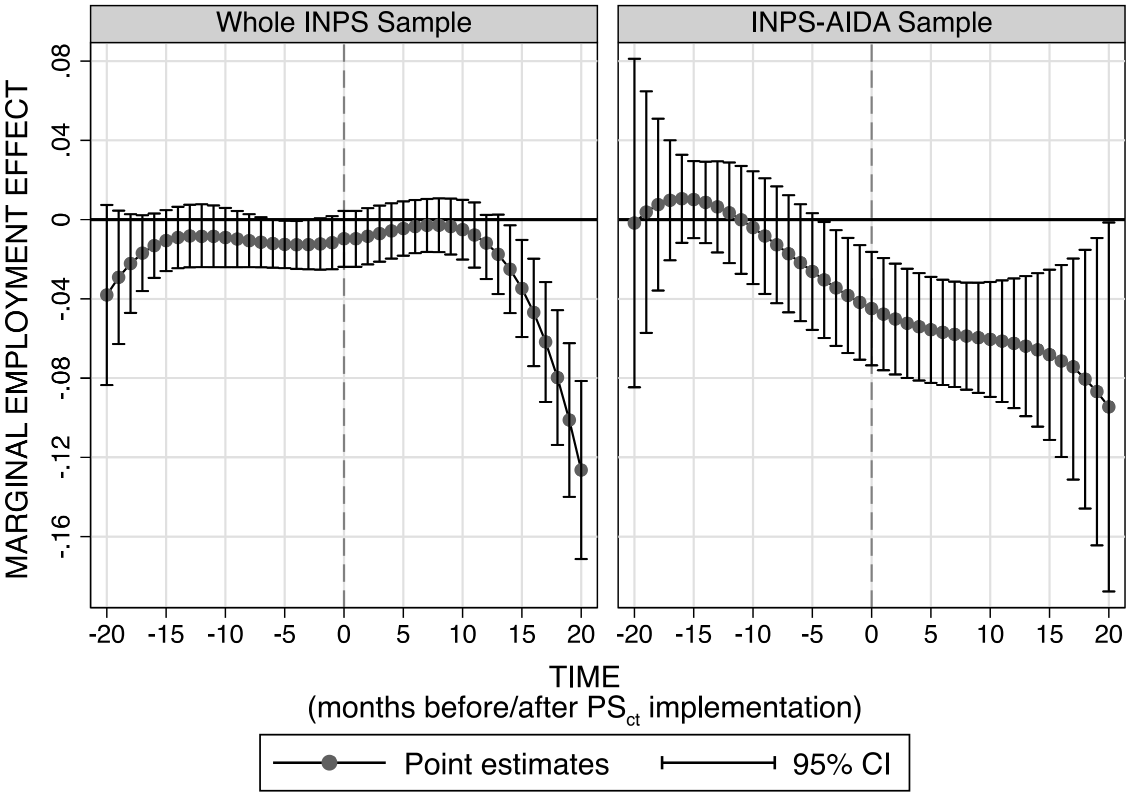
be noticed, in this more robust specification the significance of $\beta_{(t-15)}$ in the whole INPS sample disappeared, which casts doubts on its actual relevance. Instead, the $\beta_{(t-5)}$ and $\beta_{(t)}$ parameters were still significant and, in some cases, even more negative. This last result is probably driven by omitted variable bias, given that all lags of P_{ct} were excluded from this model while being negative and significant in less restricted specifications.

One obvious limitation of the results presented so far is represented by the high number of strong restrictions $\beta_i = 0$ that were imposed. On the other hand, letting all parameters $\beta_{(t-i)}$ be unrestricted would be infeasible due to extreme volatility. This identification problem can be addressed also using an alternative method to OLS, in particular the estimator proposed by Almon [1965]. The Almon technique relies on restricted least squares, as it transforms the independent variables providing lagged effects on the outcome into weighted sums and the unrestricted parameters of distributed lags into linear combinations of each other. This solution can potentially brake the problem of multicollinearity, but at the cost of restricting the effects β_i to lie on a polynomial. For a given degree p of such polynomial, $p+1$ parameters have to be estimated, so that its shape can not be too flexible in the presence of a highly persistent process, while there are no standard procedures for choosing p, the minimum number of lags to be included, or to detect the presence of bias induced by an incorrect interpolation of the parameters β_i (see *e.g.* Hendry et al. [1984]). Despite these limitations, the Almon technique has the clear advantage of allowing the estimation of a large number of parameters adopting a low-dimensional model. Thus, in our context the use of an Almon estimator seems particularly helpful for predicting the outcome's adjustment path at each point of time.

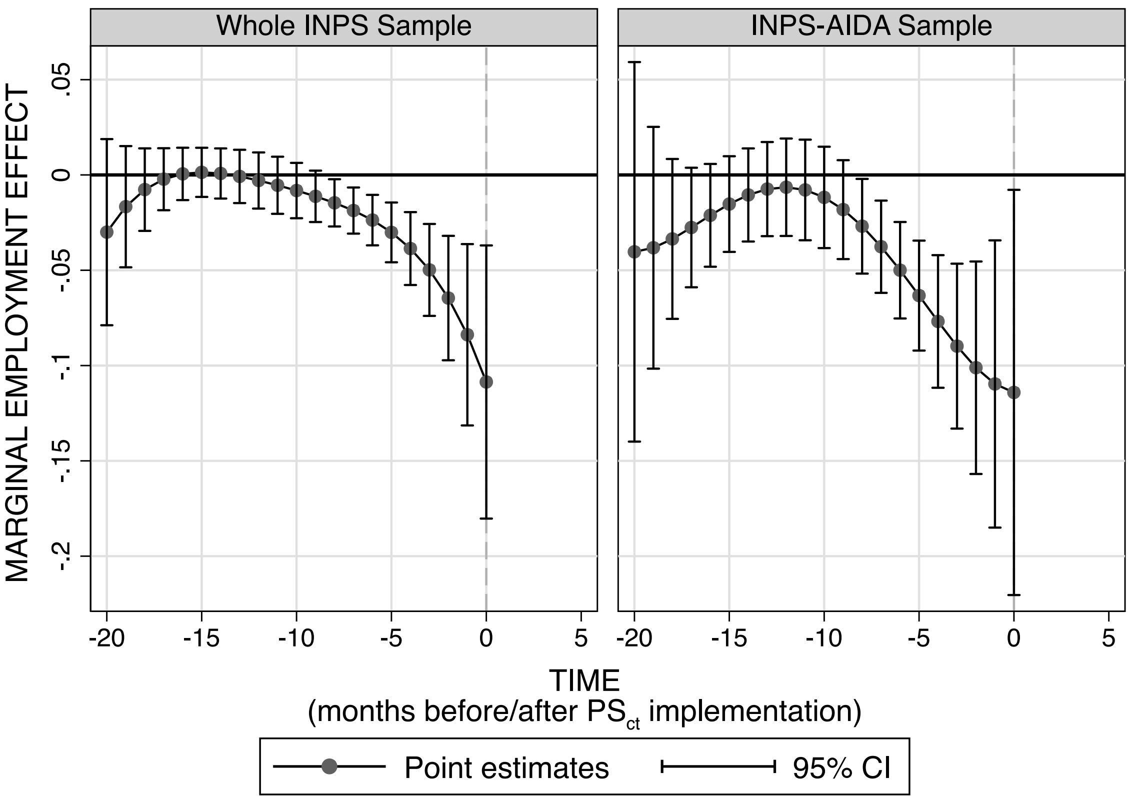
Figure 6 shows results obtained by estimating the Almon model on the same 41 months (top panels) and 21 months time windows adopted when using OLS. We have chosen to restrict the parameters on a fourth degree polynomial, as this was the highest feasible degree for computing the variance covariance matrix. As can be noticed, in both samples long-run anticipatory effects were not significant. When considering the 41 months time window, in the whole INPS sample we found marginally significant effects associated to the policy five, four and three months before its implementation. In later periods, the policy effect was always close to zero until fourteen months after the contractual wages' implementation, when

Figure 6: **Almon-Distributed Lags Specifications**

41 Months Window, β_i Restricted on a Fourth Degree Polynomial



21 Months Window, β_i Restricted on a Fourth Degree Polynomial



it started to become always more negative. In the INPS-AIDA sample, anticipatory effects started only from four months before the policy, but were generally stronger in magnitude. Similarly to the results documented using the OLS model, the adjustment path observed among incorporated businesses was relatively smooth and of similar size across periods after the contractual wages' introduction.

In the specification reported by the bottom panels of Figure 6, where the Almon coefficients are estimated using only a 21 months window of leading terms and the contemporaneous policy level, both the contemporaneous and anticipatory effects were biased downwards, given the omission of lagged terms. In both samples the estimated parameters were starting to be significantly different from zero only from eight months before the policy implementation onwards. This last result casts further doubts on the robustness of the $\hat{\beta}_{t-15}$ estimate reported by the top-left panel of Figure 5, which does not appear to be different from zero when using alternative specifications.

Overall, distributed lags estimated using various methods pointed out to the absence of long-run anticipatory effects -a finding consistent with our identifying assumptions- and to the presence of significant long-run employment elasticities, with adjustments to the policy starting from around five months before its implementation and taking place across more than twenty months. In order to further test whether underlying endogenous employment trends occurring within collective agreements were threatening our identification, we have also estimated the static model of equation (1) including separate linear time trends for each collective agreement. This specification runs the risk of controlling for actual policy effects, as it relies only on sharp employment adjustments taking place within collective agreements around their renewals' date in order to identify the parameter of interest. Table A6 (in the Appendix) presents the results of this test computed on both samples. While there were still sizeable and significant (but smaller) negative employment effects of contractual wages in the whole INPS sample when using this specification, the same parameter was not significant in the balanced panel of incorporated businesses. However, in this latter case it is likely that the relatively smooth adjustment path observed among such firms (Figures 5 and 6), which is in part artificial due to the balanced panel sampling that excludes new entrants and closing-down companies, made the identification of sharp adjustments to the policy infeasible.

8 Conclusions

In this paper we have shown that Italian collective bargaining exerts a strong influence on wages and on employment levels. The labour demand elasticities estimated using contractual pay levels as an instrument for wages were in general more negative than those typically documented in the minimum wage literature. The most plausible reason for this discrepancy in the results are the substantial differences existing between these two wage-setting policies. A government-legislated pay floor typically affects only workers at the bottom of the earning distribution. In this context, there are at least two mechanisms that, consistently with standard theory on labour demand, could rationalise heterogeneities in the findings associated to each of these policies. First, differently from the collective bargaining case, employees affected by a minimum wage represent on average only a small fraction of firms' production costs. Second, they could be concentrated into jobs characterized by a low labour substitutability. This latter possibility has been emphasised also in the context of theories of polarization of the workforce, which have aimed at rationalizing the secular growth in wage inequality observed in most Western countries (*e.g.* Goos and Manning [2007]).

Collective bargaining is a policy more suited to measure the size of employment adjustments to a generalised growth in the cost of labour. Indeed, the provisions of this institution affect workers in all occupations and, in the Italian context, tend to be binding also for employees paid above the relevant minimum levels. For these reasons, the labour demand elasticities that we have documented are probably closer to the underlying economy-wide parameters. The quality and reliability of these estimates was further evaluated through a rich set of specifications and robustness tests. We have documented the shape and relevance of dynamic adjustments to increased labour costs, finding negative employment responses as far as twenty months after (but only five months before) the policy implementation. On this respect, a critical evaluation of the standard regression approach for estimating long-run elasticities (the distributed lags model) revealed that its results should always be carefully considered and compared using alternative specifications and estimation methods, given that the risk of running into high volatility is substantial whenever the policy of interest is persistent.

We have performed a wide range of heterogeneity analyses for the static labour demand elasticity. We found that workers most hit in terms of employment probability were young and prime-aged individuals and those under fixed-term contracts with low levels of employment protection. We did not find associations always consistent with a positive relationship between the tradeability of an industry and its factor demand elasticity, but, in our context, the availability of pass-through mechanisms in the product market (*e.g.* higher prices or lower sales) were likely to be influenced also by the bargaining structure within each sector. Instead, we found associations broadly consistent with the technological determinants of the labour demand elasticity implied by the Hicks-Marshall theory. In particular, the sensitivity of employment to wages was higher at firms where the share of collective contract workers' costs in revenues was higher, and it was higher in establishments where the capital share in total labour costs had increased the most during the period covered by our data. Both of these evidences suggest that scale effects and opportunities to increase the share of equipment in total costs play a role in shaping firms' adjustment decisions.

We found evidences consistent with the presence of rents among most productive firms, as they did not show sizeable employment adjustments to increased labour costs. Concentration of resources and presence of excess profits at more efficient establishments are the predictions of traditional models of collective bargaining (*e.g.* Moene and Wallerstein [1997]). This outcome can arise in a context where most employers adhere to coordinated and centralized wage standards, which in the Italian case is consistent with the observation of persistently flat wage inequality trends during the period covered by our study (Devicienti et al. [2019]). This characteristic of collective bargaining was traditionally deemed as beneficial, as it leads to a redistribution of resources toward most efficient companies, but this conclusion is arguably more dubious in the presence of high unemployment.

We have also documented no associations between the labour demand elasticity and faster than average growth in value added per worker. This evidence is not consistent with the presence of efficiency-enhancing effects of binding wage floors occurring through larger employment cuts and capital-labour substitution (*e.g.* Acemoglu [2003]). For what concerns another potential mechanism shaping the relationship between higher wage standards and productivity, namely the adoption of better organizational practices for a constant employment level –documented in the context of minimum wages by Riley and Bondibene [2017]–, the absence of any correlation between a firm's value added per worker growth and its labour demand elasticity suggests that also this factor was a rather limited driver of the heterogeneity in the size of employment adjustments across companies. Overall, profits reductions and labour hoarding –rather efficiency growth– probably represented the second most important channel through which firms adapted to contractual wages' provisions. However, further work is warranted in order to test more nuanced implications related to the potential "industrial policy" effects of collective bargaining.

Overall, the evidences presented in this paper suggest that the adoption of a decentralized system of wage setting could be a promising tool to generate higher employment and to reduce rents among most efficient firms, without harming investment and innovation incentives in less performing companies, but at the cost of greater pay inequality. For what concerns employment and earnings, Boeri et al. [2019] reach similar conclusions by comparing the Italian system to the more flexible German one. Further welfare analyses should be carried out in order to provide precise characterizations of other counterfactual scenarios.

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Appendix

A Other Figures and Tables

Table A1: **Effect of Pay Scales on Employment - Alternative Definitions of the Main Variables**

Sample	*Whole INPS*		*INPS-AIDA*	
Dependent Variable				
Group's Log FTE Empl. Rate	✓		✓	
Group's Log Empl. Rate		✓		✓
Coefficients				
Median PS_{ct}		$\mathbf{-0.455}^{**}$		$\mathbf{-0.580}^{**}$
S.e.		*0.083*		*0.149*
Average PS_{ct}	$\mathbf{-0.302}^{**}$		$\mathbf{-0.490}^{**}$	
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 labour markets and two-digit sectors (whole 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 A2: **2SLS Estimates of the Employment Elasticity to Wages**

Sample:	*Whole INPS*			*INPS-AIDA*		
	(1)	**(2)**	**(3)**	**(1)**	**(2)**	**(3)**
Coefficient						
w_{gt}	$\mathbf{-0.806}^{**}$	$\mathbf{-0.795}^{**}$	$\mathbf{-0.829}^{**}$	$\mathbf{-1.107}^{**}$	$\mathbf{-0.916}^{**}$	$\mathbf{-0.976}^{**}$
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 labour markets and two-digit sectors (whole 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 A3: **Descriptive Statistics on Selected Firms' Outcomes**

Variables	*Firms' averages over the years 2007-2015* **Mean**	**St.dev.**	**N. groups**
Log revenues	14.358	1.625	260,292
Log value added p.w.	10.902	0.563	260,292
Log contract's costs/revenues	-7.212	1.372	260,241
Log phys. capital/labour costs	4.326	1.874	259,019
Variables	*Differences between the 2013-15 and 2007-09 averages* **Mean**	**St.dev.**	**N. groups**
Δ Log revenues	-0.075	0.553	200,494
Δ Log physical capital	-0.104	1.129	197,872
Δ Log value added p.w.	-0.026	0.420	200,494
Δ Log phys. capital/labour costs	-0.175	1.145	197,870

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 selected periods.

Table A4: **Wage and Employment Effects of Pay Scales across Quintiles of Average Firm-Level Outcomes**

Firms' outcomes	Total Revenues	Value Added per Worker	Capital/ Labour Costs	Contract's Costs/ Revenues
Dependent variable	*Group's Avg. Log Wages*			
Coefficients:				
$\text{PS}_{ct} * q_{\hat{r}_g}(1)$	$\mathbf{0.438}^{**}$	$\mathbf{0.381}^{**}$	$\mathbf{0.464}^{**}$	$\mathbf{0.540}^{**}$
S.e.	*0.034*	*0.054*	*0.057*	*0.048*
$\text{PS}_{ct} * q_{\hat{r}_g}(2)$	$\mathbf{0.501}^{**}$	$\mathbf{0.507}^{**}$	$\mathbf{0.535}^{**}$	$\mathbf{0.612}^{**}$
S.e.	*0.029*	*0.043*	*0.037*	*0.047*
$\text{PS}_{ct} * q_{\hat{r}_g}(3)$	$\mathbf{0.484}^{**}$	$\mathbf{0.440}^{**}$	$\mathbf{0.583}^{**}$	$\mathbf{0.554}^{**}$
S.e.	*0.028*	*0.034*	*0.034*	*0.034*
$\text{PS}_{ct} * q_{\hat{r}_g}(4)$	$\mathbf{0.462}^{**}$	$\mathbf{0.544}^{**}$	$\mathbf{0.542}^{**}$	$\mathbf{0.483}^{**}$
S.e.	*0.028*	*0.034*	*0.031*	*0.032*
$\text{PS}_{ct} * q_{\hat{r}_g}(5)$	$\mathbf{0.547}^{**}$	$\mathbf{0.652}^{**}$	$\mathbf{0.482}^{**}$	$\mathbf{0.494}^{**}$
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:				
$\text{PS}_{ct} * q_{\hat{r}_g}(1)$	$\mathbf{-2.019}^{**}$	$\mathbf{-1.955}^{**}$	$\mathbf{-0.784}^{**}$	$\mathbf{-0.023}$
S.e.	*0.133*	*0.247*	*0.221*	*0.205*
$\text{PS}_{ct} * q_{\hat{r}_g}(2)$	$\mathbf{-1.335}^{**}$	$\mathbf{-1.018}^{**}$	$\mathbf{-0.287}$	$\mathbf{-0.294}$
S.e.	*0.130*	*0.169*	*0.162*	*0.189*
$\text{PS}_{ct} * q_{\hat{r}_g}(3)$	$\mathbf{-0.947}^{**}$	$\mathbf{-0.615}^{**}$	$\mathbf{-0.172}$	$\mathbf{-0.462}^{**}$
S.e.	*0.126*	*0.157*	*0.151*	*0.156*
$\text{PS}_{ct} * q_{\hat{r}_g}(4)$	$\mathbf{-0.411}^{**}$	$\mathbf{-0.205}$	$\mathbf{-0.467}^{**}$	$\mathbf{-0.897}^{**}$
S.e.	*0.133*	*0.157*	*0.151*	*0.229*
$\text{PS}_{ct} * q_{\hat{r}_g}(5)$	$\mathbf{-0.448}^{**}$	$\mathbf{0.143}$	$\mathbf{-1.233}^{**}$	$\mathbf{-0.627}^{**}$
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 A5: **Wage and Employment Effects of Pay Scales across Quintiles of Growth in Firm-Level Outcomes**

Firms' outcomes	Δ Value Added per Worker	Δ Capital/ Labour Costs
Dependent variable	*Group's Avg. Log Wages*	
Coefficients:		
$\text{PS}_{ct} * q_{\hat{r}_g}(1)$	**0.033**	**0.552****
S.e.	*0.046*	*0.047*
$\text{PS}_{ct} * q_{\hat{r}_g}(2)$	**0.319****	**0.615****
S.e.	*0.039*	*0.040*
$\text{PS}_{ct} * q_{\hat{r}_g}(3)$	**0.520****	**0.570****
S.e.	*0.037*	*0.036*
$\text{PS}_{ct} * q_{\hat{r}_g}(4)$	**0.709****	**0.495****
S.e.	*0.045*	*0.035*
$\text{PS}_{ct} * q_{\hat{r}_g}(5)$	**0.968****	**0.402****
S.e.	*0.037*	*0.067*
Adjusted R^2	0.825	0.825
RMSE	0.164	0.163
N. of observations	18.3M.	18.0M.
Dependent variable	*Group's Log FTE Employment Rate*	
Coefficients:		
$\text{PS}_{ct} * q_{\hat{r}_g}(1)$	**−0.181**	**0.042**
S.e.	*0.232*	*0.235*
$\text{PS}_{ct} * q_{\hat{r}_g}(2)$	**−0.614****	**−0.128**
S.e.	*0.208*	*0.193*
$\text{PS}_{ct} * q_{\hat{r}_g}(3)$	**−0.317**	**−0.351***
S.e.	*0.168*	*0.156*
$\text{PS}_{ct} * q_{\hat{r}_g}(4)$	**−0.924****	**−1.309****
S.e.	*0.195*	*0.185*
$\text{PS}_{ct} * q_{\hat{r}_g}(5)$	**−1.242****	**−1.087****
S.e.	*0.235*	*0.263*
Adjusted R^2	0.985	0.985
RMSE	0.293	0.292
N. of observations	18.3M.	18.0M.
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 A6: **Effects of Pay Scales on Employment - Robustness to Contract's Time Trends**

Dependent Variable	*Group's Log FTE Empl. Rate*	
Sample	*Whole INPS*	*INPS-AIDA*
Coefficients		
PS_{ct}	$\mathbf{-0.198}^{**}$	$\mathbf{-0.088}$
S.e.	*0.066*	*0.160*
Activity rate	-0.002^{*}	-0.016^{**}
S.e.	*0.001*	*0.003*
Unemployment	-0.015^{**}	-0.015^{**}
S.e.	*0.001*	*0.001*
Time Trends		
Contract	✓	✓
Fixed Effects		
Group	✓	✓
Time∗ISIC 22∗region	✓	✓
Adjusted R^2	0.978	0.985
RMSE	0.258	0.292
N. of observations	17.366M.	19.936M.

∗∗: 1%; ∗: 5% significance levels. Groups are defined by the interaction of collective contracts, local labour markets and two-digit sectors (whole 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).

B 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. Each sector and population group was typically subject to different collective agreements, which could had 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 labour demand elasticity.

Table B1 provides the sector-specific elasticities of average wages and employment to contractual pay levels. We have defined industries using the Isic rev. 4 eleven groups (or *high-level*) classification. Results in the left coefficients' column of Table B1 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, for what concerns other relatively large sectors, all of the estimates laid 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 labour contracts, through which employers can provide performance-related and additional pay components on top of contractual wages. Indeed, on one hand these top-up components can make the growth in actual wages different from the one set by collective bargaining, on the other dates of implementation in these compensation schemes are likely to interact with collective agreements' renewals, due to the reduced levels of uncertainty regarding baseline wages.[46] 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 model the number of policy effects to be estimated was higher -and the number of available contrasts for each parameter lower-

[46] Unfortunately, coordinated information on the economic content and dates of application of decentralized agreements is not available in the Italian context.

Table B1: **Wage and Employment Effects of Pay Scales across Industries**

Linear combinations of: PS_{ct} *and its industry interactions*	*Dep. Variable* Groups' avg. wages	Groups' FTE empl.	Weighted 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, transports & 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 service act.	**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 whole INPS sample. Groups are defined by the interaction of collective contracts, local labour 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.

Table B2: **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 labour 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 B3: **Wage and Employment Effects of Pay Scales across Local Business Cycle Fluctuations**

Linear combinations of: PS_{ct} *and its interactions with LLM unemployment growth indicators*	*Dep. Variable* Groups' avg. wages	Groups' FTE empl.	Weighted frequency
Negative yearly LLM unemployment growth	$\mathbf{0.482}^{**}$	$-\mathbf{0.270}^{**}$	63.7%
S.e.	*0.019*	*0.082*	
Positive yearly LLM unemployment growth	$\mathbf{0.483}^{**}$	$-\mathbf{0.267}^{**}$	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 whole INPS sample. Groups are defined by the interaction of collective contracts, local labour 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 labour market was higher or lower than in the previous year.

than in the baseline specification.

The right coefficients' column in Table B1 provides estimates of the elasticity of employment to contractual wages. The classical theory of labour demand suggests that this parameter should be smaller, the less price-elastic the product demand faced by firms (*e.g.* Hamermesh [1993]). Evidences broadly consistent with this hypothesis have been documented *e.g.* by Harasztosi and Lindner [2019], but our results are not completely consistent with this mechanism. For example, the sensitivity of employment to contractual wages was higher in what is considered the classical example of a non-tradeable sector (construction) than in manufacturing, which is a typically tradeable sector, although 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 also in some relatively insulated domestic markets. Moreover, tradeability is usually taken as a proxy for the presence of competitors not affected by higher costs related to contractual wages, which in our context could vary also depending on the market share of self-employed or on degree of homogeneity and coordination among collective agreements within a given sector.[47]

Establishing whether the heterogeneities emerging from Table B1 are more broadly consistent with other expected determinants of the labour demand elasticity is difficult. The complex way in which technical rates of substitution between labour and capital, the share of labour costs in production, investment costs, outsourcing opportunities and similar factors are combined across sectors make it difficult to draw conclusive evidences on the relative importance of each of these factors using only the aggregated analysis presented in Table B1. As mentioned, more precise characterizations of the interaction between the elasticity of labour demand and firm-level outcomes are presented in Section 6.

Table B2 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 an equivalent procedure to that applied in constructing the whole INPS sample. From the top part of the table, it can be noticed 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 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 B2 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

[47]Notice that in our context the relationship between product and labour markets' demand elasticities is not identified by changes in output at the industry-wide or regional level. Only residual contract-specific shocks in employment contribute to the estimation of our marginal effects.

institution. Instead, old employees and those with a high level of employment protection -two characteristics that often overlap in the Italian context- were not affected. This last evidence is consistent with cross-country evidences 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]).

Table B3 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 labour markets into groups where the unemployment rate was higher than the one observed in the previous year –which was the case for around one third of the local labour 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 whole INPS sample, excluding the first available year (2006). As can be noticed, differences in the results across local labour market conditions were negligible for what concerns both, the influence of contractual wages on pay levels and on employment.

C A Monte Carlo Experiment on Distributed Lags with Autocorrelated Policies

We have built a Monte Carlo experiment that allows to evaluate the performance of several distributed lags specifications when the independent variable providing dynamic effects on the outcome is a more or less persistent AR(1) process. The purpose of this section is to discuss some shortcomings of alternative modelling choices related to near-perfect multicollinearity and to illustrate the bias/precision trade-off arising in this context.

We have considered the following structural equation

$$y_{it} = \sum_{j=0}^{2} \beta_{t-j} x_{i(t-j)} + \alpha_i + \phi_t + \epsilon_{it} \tag{3}$$

where y_{it} is a continuous outcome for unit i at time t. We have set $\epsilon_{it} \sim \mathrm{N}(0, 0.5)$, $\alpha_i \sim \mathrm{N}(\mu_i, 0.005)$ with an unit-specific mean μ_i uniformly distributed in $[0, 3)$ and $\phi_t \sim \mathrm{N}(\mu_t, 0.01)$ with a time-specific mean μ_t uniformly distributed in $[-1, 4)$. The parameters β_{t-j} were defined as

$$\beta_t = -0.3 \qquad \beta_{t-1} = -0.1/\xi_{t-1} \qquad \beta_{t-2} = 0.1/\xi_{t-2}$$

where ξ_{t-1} (and similarly ξ_{t-2}) was the coefficient associated to x_{it} in the linear projection of $x_{i(t-1)}$ onto x_t, α_i and ϕ_t. This choice was made to let the bias in $\hat{\beta}_t$ estimated using a static OLS model tend to zero. We have defined x_{it} as the following AR(1) non-stationary process

$$x_{it} = x_{i(t-1)} + \pi(p) r_{it} \tag{4}$$

setting $r_{it} \sim \mathrm{N}(0.03, \sigma_r)$ and defining $\pi(p)$ as a binomial random variable with mean p, for which we have chosen a value $p \approx 0.065$. Thus, realizations of the (normal) random variable r_{it} affected x_{it} only in around 6.5% of the periods t. Finally, we have set $x_{i1} \sim \mathrm{N}(4, 0.1)$ and defined all other leads and lags of this variable using equation (4).

Using the above definitions, we have generated 200 random samples of N units i observed for 100 periods t, evaluating the performance of alternative regression models for different choices of N (total number of units i) and σ_r (different degrees of correlation between leads and lags of x_{it}). In particular, we have set N alternatively equal to 10,000, 100,000 and one million, while σ_r was set equal to either 0.01 or 1. When $\sigma_r = 0.01$, x_{it}'s average growth and the standard deviation of this growth were fairly close to those observed among contractual wages in our application (*i.e.* respectively around 0.0019 and 0.0078).

In a first experiment, we have evaluated the precision of the estimates $\hat{\beta}_t$ obtained from four regression models. An OLS distributed lags model (DL) specified as follows

$$y_{it} = \sum_{j=0}^{2} \beta_j x_{i(t-j)} + a_i + f_t + e_{it}$$

where a_i and f_t were unit- and time-fixed effects. We have estimated the above equation using also a first-degree polynomial Almon transformation (AL1) and a third-degree polynomial Almon transformation (AL3). As a fourth model, we have tested the following static OLS specification (SM)

$$y_{it} = \beta_t x_{it} + a_i + f_t + e_{it}$$

For all models, standard errors were computed clustering at the unit level. Moreover, we simulated a number of lags sufficient to estimate all static and dynamic regressions on the same sample size. Table C1 compares the performance of each regression model for different choices of the parameters σ_r and N. As diagnostic tests, we provide the average absolute bias, which measures the precision in the point estimates of β_t, the average standard error associated to $\hat{\beta}_t$, the coverage error probability (*i.e.* the probability that the 95% CI does not include β_t) and the probability that the 95% CI does not lie in a desired range (in particular, that its upper bound is above zero, given $\beta_t = -0.3$).

As can be noticed, for small sample sizes the bias of the estimates was always sizeable when $\sigma_r = 0.1$. Despite this poor performance, the coverage error probability was not much affected, but this was related to the generally large confidence intervals arising in this context. Instead, the probability that the confidence interval includes or is above zero was found as high as 93% in the smallest sample. These problems were consistently mitigated when the autocorrelation of x_t was lower (*i.e.* when $\sigma_r = 1$), apart from the coverage error probability,

Table C1: **Estimates and Inference for β_t**

Simulation parameters	σ_r / N	0.01 / 10,000	0.01 / 100,000	0.01 / 1,000,000	1 / 10,000	1 / 100,000
		Average absolute bias $\left\vert\hat{\beta}_t - \beta_t\right\vert$				
Estimation method	DL	0.501	0.169	0.049	0.016	0.005
	AL1	0.376	0.121	0.037	0.011	0.004
	AL3	0.501	0.169	0.049	0.016	0.005
	SM	0.120	0.040	0.013	0.004	0.001
		Average size of $se(\hat{\beta}_t)$				
Estimation method	DL	0.649	0.204	0.056	0.020	0.006
	AL1	0.461	0.145	0.046	0.014	0.004
	AL3	0.649	0.204	0.064	0.020	0.006
	SM	0.154	0.050	0.016	0.005	0.001
		Coverage error probability for β_t				
Estimation method	DL	4%	4%	4.5%	7%	4%
	AL1	6.5%	6.5%	3.5%	4%	5%
	AL3	4%	4%	4.5%	7%	4%
	SM	6%	6%	4.5%	4%	5%
		$\mathrm{P}\left\{\hat{\beta}_i + 1.96 * se(\hat{\beta}_i) > 0\right\}$				
Estimation method	DL	93%	64.5%	0%	0%	0%
	AL1	89%	48%	0%	0%	0%
	AL3	93%	64.5%	0%	0%	0%
	SM	55%	0%	0%	0%	0%

Table C2: **Estimates and Inference for the Irrelevant Parameters $\beta_{(t+1)}$ and $\beta_{(t-3)}$**

Simulation parameters	σ_r / N	0.01 / 10,000	0.01 / 100,000	0.01 / 1,000,000	1 / 10,000	1 / 100,000
		Average absolute bias $\left\vert\hat{\beta}_{t+i}\right\vert$ $i = 1, -3$				
Estimation method	DL	0.524	0.150	0.051	0.016	0.005
	AL1	0.242	0.113	0.096	0.095	0.094
	AL3	0.500	0.147	0.051	0.016	0.005
		Average size of $se(\hat{\beta}_{t+i})$ $i = 1, -3$				
Estimation method	DL	0.642	0.202	0.064	0.020	0.006
	AL1	0.259	0.081	0.026	0.008	0.002
	AL3	0.610	0.192	0.061	0.019	0.006
		Type II error probability for $\mathrm{H}_0 : \beta_{t+i} = 0$ $i = 1, -3$				
Estimation method	DL	5.5%	3%	5.5%	6%	3.5%
	AL1	36%	100%	100%	100%	100%
	AL3	6%	2.5%	6%	5%	6.5%

which, as mentioned, is also influenced by the size of the standard errors for β_t.

When comparing the performance of the alternative models, it can be noticed that the static model (SM) had a much less volatile behaviour than the alternative specifications. Given that we mechanically corrected for the omitted variable bias induced by the exclusion of $x_{(t-1)}$ and $x_{(t-2)}$ from the regression equation, the comparison across models provides a rather neat illustration of the risks in terms of loss of precision associated to estimating distributed lags. In general, the higher is the number of parameters to be estimated, the lower becomes their precision, a consideration that holds true also when comparing the low-dimensional AL1 model to the AL3 model. Finally, notice that the AL3 model converged to the OLS in this context, given that three data points (β_t, $\beta_{(t-1)}$ and $\beta_{(t-2)}$) can be exactly interpolated by a third degree polynomial, while the reduction in multicollinearity was not effective in this context (the AL3 model requires the estimation of four parameters).

Using the same Monte Carlo experiment, we have also compared the performance of the DL, AL1 and AL3 methods when two lags irrelevant in equation (3) $-x_{(t-3)}$ and $x_{(t+1)}-$ were included in the regressions. Table C2 provides the average absolute value of $\hat{\beta}_i$ for $i = -3, 1$, the average size of the associated standard errors, and the type II error probability for the test on the joint significance of these coefficients. In this case, the AL1 model suffered from mis-specification bias, while some marginal differences emerged also between the AL3 and DL models. Notice that the probability of rejecting the null hypothesis $\hat{\beta}_i = 0$ was not negligible in all of the simulations, irrespective of the estimation method. Moreover, this error did not appear to be monotonically reduced with the sample size. This suggests that using distributed lags to test the significance of potentially irrelevant anticipatory or long-run effects could result in the estimation of spurious, but statistically different from zero effects. Thus, similar exercises should be carried out testing the robustness of the results for several specifications and estimation methods.

D 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 agreement was covered by our sample. The list of contracts is presented separately for the *whole INPS* and the *INPS-AIDA* samples. The INPS contract code refers to the official classification number of the contract provided by the Italian Social Security Institute.[48] For each of these agreements, we have computed their relative size, measured as the proportion of total worker-months observations considered in the estimation sample that belonged to them.

Table D1: **Collective Agreements included in the Whole 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

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

Table D1 continuation

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
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

Continues next page

Table D1 continuation

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
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 D2: **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

Continues next page

Table D2 continuation

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
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

Continues next page

Table D2 continuation

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
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

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