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IZA DP No. 17670

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ABSTRACT

Rigid Yet Resilient: Firms' Margins of Adjustment to Demand Shocks in Regulated Labour Markets*

We investigate how firms adjust to demand shocks when wages and employment determination are regulated. Using firm-level data for the Italian metal engineering industry from 2009 to 2021, we estimate the elasticity of the wage bill to changes in firm's real sales. We disentangle the effect on wage components (base wage and wage cushion) and labour inputs (permanent or temporary employment and working hours). Results show that the elasticity of the wage bill to demand shocks mainly works through adjustment of working hours (especially via short-time work) and partly employment, while wages are less sensitive. Unions at the workplace reduce employment adjustment through a more intensive use of short-time work schemes. The lower employment adjustment to changes in sales in unionized firms does not depend on past investments or innovation, and it is associated to higher responsiveness of profits to declining sales only in weakly unionized firms.

JEL Classification: J30, J58, C81

Keywords: labour adjustment, product demand shock, short-time work, unions, collective bargaining

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1. Introduction

The 2008 economic crisis and the Covid-19 pandemic exposed most firms to unprecedented large and persistent product demand shocks. Firms' reaction has been quite heterogeneous between and within countries, with large employment adjustments. To explain firm's resilience to these shocks, and the differential impact across countries, the economic literature has renewed attention on the interactions between economic shocks and labour market institutions (Bassanini and Duval, 2009; Boeri and Jimeno, 2016), also investigating how the latter interact with firm-specific shocks thanks to the availability of company-level data (Bertola et al., 2012; Magruder, 2012; Fabiani et al., 2015; Marotzke et al., 2020). However, little is known about a firm's specific reaction under different firm-level bargaining arrangements and the presence of unions in the workplace (Addison, 2019).

In this paper, we fill this gap in the literature and investigate how firms adjust labour costs to demand shocks, as proxied by changes in real sales, focusing on the role of firm-level unions. Cutting labour costs is one of the main strategies that firms use to face negative product demand shocks, especially in highly competitive markets (Druant et al., 2012). Firms can, in principle, leverage a wide menu of margins to adjust labour costs, involving changes in different employment, wages or working hours dimensions. However, the actual menu is heavily influenced by the presence of unions within the firm. Strong unions typically resist employment adjustments, favouring working hours flexibility and short-time work schemes (Biancardi et al., 2022). They can also use their bargaining power to limit firm's ability to adjust wages, including variable components such as bonuses and non-monetary benefits that are usually more sensitive to changes in business conditions when nominal wage rigidity prevents cutting the base wage (Babecky et al., 2012 and Dias et al., 2013). Resistance to adjusting both wages and employment in highly unionized firms is likely to shift the burden of the adjustment costs onto the employer, for instance by reducing profit margins (Lee and Mas, 2012).

To address these unexplored issues, in our empirical analysis, we exploit unique firm-level panel data of metal engineering firms in Italy (from 2009 to 2021), providing detailed information on wages,

employment, as well as firm's performance indicators and industrial relations at the workplace. We analyse firms' margins of adjustments to firm-level changes in real sales. We first consider the total wage bill (as a proxy for labour costs), per-capita annual wages and total employment as outcome variables. We then investigate further the sensitivity of different wage (base wage and wage cushion) and employment components (permanent full-time, permanent part-time and temporary employment) to changes in sales. Given the extensive use of short-time work schemes during the recent crises (Arpaia et al, 2010; Boeri and Bruckner, 2011), we also consider adjustments in the number of per-capita hours worked and the number of hours of short-time work. We then exploit rich information available on firm-level union density and collective bargaining to investigate how they influence the choice of margins of adjustment in presence of nominal wage rigidity caused by multi-period sectoral bargaining (Adamopoulou et al., 2016; Adamopoulou and Villanueva, 2022; Card and Cardoso, 2022; Cardoso and Portela, 2009; Cardoso and Portugal, 2005; Dickens et al., 2007; Du Caju et al., 2015; Messina et al., 2010). Finally, since in an industry open to international trade, such as the metal engineering sector, increases in labour (and capital) costs cannot be easily passed onto prices, we investigate whether there is any effect on other margins, such as firm's profitability (Bertola et al. 2012).

To account for the endogeneity of changes in real sales, we estimate Fixed-Effects Instrumental Variables (FE-IV) models, using as instrument industry-wide sales shocks weighted with pre-determined firm's market shares. Identification relies on the assumption that differential exposure to a common shock should differently affect outcome variables at the firm level.

We find that the total wage bill is sensitive to changes in firm's real sales, though the estimated elasticity is rather small: a 10% increase in sales determines an increase in the wage bill of 2.3%, which is almost entirely driven by changes in working hours through the use of short-time work schemes and employment changes, while wages remain largely unaffected. The estimated elasticity of temporary employment is significantly higher than that of permanent full-time employment (0.66 and 0.14 respectively), but given the prevalence of full-time permanent employment in the metal-

engineering industry, much of the employment adjustment to changes in sales is driven by changes in the number of full-time permanent employees. When we consider different wage components, we find a significant but small elasticity only for the base wage and in the case of positive sales shocks, confirming the role of nominal wage rigidity in preventing downward wage adjustment. One explanation for the lack of statistical relevance for the "wage cushion" component may be related to the fact that often, upon the renewal of a collective agreement, part of the variable component of pay is "absorbed" in the "base wage" component. This feature may vary according to the bargaining power of negotiating parties, as well as the collective contract being renewed.

Estimates by type of shock confirm that firms react with a richer menu of margins of adjustment to positive relative to negative shocks. An increase in real sales causes changes in employment, working hours and base wages, while only employment significantly reacts to declining sales. The lack of significant adjustments of short-time work hours to declining sales may be due to the institutional changes in the use of short-time work schemes implemented during the Covid pandemic, when eligibility was extended to all firms reporting any reduction in work activity, regardless of its effect on actual sales conditions.

The estimated adjustment in permanent employment to changes in real sales is much lower in unionized firms compared with firms in which unions are not present. We show that labour input adjustment to changes in sales in unionized firms occurs mainly along the intensive margin, *via* short-time work schemes. Simple back-of-the-envelope calculations show that, without short-time work, total employment reduction in unionized firms would have been at least twice as high as that estimated with our model, and closer to that registered in comparable non-unionized firms. We also show that the lower employment adjustment to changes in sales detected in unionized firms does not depend on past investments or innovation, and it is associated with higher responsiveness of profits to declining sales only in weakly unionized firms.

Our paper contributes to the literature on firm adjustments to demand shocks that has regained momentum after the 2008 economic crisis.

Much of the recent research on this topic in Europe uses firm-level cross-section survey data and qualitative information based on self-reported answers from the Wage Dynamics Network project (WDN).[2] Studies based on the WDN data confirm that downward nominal wage rigidity is a relevant phenomenon, which forced most European firms to adjust more employment than wages to cut labour costs during the 2008 crisis (Fabiani et al., 2015; Marotzke et al., 2020). However, the qualitative nature of the questions in the WDN survey does not enable to assess the size of the elasticity of different wage components, employment and hours, as we do in this paper.

A second strand of literature provides more quantitative evidence, especially concerning wage adjustment, based on detailed information on individual wages from payroll records (Elsby and Solon, 2019; Kurmann and McEntarfer, 2019; Jardim et al., 2019). Overall, these studies confirm that nominal wage flexibility is more common in the US compared to Europe, but the base wage in the US is as rigid as in most European countries, showing that wage adjustments also in the US occur mainly through changes in variable wage components or adjustment in working hours (Grigsby et al., 2021). Research based on personnel records provides accurate estimates on wage, employment and hours adjustments, but they are a sort of "black box" for what concerns other firm characteristics, such as productivity or profits, work organization and industrial relations.

Finally, a third strand of literature focuses on wage adjustment as a mechanism of insurance and tests the elasticity of wages to revenues or value-added shocks using matched employer-employee data for a number of countries (see, among others, Guiso et al., 2005 on Italy; Card et al., 2016 on Portugal; Juhn et al., 2018 on the US). Most of these studies find positive but small elasticities, suggesting the lack of workers' full insurance, especially against permanent firm-level shocks, but the transmission of the latter to workers through lower wages is rather limited.

[2] The WDN project was promoted by 25 National Central Banks of euro and non-euro area EU Member States. WDN carried out three firm-level surveys (in 2007, 2009 and 2014) with information on labour market adjustments following the 2007 Great Recession. For further details on WDN, see https://www.ecb.europa.eu/pub/economic-research/research-networks/html/researcher_wdn.en.html

Our data allow us to uncover the mechanisms behind firm's adjustment to demand shocks, pointing to the mediating role of unions in influencing the menu of margins of adjustment.

The remainder of the paper is organized as follows. In Section 2 we discuss the main institutional features influencing labour costs adjustments (in terms of wages, employment and working hours) in Italy. In Section 3 we present the dataset, the main variables of interest and some preliminary descriptive evidence. In Section 4 we discuss the empirical strategy, while the main econometric estimates are reported in Section 5. In Section 6 we delve into the role of local unions and firm-level bargaining in influencing the menu of margins of adjustments. Further estimates and robustness checks are reported in Section 7. The last Section concludes.

2. The institutional setting

Italy does not rank among the OECD countries with the strictest labour market regulation; nonetheless, labour utilization and reallocation are hindered by a rigid wage bargaining system coupled with asymmetric employment protection regulation between permanent and temporary contracts (Schindler, 2009).

Collective bargaining in Italy operates within a relatively weak legal regulation and is centred around the role of the most representative employers and workers' organizations. The main labour law, the so-called *Statuto dei lavoratori* of 1970[3], voluntarily did not regulate industrial relations, leaving social partners responsible for setting the rules through collective bargaining. Although information on union membership and employers' associations in Italy is rather uncertain, recent available estimates set union membership, in the private sector, around 30-40%, and employers' associations close to 50% (Visser, 2019). Since the first half of the 1990s, collective wage bargaining has been organized in a two-tier system: base wages are set in industry-level collective agreements (*Contratto Collettivo Nazionale di Lavoro, CCNL*) and indexed to expected inflation with the aim to preserve

[3] The Italian *Statuto dei lavoratori* has been in force for more than 50 years, only partially modified over time in some respects.

purchasing power, while additional wage components linked to specific indicators of productivity, profitability or other measures of firm performance can be bargained at the firm or local level. A collective agreement is in general binding only for the partners who sign the contract, as there are no formal extension mechanisms to other firms or workers. However, a surrogate of an *erga omnes* extension exists, as Labour Courts take wage levels set in collective agreements as a reference for the application of Art. 36 of the Italian Constitution (e.g. stating that workers have the right to a 'fair wage'). This second tier of collective bargaining has always been subordinated to the national level, and it is subject to the *in melius* or favorability principle: that is, wages and working conditions cannot be worse than those agreed at the sector level. Furthermore, since collective agreements cannot be typically derogated, they were blamed for causing excessive wage rigidity during the 2008 economic crisis (IMF 2016). New rules were then introduced, allowing firms in economic distress to temporarily opt-out from their industry-level collective agreement - though wages have been excluded from the issues that could be derogated. In this setting, even if industry-level base wage cannot be adjusted downward, the overall responsiveness of wages can potentially count on the adjustment of the flexible wage components linked to firm's productivity or other performance indicators (e.g. wage cushion) in companies with a firm-level agreement. Depending on the collective contract applied by the firm and the specific agreement with the employee, some components of the wage cushion can also be reduced and "absorbed" in the base wage when the firm has to increase the latter after the renewal of the industry-wide agreement.

Despite the progressive diffusion of collectively negotiated performance-related-pay schemes, their actual incidence on the total wage is rather small (e.g. close to 5-6% of the total gross wage; Casadio, 2003; Brandolini et al., 2007; Lucifora and Origo 2015). Furthermore, firm-level bargaining is still largely confined to the largest firms and in the Northern regions (D'Amuri and Giorgiantonio, 2014).[4]

[4] Available data from the Survey of Industrial and Service Firms (INVIND) by the Bank of Italy suggest that in 2010 only 21 percent of firms had some form of second-level agreement.

Workers with an open-ended contract also enjoy considerable employment protection due to a combination of severance payment and reinstatement rights in case of labour disputes which makes firing extremely costly for firms (Sestito and Viviano, 2018). A 2012 reform (known as the "Fornero Law") and the "Jobs Act" introduced in 2015 changed the firing rules for new hires in firms with more than 15 employees (that is, those subject to stricter Employment Protection Regulation since the 1970 *Statuto dei lavoratori*), allowing employers greater discretion in workforce reduction to favor youth employment and increase labour market overall flexibility, also extending passive and active policies.

In a context of nominal wage rigidity and strict employment protection of permanent contracts, working time flexibility, especially under the form of short-time work schemes, has been extensively used by firms to adjust employment to negative demand shocks.[5] Particularly in the years following the 2008-2009 economic crisis and in the current Covid-19 pandemic crisis, Italy features as one of the EU Member States with the highest share of workers on short-time work schemes (Eurofound, 2010 and 2020), and the country with the most generous scheme (corresponding to 80% of the previous gross earnings and lasting up to three years). Furthermore, before the adoption of the "Jobs Act" in 2015, this benefit was significantly higher than ordinary unemployment benefits and hence it was very attractive for both employers and workers. Finally, in 2009 and especially during the Covid pandemic, its coverage was temporarily extended to firms and workers previously excluded (Arpaia et al, 2010; Giupponi and Landais, 2023). A distinctive feature of short-time work during the Covid period (the so-called "*Cassa Covid*") is that firms could use it regardless of their business conditions,

[5] *Cassa Integrazione Guadagni* (CIG) is the main short-time work scheme in Italy. CIG is made of three main programmes: Ordinary CIG (*CIG ordinaria*), Extraordinary CIG (*CIG straordinaria*) and Derogatory CIG (*CIG in deroga*). The Ordinary CIG is used in case of product demand declines in manufacturing companies due to temporary events that cannot be ascribed to the company, such as adverse weather conditions. The maximum duration is 13 weeks, which can be extended up to 52 weeks. The Extraordinary CIG is used in case of business crisis or restructuring by manufacturing companies with more than 15 employees (or 50 employees in services sectors). Derogatory CIG was introduced in 2009 to cover firms and workers (such as small firms and temporary workers) not covered by the previous two schemes. In all these schemes, public subsidy covers 80% of forgone earnings up to a threshold (the highest benefit amounts to around 1200 Euros). The use of these schemes has been further potentiated and extended during the COVID-19 crisis. For more institutional details, see: https://www.eurofound.europa.eu/it/observatories/emcc/erm/support-instrument/short-time-allowances-ordinary-wages-guarantee-fund-cigo-and-extraordinary-wages-guarantee-fund-cigs#

as long as they reported a halt or reduction in work activity due to the pandemic.[6] Using data for 20 EU Member States from the third wave of the WDN survey, Lydon et al. (2018) find that the take-up rate of short-time work schemes is higher in firms operating in countries with stringent Employment Protection Legislation or in sectors where wages are more rigid. Hence, short-time work schemes appear as a relevant margin of adjustment, especially where both wage and employment are difficult to adjust due to institutional constraints.

The current debate in Italy is still centred around the role of collective bargaining and wage rigidity in preventing labour reallocation, particularly in times of economic crises when the need for wage adjustments is higher. Moreover, given the existing large productivity differentials across firms and regions in Italy, another issue hotly debated is whether industry-level collective bargaining, by compressing the wage distribution, might be another source of inefficiency due to biased incentives for worker job mobility, factors misallocation, higher unemployment and lower resilience across regions (Boeri et al. 2021). This paper contributes to the above debate providing an empirical framework to analyse the different margins of adjustment and firms' reaction to product demand shocks.

3. Data and descriptive statistics

Data and sample selection

The empirical analysis is based on a unique firm-level panel dataset combining detailed survey information with balance sheet data for a representative sample of metal engineering firms in Italy. This industry gathers quite heterogeneous sub-industries (including, for example, foundries, the automotive industry and IT-producing companies), which greatly differ by size, technology, export intensity and other firm characteristics. Furthermore, it accounts for almost 40% of the firms and

[6] Employers' contribution to the scheme was temporarily suspended. Firms were asked to provide formal evidence of a severe sales reduction (larger than 20% on an annual basis), for example trough balance sheet data, only if they required an extension of the subsidy. For more details, see OECD (2020).

employment in manufacturing in Italy and 50% of total exports in the Italian economy. Most importantly, it is the leading sector for collective bargaining in Italy and its national agreement represents a benchmark for collective bargaining in other industries (Lucifora and Origo, 2015). Although the focus on a single industry may in principle limit the external validity of the results, the size, heterogeneity and institutional features of this industry make it an interesting context to study firms' reaction to output changes under different conditions concerning unions and industrial relations at the workplace.

The survey is carried out by the main national employers' association of this industry, with the aim to collect information on issues related to the labour market, firm-level bargaining and industrial relations. It is run every year since 2009; for our analysis, we could access data from 2009 to 2021. On average, approximately 1,300 firms employing around 225,000 workers are surveyed each year, corresponding to almost one fifth of the employees in this industry. Overall, around 6,000 different firms took part in the survey in at least one of the years considered, and more than two thirds of them participated at least twice, providing an unbalanced panel over the period considered. The survey reports information for each firm on employment levels, composition and changes (with some information by skill, gender, education, and type of contract); working hours and absenteeism; wage levels and composition by skill (*qualifica*) and job title (*livello di inquadramento*); firm-level bargaining and industrial relations.[7]

We merged the survey data, using a unique firm identifier, with balance-sheet data drawn from AIDA dataset (*Analisi Informatizzata delle Aziende Italiane* - Computerized Analysis of Italian Firms) for the 2006-2021 period.[8] With this procedure, we successfully merged information for 3,131 different firms, corresponding to around 47% of the firms in the initial sample. To select the final sample used

[7] In specific waves, there are also additional questions on specific policies related to human resources management or labour market reforms implemented over the period covered by the survey.

[8] The AIDA database is updated and distributed by Bureau van Dijk and it contains the financial statements of all the active and bankrupt Italian companies (excluding banks, insurance companies and public bodies). AIDA is the main Italian source feeding Amadeus, the international Bureau van Dijk's dataset containing similar comparable information on public and private companies across Europe.

in the empirical analysis, we dropped observations with missing or negative values for the main variables of interest (sales, wage components, and employment), and trimmed each wage component dropping observations below and above the 1st and 99th percentile of the corresponding distribution. The final sample consists of around 2,300 firms, corresponding to almost 75% of the merged sample. Given the survey nature of our data and the relatively long period considered, attrition bias may be a reason for concern. Firms may exit the sample and eventually re-enter due to the random sampling or exit permanently when closing down due to a negative shock. Inspecting our sample, we find that around 18% of the firms exit permanently at some point in our time window, while the great majority of the firms (around 80%) exit for one or more years and then re-enter the sample. The latter are likely to be firms that are randomly selected in the sample, while their absence is probably not because they closed down their business. A simple test to see whether firm-level attrition should be a matter of concern in our sample, is to investigate the correlation between changes in sales and the probability of a firm exit (permanently or temporarily) from the sample. We test this hypothesis in the robustness checks Section, using permanent versus temporary exits as dependent variables.

Main variables of interest and descriptive evidence

The empirical analysis investigates firms' margins of adjustment to product demand shocks, the latter defined as changes in firm-level sales at 2015 prices. We deflated accounting nominal sales using production price indexes computed at the fourth digit of industry classification. Trends in real sales of the metal-engineering industry mirror the Italian business cycle after the 2008-2009 economic crisis, characterized by a short recovery in 2010-2011, followed by a decline in sales in 2012-2013 (corresponding to the "double-dip" recession caused by the sovereign debt crisis) and the subsequent recovery since 2014. The recovery was abruptly stopped by the Covid-19 pandemic, which caused a significant decline in sales in 2020, averaging around 20%. Such shock was severe but temporary, with sales substantially increasing and recovering the pre-Covid levels in 2021.

Regarding wages of full-time permanent employees, for each job title, the survey provides information on the total gross monthly wage, annual collective performance-related pay, and other annual bonuses. The survey also reports detailed information on two main components of the gross monthly wage: seniority premia and individual monthly premia (the so-called "super-minima" that are paid on top of the collectively negotiated wage levels). Exploiting this information, we compute the monthly 'base wage' by subtracting these two components from the gross monthly wage. Hence the 'base wage' includes both the wage levels set in industry-level agreements by job title ("*livelli minimi di inquadramento*"), as well as other monthly wage components (fixed or variable) different from seniority premia or the super-minima.[9] This is a considerable advantage compared to household and administrative datasets, which usually do not contain details about the different components of total pay. This is a crucial issue, especially when firms and workers are interested in long-term employment relationships. In this context, it is not the spot wage of new hires that matters, but rather the user cost of labour defined as the expected present value of costs to the firm associated with a new hire in current period, compared to wait and hire the worker in the following period (Kudlyak, 2014). Grigsby et al (2021) show that base wages are a better proxy of the user cost of labour relative to measures of compensation inclusive of bonuses.

For our analysis, we compute the firm-level annual gross wage by adding up the 'base pay', the variable pay, and annual bonuses. To gain insights into the role of different pay components, we also separately consider the monthly base wage and the annual wage cushion.[10] On average, over the period considered, the latter represents around 23% of the total annual gross wage and its share has

[9] While the survey does not provide direct information on the collectively negotiated wages set, at the national level, in industry-wide agreements, nor information on the specific national agreement (*CCNL*) applied by each firm, still the 'base wage' should largely correspond to the negotiated wage levels set by national collective bargaining. To a smaller extent the 'base wage' may also include other fixed components that firms decide to include as part of the wage increase granted in national collective bargaining.

[10] In practice, we compute total annual gross wage by skill and job title as follows: monthly wage*13 + bargained performance-related pay + other annual bonuses. The corresponding firm-level wage is computed using as weights the distribution of full-time permanent employees. The wage cushion is computed as the difference between the total wage and (annual) base wage. Variable pay components include different wage premia set by the employer or negotiated with the employee at the time of employment, or other skill-specific components defined by the employment contract.

progressively increased from about one fifth, in 2009, to around one fourth of the total wage, by 2021. Firm-level collective performance-related pay increased, as a share over the wage cushion, from 7% to 10%, still accounting for around 3% of the total annual wage.[11]

Finally, we compute the total wage bill as the sum of the products between the average wage and total number of employees by skill.

Concerning firm's employment levels and composition, we distinguish between temporary and permanent employees and, within the latter, between full-time and part-time workers. Information on employment by type of contract is relevant because firms may use temporary employment as a buffer stock to cope with changes in product demand, especially in the presence of high firing costs of permanent workers caused by strict employment protection legislation (Bertola et al., 1999; Kugler and Pica, 2008). Furthermore, resorting to part-time contracts has been used by firms during the 2008 crisis to prevent excessive employment cuts (Horemans et al., 2016). It should be noted that both temporary and part-time employment, in the metal engineering industry, represent only a small share of total employment (around 5% of the total workforce for each type of atypical contract).

Finally, detailed information on contractual weekly hours, annual hours of short-time work schemes and absenteeism are also available, allowing to compute two accurate measures of annual working hours per employee, where the difference between the two is given by the exclusion of short-time work schemes per employee (see Appendix I). Given the relevance of short-time work schemes in the Italian context, we also consider the total number of hours of short-time work schemes used by the firm as a further margin of adjustment to demand shocks.

Table 1 reports the main summary statistics. The average firm employs 181 employees, but the number of part-time and temporary workers is rather small (6.9 and 5.2 respectively). Per-capita annual gross wage is around 30 thousand euros, and around one fifth is made of the wage cushion (6819 euros). On average each firm used almost 16 thousand hours of short time work in a year,

[11] This is despite the fact that one of the aims of the 1993 reform of the wage bargaining system was to increase the adoption of collective variable bonuses linked to indicators of firm performance.

corresponding to about 87 hours per worker. Around 24% of the workforce is unionized, and almost half of the firms have a firm-level agreement. Compared to the initial sample, these firms employ more employees, use more short-time work and are more unionized. They also pay lower wages, but the difference is small (around 2% of total wage) and not statistically significant for the wage cushion. Differences in observable characteristics are smaller when we compare our final sample with the one merged with balance sheet data (see Table A1 in Appendix).[12]

(TABLE 1 AROUND HERE)

As first descriptive evidence on the variation of the different margins of adjustment, Figure 1 plots percentage annual changes in base wage, wage cushion, total employment and per-capita working hours (net of short-time work hours) by firm and year. In each panel, the dashed black line indicates the zero change, while for the two wage components, the dashed green line indicates the expected inflation rate defined in the industry collective agreement, which may be considered the threshold for real wage rigidity. However, since the years of the analysis are characterized by very low inflation, it is difficult to statistically distinguish between nominal and real wage rigidities (Adamopoulou et al. 2016). Hence, we shall interpret any spike between 0 and the inflation rate as a signal of wage rigidity, without distinguishing between nominal and real one.

The two upper panels of the figure confirm that wage rigidity is relevant mainly when we consider the base wage, given the mass of the distribution between zero and the inflation rate. Such mass is less evident when we consider the wage cushion, which is characterized by a longer and thicker left tail compared to the other wage component.

On the whole, descriptive evidence suggests that the rigidity of total annual wage is due to the rigidity of the base wage, which on average accounts for more than three quarters of total wage, but with great variability across firms (ranging from 60% at the 1st percentile to 100% at the 99th one).

[12] Differences in industry composition largely explain differences across samples, especially in the case of short-time work hours. If we control for industry fixed effects, the estimated differences shrink substantially (2339 hours between the final and the initial sample, 709 hours between the final and the merged sample) and are not statistically significant.

The two bottom panels show also in the case of total employment a mass at zero, implying some rigidity also in terms of employment adjustment. Nonetheless, the overall distribution looks less skewed than those reported for wages. Much more variation emerges when we consider annual per-capita working hours, whose distribution is characterized by a relatively long right tail driven by the reduction in the use of short-time work during the recent recovery years.

(FIGURE 1 AROUND HERE)

4. Empirical strategy

To estimate the elasticity of the wage bill to demand shocks, we specify the following baseline model:

$$\log (Y)_{it} = \alpha + \beta_1 \log (sales)_{it} + \tau_{kt} + \mu_i + \varepsilon_{it} \qquad [1]$$

where Y_{it} represents the wage bill, wage or labour input indicators, as previously discussed, in firm i at time t, τ_{kt} are 2-digit industry-specific time fixed effects, μ_i are firm fixed effects and ε_{it} is the error term.[13] The coefficient of interest is β_1, which measures the elasticity of the margin of adjustment considered to changes in real sales. The baseline specification is parsimonious in the number of covariates included, but we check the sensitivity of our main results to the inclusion of firm-level time-varying controls.

Given the survey nature of the data covering a rather long time period, in our time window (2009-2021), around one quarter of the firms are observed for two years, 42% between 3 and 5 years, 25% between 6 and 8 years and the remaining 12% between 9 and 13 years. Since we use an unbalanced panel dataset, we estimate the model in levels rather than in first differences because the latter may significantly reduce sample size and exacerbate measurement error in the independent variables, introducing bias in the coefficients (Pozzi and Schivardi, 2016). In Section 7, we show some robustness checks about the role of firm exit from the sample and potential attrition bias.

[13] We compute log(Y+1) for the few variables that contain zeros.

Identification of β_1 as a causal effect requires conditional exogeneity of real sales. In our specification, firm fixed effects control for unobserved time invariant firm characteristics correlated with both demand shocks and firm's margins of adjustment, while industry-specific time fixed effects control for time varying common shocks at the industry level. Since other sources of endogeneity may be relevant, such as reverse causality (for example, in a production function framework, changes in employment reflect into changes in output) or firm-level time-varying unobserved factors (such as a new board of directors that simultaneously changes sales, hiring and compensation policies)[14], we adopt an IV estimator using as instrument the pre-determined firm's market shares (defined at the 4-digit industry level and measured before the time period covered by our analysis) interacted with the "leave-one-out" industry sales.

More specifically, the instrument is defined as follows:

$$\text{IV log}\,(sales)_{it} = share_{ij=2007} * \log\,(sales_{j-i,t}) \qquad [2]$$

where $share_{ij=2007}$ is the market share of the i-th firm in industry j in the pre-estimation period (i.e. 2007) and $sales_{j-i,t}$ are total real sales in industry j at time t excluding sales of the i-th firm (the so-called "leave-one-out" instrument, Borusyak et al., 2022).[15] Since we use a Fixed Effects estimator, identification in the first stage comes from deviation of industry sales from the corresponding (long-term) average. Differently from standard shift-share instruments, where identification relies on the exogeneity of pre-determined market shares (Goldsmith-Pinkham et al, 2020), in our model specification with firm fixed effects, identification relies on the time-varying component of "leave-one-out" industry sales (that is our measure of an exogenous industry-wide shock), and we use time-invariant initial market shares to weigh the exposure of different firms to a common industry-wide

[14] The issue of reverse causality may be less relevant in the years of the crisis, when severe demand shocks were initially driven by external demand and were further exacerbated by credit crunch, especially in smaller firms. These shocks fell disproportionally strongly on manufacturing firms (Fabiani et al 2015).

[15] We can compute 2007 market shares because balance sheet data from AIDA are available since 2006, while survey data from Federmeccanica start in 2009.

shock. We then assume that differential exposure to a common shock should differently affect outcome variables at the firm level.

In our preferred specification, instrument validity requires exogeneity of the industry-wide shock conditional on firm and industry-specific time fixed effects. This implies that each firm should be relatively small compared to the whole industry, so that each single firm should not be able to influence overall sales of the remaining firms in the same industry. Indeed, in our sample, the average share of the firm's sales is around 7.5% of total industry sales, and the share at the 90th percentile is 17.5%.[16]

5. Baseline results

As a first step, we estimate the elasticity of the annual wage bill and its main components (i.e. per-capita annual wage and total employment) to changes in real sales. The main estimates are reported in Table 2, where the various columns refer to the different estimation methods -- fixed effects (FE) or Instrumental Variables (IV-FE), respectively -- and the dependent variable used: total wage bill (columns 1 and 2), per-capita annual wage (columns 3 and 4) and total employment (columns 5 and 6).[17] Standard errors are clustered at the firm level[18]. The estimated elasticities show that the total wage bill is significantly correlated with changes in real sales, but the size of the elasticity is rather small, even accounting for the potential endogeneity of real sales: IV estimates show that a 10% increase in sales causes an increase in the wage bill of approximately 2.3%, slightly higher than the

[16] In the case of wages, instrument exogeneity may be threaten if unobservable firm-level shocks could influence the negotiated wage at the industry level. This should not be an issue because the timing of renewal of industry-wide agreements is set by law (every two years for wages until 2009, every three years since then). It may be the case that renewal of the contract is subject to some delay, mainly due to the lack of an agreement on the wage increase between unions and employer association at the industry level. However, over the period considered, the leading contract in the metal engineering industry has always been signed regularly (in October 2009, December 2012, November 2016 and February 2021), with the longest delay due to the Covid pandemic.

[17] Table A2 in Appendix reports first stage estimates. The F statistics confirms the relevance of the instruments.

[18] Since the number of observations varies across clusters, and in the second stage we estimate also some specifications with interaction terms between the first-stage predicted sales and other exogenous variables (such as the presence of unions or firm-level bargaining), we use the wild bootstrap procedure proposed by Cameron et al. (2008) to compute the clustered standard errors in the second stage. We obtain similar results, available upon request, even if we cluster standard errors at 4-digit industry level.

corresponding FE estimate (2%). Quite interestingly, when we look separately at the two main components of the wage bill, we find that wages are largely unaffected by changes in sales, while total employment appears more responsive: based on IV estimates, a 10% increase in sales is associated to around 0.6% increase in wages (and is not statistically significant), 1.3% increase in total employment.[19] Considering the average firm, this corresponds to roughly 2.5 employees.

Although based on different data and estimation strategies, our results on wages are very similar to those found in the literature on wage insurance. For the US, Juhn et al. (2018) also report IV estimates that are much larger than OLS estimates (the estimated elasticities for the manufacturing sector to a 1-year change in revenues, which is the most comparable case to our sample, are 0.023 and 0.007 respectively). In the case of Italy for the 1981-1994 period, Guiso et al. (2005) find that a 10% increase in value-added causes a variation of 0.7% in earnings.

Overall, our results confirm that firms partly adjust labour costs when facing a demand shock and that, given the substantial rigidity of wages, the adjustment falls onto employment and other factors not fully captured by these aggregate variables.

(TABLE 2 AROUND HERE)

One explanation for the limited adjustment of total wages to changes in sales may be related to the high incidence of the base wage, which is largely defined by collectively negotiated minimum wages set in multi-period industry-wide agreements (renewed every three years). However, other pay components should be more responsive to changes in demand, such as collective performance-related pay, which are typically set in firm-level agreements and are aligned to indicators of firm's performance. Similarly, the relatively small estimated elasticity of total employment may hide heterogeneous effects across different types of employment contracts. For example, much of the employment adjustment may occur through the use of temporary employment or changes in working hours acting as a buffer stock.

[19] IV estimates are significantly larger than OLS estimates in the case of wages, possibly due to attenuation bias caused by measurement error in this variable.

In order to test the elasticity of specific wage and employment components, we estimated equation [1] separately for the base wage and the wage cushion; while for total employment, we differentiate between extensive (permanent full-time, permanent part-time and temporary workers) and intensive labour margins (per-capita annual contractual working hours and hours of short-time work).

Table 3 reports the main coefficients of interest estimated with the IV-FE estimator. Figures in the table reveal a statistically significant albeit small elasticity only for the base wage (a 10% increase in sales increases the base wage by 0,8%), while a negative but no significant effect is found for the wage cushion. These results are consistent with the "absorption" effect previously discussed, that is the shift of some components from the wage cushion to the base wage (i.e., absorption of variable components in the base wage) that some firms apply upon renewal of the industry-wide agreement.

Regarding different employment contracts, the estimated elasticity for temporary employment is larger compared to that for permanent employment (0.66 and 0.14, respectively). However, since temporary contracts account for only a small share of total employment in the metal-engineering industry, changes in total employment are primarily driven by changes in the number of full-time permanent employees. The estimated elasticity for permanent full-time employment is indeed very close in magnitude to that estimated for total employment.

As discussed in Section 2, short-time work was one of the main instruments used by large manufacturing firms to cope with the 2008-2009 crisis, as well as during the early phase of the Covid-19 pandemic crisis. In light of these features, it is crucial to consider also changes in total working hours, especially in short-time work hours, to get a full picture of the margins of adjustments that firms can use to react to demand shocks. Results in the last two columns of Table 3 clearly show that working hours set in collective agreements are rather insensitive to demand shocks. Conversely, total short-time work hours display a much larger elasticity: a one per cent increase in real sales causes a 3.3% decline in total hours of short-time work. The latter, in line with the institutional context previously discussed, proves to be the most reactive margin of adjustment to changes in real sales.

(TABLE 3 AROUND HERE)

Since our period of analysis covers two severe though quite different crises, we test whether the elasticity to sales of the different margins of adjustment changes over time. Specifically, we consider three different sub-periods: the Great Recession (2009-2014)[20], the recovery (2015-2019) and the Covid Period (2020-21). Overall, we do not find any significant change in the estimated elasticity in the two periods of crisis compared to the recovery one (see Table A3 in the Appendix).

As a further step of the analysis, we test the existence of asymmetries in the adjustment to, respectively, increasing and declining sales. In the presence of downward nominal wage rigidity, it may be the case that firms cannot reduce wages as they would like when they face a negative shock, but in principle, they can fully adjust in case of a positive shock (Snell et al., 2018; Marotzke et al., 2020).[21]

Estimates in Table 4 show that the adjustment of the wage bill to positive shocks occurs through changes in employment, working hours (via a reduction in short-time work), and base wages, although the elasticity of the latter remains relatively small compared to that of employment. Conversely, firms respond to declining sales primarily by reducing permanent employment. The asymmetric adjustment of wages highlights the relevance of downward nominal wage rigidity. Quite surprisingly, the estimated elasticity of short-time work hours to a negative shock is not statistically significant. As previously discussed, this result may be driven by the extension, during the Covid period, of short-time work schemes to all firms reporting any reduction in work activity, for example during strict lockdown periods, regardless of real sales reductions. If we exclude the Covid period our results do not change substantially, except for a lower elasticity of short-time work to a positive shock.[22] However, this may be due to the role played by unions in influencing the design of the

[20] This includes the second dip caused by the 2011 Sovereign debt crisis.

[21] Wage rigidity caused by collective bargaining has progressively caused a declining coverage of industry-wide agreements and the diffusion of pirate agreements (Garnero, 2018; Garnero and Lucifora, 2022). However, these phenomena should be less relevant for the sample of firms used in our analysis, since they are affiliated to the main national employer association of the metal-engineering sector and hence they should comply with the industry-wide collective agreement.

[22] Estimates are available upon request.

scheme and the type of workers involved (Biancardi et al., 2022). We shall reconsider this issue in the next Section.

(TABLE 4 AROUND HERE)

6. The role of unions in the workplace

The results shown so far provide interesting insights on how firms rely on the different margins of labour costs adjustment to cope with changes in sales, but they do not explicitly consider that firms' behaviour may be influenced by specific institutional constraints. As discussed in the introductory Section, both union power and collective bargaining at the firm level are likely to play a crucial role in determining the actual menu of margins of adjustment. To preserve employment and purchasing power of the incumbent (i.e., full-time permanent) workers they represent, strong unions are more likely to support working time flexibility, also through an extensive use of short-time work, and to use their bargaining power to limit firm's ability to adjust wages.

In this Section, we investigate how the presence and bargaining power of local unions significantly affect the menu of margins of adjustment that firms can use to react to sales shocks. To minimize endogeneity issues, we define unions' power based on the time-invariant initial level of firm-level union density. On the basis of this variable, we classify firms into three groups: those without unions, those with weak unions (i.e., with an initial union density lower than 40%, corresponding to the 75th percentile of this indicator in unionized firms) and those with strong unions (i.e, with an initial union density above 40%).[23] We then compute three dummy variables identifying these three groups of firms and interact each of them with the predicted logarithm of real sales from our first stage regression.

[23] In the total sample, 15.3% are non-unionized firms, 53.8% are firms with weak unions and the remaining 30.9% are firms with strong unions. We experimented with other cut-offs along the union density distribution to define strong unions. While results are qualitatively the same, the estimated elasticity become larger in size and more statistically significant when we move the cut-off towards higher percentiles.

Figure 2 reports the main estimated IV-FE elasticities for these three groups of firms. The first panel shows that the elasticity of the wage bill to demand shocks is significantly lower in unionized firms, particularly in those with strong unions, mainly due to the lower sensitivity of the permanent employment component. This does not happen at the cost of higher elasticity of wages. In unionized firms, much of the adjustment takes place in working hours, especially through the use of short-time work schemes. The presence of unions is more relevant than the level of workforce unionization: while our estimates suggest that the sensitivity of both the wage bill and employment decline when unionization increases, the differences between firms with weak unions and those with strong unions are not statistically significant. Overall, our results suggest that unions seem to resist any form of adjustment that could affect incumbent workers (i.e. the insiders). In other words, unionized firms manage to mitigate overall employment fluctuations (both positive and negative), thereby reinforcing the impact of employment protection within the firm, consistent with the empirical evidence documented at the macro level (Checchi and Lucifora, 2002; Bertola and Rogerson, 1997).

(FIGURE 2 AROUND HERE)

Estimates by type of shock confirm that firms, regardless of union presence, adjust more to positive relative to negative shocks in sales, and much of the adjustment in unionized firms occurs through the use of short-time work when product demand declines, especially in the pre-Covid period (see Table A4 in the Appendix). These estimates point out that the extension of the use of short-time work to all firms during the Covid period, regardless of their actual sales conditions, has reduced the sensitivity of this margin of adjustment to declining sales mainly in unionized firms.

As an alternative measure of the presence of unions in the workplace, we conduct a similar exercise interacting predicted sales with a dummy variable that equals one for firms with a firm-level collective agreement at the beginning of the period. Estimates reported in Table A5 in the Appendix show a lower responsiveness of permanent employment to changes in sales in bargaining firms compared to non-bargaining ones. The estimated elasticity of wages remains small and not statistically different between the two groups. Firms without a firm-level agreement seem to adjust temporary employment

more than bargaining firms, but the difference is not statistically significant. Similarly, no significant difference emerges in the adjustment of short-time work, but this is due to the fact that around 70% of non-bargaining firms are unionized.[24]

Discussion

Unions in the workplace appear to successfully mitigate the impact of sales shocks on employment or wages. The above results are consistent with an "insider effect", suggesting that strong unions reinforce the effect of institutional constraints in protecting incumbent workers (i.e. their members). In other words, working time flexibility through short-time work is used to protect employees with a permanent contract when demand is contracting.

To get deeper insight into the number of jobs virtually saved through the use of short-time work in unionized firms, we carry out a simple simulation using our estimates by union density reported in Figure 2 with the average firm in the sample (i.e., with 181 employees and 15,917 hours of short-time work, equivalent to approximately 87 hours per employee). Table 5 summarizes the results of this exercise. For the average firm without unions, an 11.7% decline in real sales (corresponding to the first quartile of the distribution of the change in sales in percentage terms), would imply the layoff of 10 employees (5.3% reduction in total employment). Conversely, in a firm with the same characteristics but with unions, a comparable shock in sales would reduce employment by less than one third, with a decline in total employment of 3 employees (1.9%) in firms with weak unions, 1 employee (0.7%) in highly unionized firms. The same shock would significantly increase (by 41-45% compared to the baseline) the total number of hours of short-time work if the firm were unionized, with no significant changes under the "no unions" scenario. Considering the average per-capita annual working hours (1,756 hours), such increase in the number of hours of short-time work in the unionized firm corresponds to roughly 4 full-time equivalent workers. This implies that, in the

[24] If we split non-bargaining firms into those without unions and unionized firms, we obtain that the elasticity of short-time work hours to sales is larger and statistically significant in both groups of unionized firms compared to non-unionized ones, with no statistically significant difference between these two groups. Results are available upon request.

absence of short-time work, the total reduction in employment would have been at least twice as large as that estimated by our model. Our results confirm that short-time work is effective in reducing employment cuts (Boeri and Bruecker, 2011; Cahuc and Carcillo, 2011; Giupponi and Landais, 2023), but this occurs mainly in unionized firms.

(TABLE 5 AROUND HERE)

Notice also that the higher rigidity of both wages and employment in unionized firms is likely to shift the burden of the adjustment cost onto the employers, who may react by changing output prices or profit margins. While we have no direct information on firm-level prices in our data, we exploit information drawn from firms' balance sheets to check whether firm's profitability is sensitive to changes in sales and if the estimated elasticity varies with higher union density. To this end, we use the same model specification of Figure 2, with two different profitability indicators (ROA and ROE) as dependent variables. We also distinguish between positive and negative shocks. IV-FE estimates reported in Table 6 do not show clear-cut patterns. However, differently from what we found for labour cost adjustment, profits seem to react more to negative rather than to positive shocks (especially in the case of ROA), and firms with weak unions are those displaying the largest (and statistically significant) responsiveness.

(TABLE 6 AROUND HERE)

Our estimates complement the firm-level qualitative evidence based of WDN data, showing that adjustment of profit margins is a relevant strategy to cope with a negative demand shock for a lower share of firms compared to those adjusting costs or prices (Bertola et al., 2012).[25] In this respect, our results suggest that this strategy is more likely in firms without unions or where the latter are weak, and that employment and wage rigidity does not necessarily reflect into a higher sensitivity of profits to sales shocks, as in the case of firms with strong unions.

[25] At the EU level, approximately 70% of the respondents indicate that a reduction of other costs and price increases are "very relevant" or "relevant" options, while around 57% declare that a reduction in profit margins is a relevant response.

Another issue is related to whether previous investments may influence firm's strategy to cope with current shocks, and whether such effect depends on the presence of unions in the workplace. For example, firms may anticipate bad shocks and plan new investments in previous periods. If investments depend also on the presence of unions, for example because strong unions support them, especially when they can generate positive employment or wage effects for their members (Bryson et al., 2020), the lower sensitivity of employment in unionized firms to current shocks may be driven by past investments. While there is no consensus in the literature on the effects of unions on firm investments (see da Silva Bichiara et al., 2023 for a recent review), evidence for Italy does not support the existence of the traditional hold-up problem, according to which the presence of unions at the workplace should discourage innovation because part of the rents will be captured by workers through wage bargaining. If any, in the Italian case the presence of unions increases product innovation (Berton et al., 2021), and there is little evidence that wage bargaining at the firm level reduces the returns on investment (Card et al, 2014). Unfortunately, our dataset does not contain direct information on product or process innovation or the number of patents, but we can exploit accounting data on investments and revenues from patents to test whether the margins of adjustments to changes in sales by union density are influenced by past investments. Specifically, we define "investing firms" as those which have increased their total investments in both tangible and intangible assets between year t-1 and year t[26]; alternatively, we define "innovating firms" as those which have increased their revenues from patents over the same period. We then interact these dummies with both predicted sales and the presence of unions at the workplace.[27] Estimates for the triple interaction term reported in Table A6 in the Appendix are very small and not statistically significant in the case of wage, employment and short-time work, confirming that our main results by union density are not driven by past investments.

[26] We obtain similar results even if we define investing firms on the basis of investments in tangible or intangible assets separately. Estimates are available upon request.

[27] Since we are using a triple interaction term and previous estimates did not show large and statistically significant differences between firms with weak unions and firms with strong unions, we distinguish between unionized and non-unionized firms.

7. Robustness checks and further estimates

In this Section, we present a set of further estimates we carried out to test the robustness of the baseline estimates. The main results are in Table 7, where in the last row we report our baseline estimates from Tables 2 and 3 to ease comparison.

Each cell displays the estimated IV-FE elasticity of the dependent variable to real sales, as indicated in each column. Rows differ for the empirical strategy or model specification used.

First, in order to control for time-varying factors that may be correlated with changes in sales and have a direct effect on either wages or employment components, we estimated a richer model specification, controlling for a vector of firm-level characteristics, including workforce composition and local industrial relations (row 1). Second, we exclude the Covid period to test whether our main results are driven by the large but very peculiar shock caused by the pandemic (row 2). Finally, we control for the two major reforms that we discussed in the institutional setting, that is the 2012 Fornero law and the 2015 Jobs Act, which have both reduced firing costs associated with permanent contracts in firms with more than 15 employees. Specifically, we created a dummy equal to 1 for firms with more than 15 employees before 2012 and interacted it with a dummy equal to 1 for the years following the two reforms (i.e. since 2012 for the first reform and since 2016 for the second one). We then re-estimated our model controlling for each reform at a time and for both reforms (part 3 in Table 7). Overall, estimates in Table 7 are very similar to our baseline estimates, although permanent employment elasticity (and hence that of the wage bill) slightly declines once we control for the two major reforms and is not statistically significant.

A last concern may be related to the fact that, since we use firm-level data, the responsiveness of average wages may be due to changes in employment composition within the firm rather than to changes in wage levels by job title. Such composition effects may explain the small but significant elasticity that we estimate in some specifications for the base wage. Unfortunately, we do not have access to individual data and we cannot distinguish stayers from movers in the workforce. However, we can exploit information on employment and wages by job title. To control for changes in

employment composition, we then re-estimated the firm-level average wage keeping employment composition constant at the initial level by job title. Estimates are reported in row 4 of Table 7. If we use the logarithm of this wage measure as the dependent variable, the estimated elasticity is smaller and weakly statistically significant for the base wage, confirming that changes in employment composition partly explain our baseline estimates for this variable.

(TABLE 7 AROUND HERE)

As previously discussed, another concern may be related to attrition bias. Given the survey nature of our data and the relatively long period considered, firms may exit the sample and eventually re-enter due to the random sampling, or exit permanently when closing down due to a negative shock. As mentioned in the data Section, around 18% of the firms exit permanently at some point in time, while the great majority of the firms (around 80%) exit for one or more years and then re-enter in the sample.[28] The latter are firms that are likely to be randomly selected in the sample, but unlikely to be missing because they close down. We then expect that changes in sales may influence the probability that a firm exit permanently from the sample, while it should not be related with the probability that a firm does not participate regularly to the survey. To test this hypothesis, we define firm exit at time t+1 when a firm is observed at time t, but she is missing at time t+1. Furthermore, we distinguish between firms exiting permanently (i.e., those that are missing from the sample starting from t+1) and "no-response" firms (i.e., those exiting at t+1 but that are observed again in later years in the sample). Estimates using these two indicators as dependent variables and reported in Table A7 in the Appendix. Given the focus of our analysis on firm-level unions and industrial relations, we also test whether firm exit may be influenced by firm-level union density and the presence of a firm-level agreement. They show that changes in sales do not significantly influence the probability of either

[28] We measure these two different types of exit with two dummy variables: a dummy variable equal to one for firms observed in year t and not observed in the following years of the survey (exit) and a dummy equal to one for firms observed in year t, not observed in year t+1 but observed again in year t+k, with k greater than one (no response).

exiting the sample (columns 1-2) or having some missing or no-response years (columns 3-4). Furthermore, neither the probability of exiting nor that of no-response is influenced by changes in union density or the introduction of a firm-level agreement (columns 2 and 4).

Finally, as noted by Jaeger et al. (2018), we should consider that adjustment to shocks may take some time. In this case, the error term of equation [1] can also include factors that reflect the ongoing adjustment to past shocks. If this is the case, our baseline estimates may mix quite different short-term and long-term responses, such as a fall in wages in the short term caused by declining real sales, followed by an increase in wages once other factors, such as capital, have time to adjust. In order to account for potential adjustment dynamics, Jaeger et al. (2018) propose to enrich the model specification with a lagged term for the shock (in our case, real sales), instrumenting this term with a lagged analogous instrument as the one used for current real sales.

IV-FE estimates of this dynamic model are reported in Table 8. The estimated elasticities for current sales confirm all the results we obtained with our baseline model, while elasticities to lagged sales are never statistically significant.[29] Hence, our main results should not be driven by confounding factors shaping adjustment dynamics.

(TABLE 8 AROUND HERE)

8. Conclusions

In this paper we have investigated firms' margins of adjustment to sales shocks, when both wages and employment determination are subject to regulations and collective bargaining.

Our results, based on firm-level panel data for the Italian metal-engineering industry over 2009-2021, confirm that firms have a number of margins of adjustments to cope with demand shocks, but they are significantly influenced by the strictness of labour market institutions and firm-level unions.

Our estimates show that the total wage bill is influenced by changes in sales, but such sensitivity is mainly driven by changes in working hours and partly by changes in employment, while total wages

[29] These estimates should be interpreted with some caution, given that the first-stage F test is close to the conventional critical value used to reject the hypothesis of weak instruments.

are largely unaffected. While collective agreements may prevent firms from cutting base wages, still other variable wage components, bargained at the firm-level and linked to firm's performance, can be adjusted. However, our estimates reveal a small but asymmetric adjustment of the base wage, which significantly reacts only in the case of increasing sales, confirming the role of nominal downward wage rigidity in preventing wage cuts. The lack of any significant adjustment in the wage cushion may be due to several institutional features, such as the negligible incidence of variable pay components (which represent less than 5% of the total wage), as well as the "absorption" effect of variable pay components into the "base wage" upon collective contract renewal.

When we turn to employment components, we find that the elasticity to sales is larger for temporary employment compared to permanent one. However, since the first represents a small share of total employment in the metal-engineering industry, the estimated elasticity of total employment to sales is economically small and mainly driven by changes in full-time permanent employment.

Working hours and the activation of short-time work schemes remain the main adjustment mechanisms firms rely upon to cope with declining sales.

Besides labour market regulation, strong unions at the workplace represent an additional source of rigidity, reinforcing the effect of institutional constraints that protect employment of incumbent workers with a permanent contract (i.e. the "insiders" who are union members). Back-of-the-envelope calculations show that adjustment of the intensive margin through the use of short-time work helps save a substantial number of full-time equivalent jobs in unionized firms where, without short-time work, total employment reduction would have been at least twice as high as that estimated with our model.

While loosening eligibility conditions could in principle favour opportunistic behaviours and be not sustainable for public finances in the long run, in a context characterized by a very severe temporary shock followed by a fast and robust economic recovery, such as the Covid pandemic, short-time work proved to be an effective policy to save jobs when most other channels were shut either by rigid regulations or by strong unions.

The policy implications of our results suggest the need for measures to improve firms' flexibility, particularly balancing the insurance effect of industry-level collective bargaining with the productivity-enhancing effect of variable pay set in decentralised agreements. In this regard, firm-level unions may play a crucial role in monitoring that the design of variable pay schemes promotes rent sharing without transferring all business risks onto workers.

We believe that the external validity of our findings extends to most manufacturing firms. Indeed, the metal-engineering industry is a leading sector in industrial relations and represents the largest share of employment and larger firms in the Italian manufacturing industry. Moreover, in our sample, firms are highly heterogeneous in terms of size, technology, export orientation, investment, and unionization. We exploit this heterogeneity in a number of exercises to provide evidence on the behaviour of comparable stylized firms. In this context, our findings could guide the design of policies aimed at reducing overall wage rigidity by increasing the wage elasticity of variable pay components to real sales. For example, increasing both the share of wage cushion in total wages and the tax exemptions on variable performance-related pay components may represent the ideal mix to support firms' resilience to business cycle fluctuations.

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Table 1 – Summary statistics 2009-2021, final sample

	Mean (1)	Standard deviation (2)	Min (3)	Max (4)
Sales (Thousands Euros)	46489.58	273975.00	48.87	1.79E+07
Wage bill (Thousands Euros)	5485.17	20261.46	42.21	808634.20
Per-capita annual wage (Euros)	30024.19	5335.84	19868.43	86015.52
Monthly base wage	1784.95	261.60	1072.93	3727.55
Annual wage cushion	6819.18	4050.06	0	63485.74
Total employment	180.89	581.02	2	24629
Part-time employees	6.88	17.91	0	415
Temporary employees	5.17	15.99	0	377
Contractual annual working hours	1755.95	76.91	1493	2032
Total hours of short-time work	15917.12	186291.20	0	1.40E+07
% unionized employees	23.71	19.30	0	100.0
% firm-level agreement	49.32	49.99	0	100.0
N observations	6733			
N firms	2300			

Table 2 – Elasticity of the wage bill to real sales

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	wage bill		annual wage		employment	
	FE	IV-FE	FE	IV-FE	FE	IV-FE
log(sales)	0.198***	0.231**	0.005	0.056	0.209***	0.132*
	[0.027]	[0.093]	[0.004]	[0.039]	[0.027]	[0.080]
Mean Y	5485.2		30.0		180.9	
Observations	6733	6733	6733	6733	6733	6733
Number of firms	2300	2300	2300	2300	2300	2300

NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects. Bootstrapped standard errors clustered at the firm level in brackets. The mean wage bill and annual wage are expressed in thousands of Euros. First-stage estimates are in Table A2 in the Appendix.
*** p<0.01, ** p<0.05, * p<0.1

Table 3 – Elasticity of wage and employment components to real sales
IV-FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Wage		Employment				
			Extensive margin			Intensive margin	
	Base (monthly)	Cushion (annual)	Permanent full time	Permanent part-time	Temporary	Per capita contractual hours	Short-time work (total hours)
log(sales)	0.079**	-0.329	0.141*	0.043	0.659**	0.022	-3.308***
	[0.034]	[0.495]	[0.083]	[0.240]	[0.323]	[0.018]	[1.237]
Mean Y	1785.0	6819.2	168.8	6.9	5.2	1756.0	15917.1
Observations	6733	6733	6733	6733	6733	6733	6733
Number of firms	2300	2300	2300	2300	2300	2300	2300

NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects. Bootstrapped standard errors clustered at the firm level in brackets. Mean wages are expressed in Euros. First-stage estimates are in Table A2 in the Appendix.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 – Asymmetric adjustment to positive and negative changes in real sales
IV-FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				Wage components:		Employment:			Working hours:	
VARIABLES	wage bill	wage	employ-ment	base wage (monthly)	wage cushion (annual)	permanent full-time	part-time	tempo-rary	anual per-capita hours	short-time work
POSITIVE SHOCK ($Sales_t > Sales_{t-1}$)										
log(sales)	0.302**	0.071	0.195*	0.115**	-1.050	0.180*	0.446	0.387	0.048***	-4.367***
	[0.145]	[0.068]	[0.100]	[0.052]	[0.780]	[0.102]	[0.277]	[0.467]	[0.016]	[1.550]
Mean Y	6024.2	30.4	199.8	1803.2	6941.8	184.8	7.6	6.4	1759.8	13772.9
Observations	3339	3339	3339	3339	3339	3339	3339	3339	3339	3339
Number of firms	1647	1647	1647	1647	1647	1647	1647	1647	1647	1647
NEGATIVE SHOCK ($Sales_t < Sales_{t-1}$)										
log(sales)	0.311**	0.024	0.252**	0.027	-0.172	0.263**	-0.213	0.730	0.014	-1.973
	[0.127]	[0.043]	[0.118]	[0.039]	[0.611]	[0.134]	[0.316]	[0.583]	[0.035]	[2.258]
Mean Y	5142.9	29.7	165.4	1767.4	6705.4	154.5	6.3	4.1	1752.0	18086.0
Observations	3394	3394	3394	3394	3394	3394	3394	3394	3394	3394
Number of firms	1811	1811	1811	1811	1811	1811	1811	1811	1811	1811

NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects.
Bootstrapped standard errors clustered at the firm level in brackets. The mean wage bill and annual wage are expressed in thousands of Euros, the mean wage components are in Euros. First-stage estimates are in Table A2 in the Appendix.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 – Estimates of the effect of a negative sales shock on employment and short-time work in the average firm by union density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	employment change		change in STW hours		"Saved" FTE workers	Change in employment w/o STW	
	number	%	number	%	number	number	% initial employment
no unions	-10	-5.3	n.s	n.s.	-	-10	-5.3
weak unions	-3	-1.9	6495.0	40.8	4	-7	-3.9
strong unions	-1	-0.7	7136.7	44.8	4	-5	-3.0

NOTE: simulation based on estimates reported in Figure 2, applied to the average firm (initial number of employees=180.9, total short-time work=15917 hours), subject to a reduction in real sales of 11.7% (corresponding to the first quartile of the distribution of changes in sales). STW= Short-time work, FTE= Full-Time Equivalent

Table 6 – Elasticity of profitability to real sales by union density and type of sales shock
IV-FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	ROA			ROE		
	All	positive shock	negative shock	All	positive shock	negative shock
no unions	0.901	1.468	5.331	15.716	21.516	14.564
	[4.510]	[4.512]	[6.314]	[9.710]	[13.332]	[14.884]
weak unions	4.006	-1.375	8.129*	11.762*	0.363	15.878*
	[2.661]	[3.284]	[4.932]	[6.402]	[9.567]	[9.068]
strong unions	2.437	-1.401	5.483	16.684**	12.000*	6.398
	[2.603]	[1.910]	[5.012]	[6.716]	[6.782]	[10.056]
Mean Y	3.233	5.149	1.478	2.355	6.891	-1.898
Observations	6605	3282	3323	6411	3215	3196
Number of firms	2270	1629	1783	2210	1593	1723

NOTE: The table reports the estimated coefficients of the interaction terms between the log of real sales and a set of dummies measuring initial firm-level union density, where "no unions" are non-unionized firms, "weak unions" are unionized firms with initial union density below the 75th percentile, "strong unions" are firms with union density above the 75th percentile. Models include also industry-specific time fixed effects. Bootstrapped standard errors clustered at the firm level in brackets. First-stage estimates are in Table A2 in the Appendix.
Positive shock indicates increasing real sales between t-1 and t, negative shock indicates declining real sales between t-1 and t.
*** p<0.01, ** p<0.05, * p<0.1

Table 7 – Estimated elasticity to real sales. Robustness checks.
IV-FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				Wage components:		Employment:		Working hours:		
VARIABLES	wage bill	wage	employ-ment	base wage (monthly)	wage cushion (annual)	Permanent	Temporary	anual per-capita hours	short-time work	First-stage F test
1) Time varying controls (a)										
	0.265***	0.049	0.163*	0.082**	-0.333	0.145*	0.623*	0.029*	-3.491***	28.34
	[0.095]	[0.036]	[0.089]	[0.036]	[0.481]	[0.088]	[0.335]	[0.016]	[1.269]	
2) Pre-Covid period (2009-2018)										
	0.259***	0.053	0.162*	0.094**	-0.577	0.145*	0.698*	0.034*	-2.785**	23.90
	[0.098]	[0.042]	[0.084]	[0.037]	[0.553]	[0.081]	[0.360]	[0.019]	[1.308]	
3) Control for 2012 and 2015 major reforms (b)										
2012 Fornero Law	0.198**	0.041	0.138*	0.070**	-0.387	0.117	0.594*	0.030*	-3.358***	26.75
	[0.094]	[0.041]	[0.077]	[0.035]	[0.489]	[0.075]	[0.322]	[0.015]	[1.233]	
2015 Jobs Act	0.192**	0.037	0.134*	0.069*	-0.434	0.112	0.599*	0.031**	-3.394***	26.51
	[0.094]	[0.042]	[0.077]	[0.036]	[0.474]	[0.075]	[0.324]	[0.015]	[1.234]	
Both reforms	0.192**	0.036	0.134*	0.069*	-0.433	0.112	0.599*	0.031**	-3.393***	26.50
	[0.094]	[0.042]	[0.077]	[0.036]	[0.476]	[0.075]	[0.323]	[0.015]	[1.235]	
4) With constant employment composition (c)										
	-	0.047	-	0.066*	0.011	-	-	-	-	
		[0.036]		[0.037]	[0.243]					
BASELINE ESTIMATES	0.231**	0.056	0.132*	0.079**	-0.329	0.141*	0.659**	0.022	-3.308***	26.62
	[0.093]	[0.039]	[0.080]	[0.034]	[0.495]	[0.083]	[0.323]	[0.018]	[1.237]	

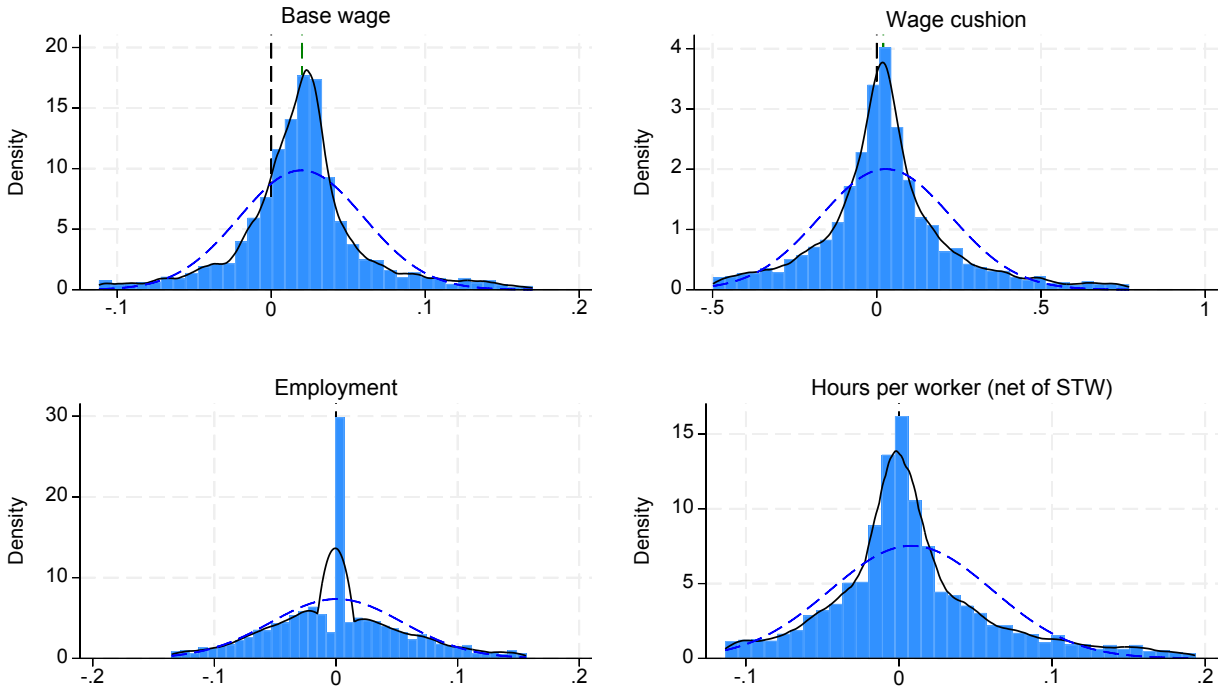
NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects. Bootstrapped standard errors clustered at the firm level in brackets.
*** p<0.01, ** p<0.05, * p<0.1
(a) Time-varying controls include: % females, % blue collars, % managers, union density, union density squared, firm-level agreement (dummy). (b) Both reforms reduced firing costs of permanent workers in firms with more than 15 employees. Models include as additional control variables a dummy variable equal to 1 since 2012 for firms with pre-treatment employment above 15 employees for the Fornero law, a dummy variable equal to 1 since 2016 for firms with pre-treatment employment above 15 employees for the Jobs Act. Pre-treatment employment is the average level of employment before 2012; (c) The dependent variable is the firm-level average annual wage (or wage component) computed keeping constant the initial employment composition by job title.

Table 8 – Estimated elasticities to current and lagged sales
IV-FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Wage components:		Employment:		Working hours:	
VARIABLES	wage bill	wage	employ-ment	base wage (monthly)	wage cushion (annual)	Permanent	Temporary	anual per-capita hours	short-time work
$\log(sales)_t$	0.208**	0.046	0.118	0.064*	-0.332	0.111	0.336	0.031*	-3.339***
	[0.099]	[0.040]	[0.083]	[0.038]	[0.442]	[0.079]	[0.280]	[0.017]	[1.153]
$\log(sales)_{t-1}$	0.140	0.067	-0.001	0.063	-0.620	0.047	-0.619	-0.010	0.791
	[0.132]	[0.069]	[0.125]	[0.058]	[0.864]	[0.124]	[0.508]	[0.024]	[1.661]
Mean Y	5552.9	30.0	181.5	1784.7	6817.4	176.3	5.2	1756.0	15945.8
Observations	6310	6310	6310	6310	6310	6310	6310	6310	6310
Number of firms	2291	2291	2291	2291	2291	2291	2291	2291	2291

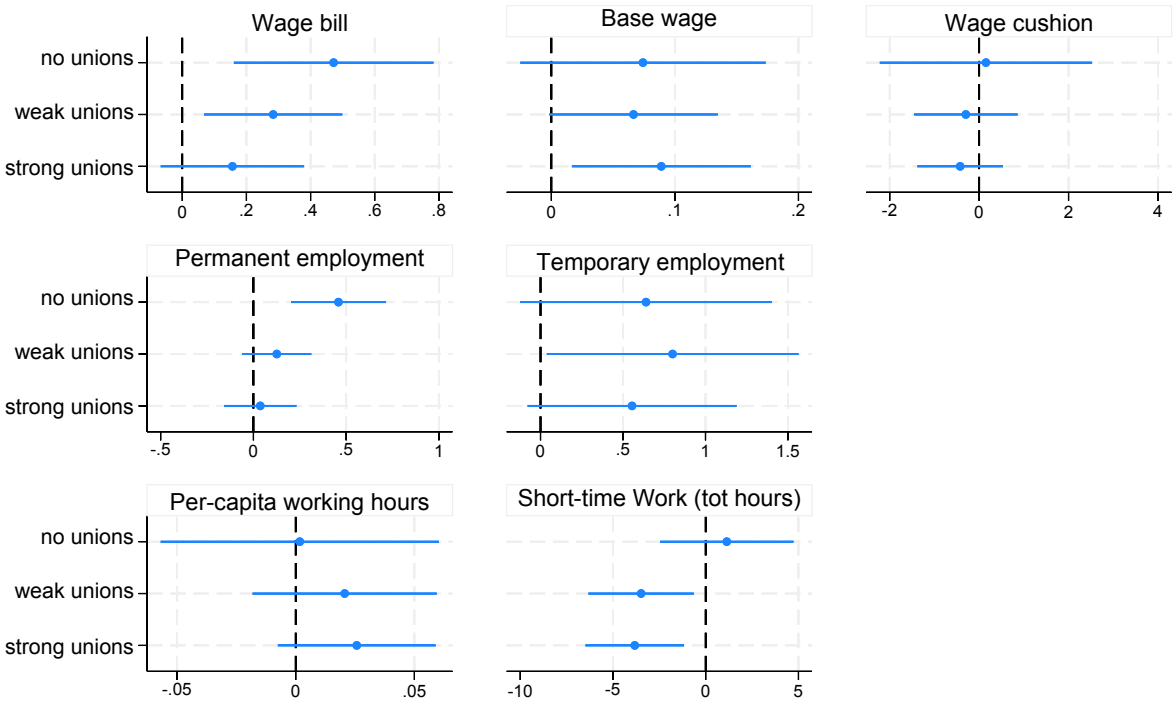
NOTE: All dependent variables are logarithms; models include also industry-specific time fixed effects. The mean wage bill and total wage are expressed in thousands of Euros, mean wage components are in Euros. F test for the first stage is 26.62 with current sales, 9.98 with lagged sales. Bootstrapped standard errors clustered at the firm level in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Figure 1 – Annual changes in main wage and employment components, 2009-2021



Note: the black dashed line corresponds to zero change, the dashed green line in the two upper panels to the target inflation rate.
The solid curve is the kernel density, while the dashed curve the normal density function.

Figure 2 – Elasticity of the wage bill and its main components to real sales by union density. IV-FE estimates.



Note: All dependent variables are logarithms. Each figure reports the estimated coefficients (and 95% confidence intervals) of the interaction terms between the log of real sales and a set of dummies measuring initial firm-level union density, where "no unions" are non-unionized firms, "weak unions" are unionized firms with initial union density below the 75th percentile, "strong unions" are firms with union density above the 75th percentile. First-stage estimates are in Table A2 in Appendix.

APPENDIX I - Estimation of annual working hours

Regarding working time, for blue and white collar full-time permanent employees, the survey provides the following information:

a. Number of days of paid holidays and other days off-work (defined in the industry collective agreement as "reduction of working time"*, riduzione dell'orario di lavoro*);
b. Weekly working hours defined by the industry collective agreement;
c. Number of minutes of paid breaks per week;
d. Number of annual hours of short-time work schemes

Furthermore, for each year we reconstructed:

e. Total number of days (365 or 366)
f. Number of Saturdays and Sundays
g. Number of days of bank holidays from Monday to Friday

Using this information, by year and firm we computed annual contractual working hours as follows:

$$\text{Annual contractual working hours} = (e-f-g-a)/5*(b-c/60) \quad [a1]$$

where the first term is the number of actual working weeks in a year and the second one is the number of contractual hours per week.

We then estimated annual working hours net of short-time work schemes as follows:

$$\text{Annual working hours net of short-time work} = \text{Annual contractual working hours} - d \quad [a2]$$

APPENDIX II – Additional Tables

Table A1 – Differences between the initial survey sample, the sample merged with accounting data and the final sample

	Initial sample (1)	Merged sample (2)	Final sample (3)	Final-Initial (3)-(1)		Final-Merged (3)-(2)	
Sales (Thousands Euros)	-	53610.45	46489.58	-		-7120.87	
Wage bill (Thousands Euros)	5626.2	6149.6	5485.2	-141.03		-664.40	*
Per-capita annual wage (Euros)	30620.61	30524.9	30024.19	-596.42	***	-500.68	***
Base wage	1807.19	1796.8	1785.0	-22.24	***	-11.88	***
Wage cushion	6902.26	6965.6	6819.2	-83.08		-146.40	**
Total employment	155.26	181.56	180.89	25.63	***	-0.67	
Part-time employees	6.52	7.20	6.88	0.36		-0.32	
Temporary employees	5.12	5.14	5.17	0.05		0.03	
Contractual annual working hours	1753.16	1752.4	1756.0	2.80	**	3.53	***
Total hours of CIG	10119.83	13265.8	15917.1	5797.29	***	2651.36	
% unionized employees	17.66	20.69	23.71	6.05	***	3.02	***
% firm-level agreement	40.96	46.92	49.32	8.36	***	2.40	***
N observations	17625	10302	6733				
N firms	6670	3131	2300				

NOTE: the initial sample refers to the survey sample, the merged sample is the survey sample matched with accounting data, the final sample is the merged sample net of missing values and outliers of the wage components

Table A2 – First stage estimates from IV-FE

VARIABLES	log(sales)
$shares_{2007}$*log(industry $sales_{j\text{-}i,t,}$)	0.437***
	[0.130]
Observations	6733
Number of firms	2300
F test (Kleibergen-Paap)	26.62

NOTE: Model specification includes also industry-specific time fixed effects. Robust standard errors clustered at the firm level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A3 – Estimates of heterogenous effects by sub-period
IV-FE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Wage components:		Employment:		Working hours:	
VARIABLES	wage bill	wage	employ-ment	base wage	wage cushion	permanent	tempo-rary	anual per-capita hours	short-time work
log(sales)	0.236**	0.058	0.129*	0.078**	-0.282	0.104	0.632*	0.022	-3.247**
	[0.093]	[0.039]	[0.079]	[0.034]	[0.502]	[0.078]	[0.330]	[0.018]	[1.264]
log(sales)*Great Recession	-0.004	-0.004	0.009	-0.000	-0.092*	0.004	0.037	-0.001	-0.072
	[0.013]	[0.004]	[0.012]	[0.003]	[0.049]	[0.012]	[0.038]	[0.002]	[0.139]
log(sales)*Covid	0.038	-0.007	0.026	-0.009	-0.119	0.032	-0.075	0.000	0.265
	[0.028]	[0.007]	[0.025]	[0.009]	[0.089]	[0.024]	[0.058]	[0.005]	[0.418]
Mean Y	5552.9	30.0	181.5	1784.7	6817.4	176.3	5.2	1756.0	15945.8
Observations	6733	6733	6733	6733	6733	6733	6733	6733	6733
Number of firms	2300	2300	2300	2300	2300	2300	2300	2300	2300

NOTE: All dependent variables are logarithms; models include also industry-specific time fixed effects. Great Recession is a dummy equal to one from 2009 to 2014, Covid a dummy equal to 1 for 2020 and 2021. The mean wage bill and total wage are in thousands Euros, mean wage components are in Euros. First-stage estimates are in Table A2.
Bootstrapped standard errors clustered at the firm level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Table A4 – Asymmetric adjustment to positive and negative changes in real sales by union density
IV-FE estimates

VARIABLES	wage bill	wage	employ-ment	Wage components: base wage (monthly)	wage cushion (annual)	Employment: permanent	tempo-rary	Working hours: anual per-capita hours	short-time work
Panel A: 2009-2021									
POSITIVE SHOCK ($Sales_t$>$Sales_{t-1}$)									
log(sales)	0.465*	0.046	0.764***	0.156*	-1.559	0.636***	1.219	0.025	-3.660
	[0.284]	[0.099]	[0.230]	[0.090]	[2.243]	[0.235]	[0.886]	[0.037]	[3.486]
log(sales)*weak union	-0.308	-0.007	-0.662***	-0.069	0.059	-0.609***	-0.108	-0.002	-1.633
	[0.260]	[0.089]	[0.201]	[0.073]	[1.658]	[0.208]	[0.691]	[0.034]	[3.036]
log(sales)*strong union	-0.157	0.032	-0.602***	-0.041	0.609	-0.485**	-0.995	0.028	-0.640
	[0.284]	[0.095]	[0.227]	[0.084]	[2.006]	[0.228]	[0.799]	[0.037]	[3.342]
Mean Y	6024.2	30.4	198.8	1803.2	6941.8	192.4	6.4	1759.8	13772.9
Observations	3339	3339	3339	3339	3339	3339	3339	3339	3339
Number of firms	1647	1647	1647	1647	1647	1647	1647	1647	1647
NEGATIVE SHOCK ($Sales_t$<$Sales_{t-1}$)									
log(sales)	0.602**	0.030	0.643***	-0.043	2.540	0.646***	1.015	0.020	1.841
	[0.295]	[0.073]	[0.223]	[0.077]	[1.676]	[0.233]	[0.721]	[0.045]	[3.400]
log(sales)*weak union	-0.195	-0.007	-0.359*	0.076	-2.812*	-0.419*	-0.344	0.005	-4.316
	[0.273]	[0.064]	[0.202]	[0.073]	[1.594]	[0.216]	[0.504]	[0.032]	[2.932]
log(sales)*strong union	-0.484	-0.003	-0.494**	0.068	-2.918*	-0.563***	-0.228	-0.025	-3.527
	[0.312]	[0.064]	[0.207]	[0.070]	[1.557]	[0.215]	[0.535]	[0.030]	[3.195]
Mean Y	5142.9	29.7	164.9	1767.4	6705.4	160.8	4.1	1752.0	18086.0
Observations	3394	3394	3394	3394	3394	3394	3394	3394	3394
Number of firms	1811	1811	1811	1811	1811	1811	1811	1811	1811
Panel B: Pre-Covid period (2009-2019)									
POSITIVE SHOCK ($Sales_t$>$Sales_{t-1}$)									
log(sales)	0.487	0.110	0.684**	0.203*	-1.529	0.647**	1.391	0.005	-7.199
	[0.418]	[0.137]	[0.305]	[0.117]	[3.228]	[0.326]	[0.953]	[0.051]	[4.522]
log(sales)*weak union	-0.281	-0.049	-0.589**	-0.058	-0.068	-0.580**	-0.435	0.002	0.538
	[0.372]	[0.125]	[0.263]	[0.094]	[2.419]	[0.283]	[0.693]	[0.046]	[3.790]
log(sales)*strong union	-0.136	-0.030	-0.468	-0.077	0.350	-0.452	-0.959	0.043	4.177
	[0.416]	[0.134]	[0.303]	[0.108]	[2.932]	[0.317]	[0.864]	[0.052]	[4.289]
Mean Y	5488.4	30.1	184.9	1788.2	6833.0	178.7	6.1	1759.6	12903.3
Observations	3161	3161	3161	3161	3161	3161	3161	3161	3161
Number of firms	1632	1632	1632	1632	1632	1632	1632	1632	1632
NEGATIVE SHOCK ($Sales_t$<$Sales_{t-1}$)									
log(sales)	0.276	-0.006	0.506**	-0.029	2.850	0.528**	0.788	0.022	4.834
	[0.271]	[0.084]	[0.237]	[0.094]	[2.009]	[0.255]	[0.742]	[0.053]	[3.559]
log(sales)*weak union	0.020	0.020	-0.317	0.086	-3.289*	-0.401*	-0.058	0.009	-6.701**
	[0.248]	[0.073]	[0.213]	[0.086]	[1.909]	[0.238]	[0.504]	[0.038]	[2.872]
log(sales)*strong union	-0.218	0.027	-0.424*	0.098	-3.763*	-0.524**	0.294	-0.024	-7.187**
	[0.251]	[0.076]	[0.219]	[0.090]	[1.980]	[0.242]	[0.571]	[0.039]	[3.248]
Mean Y	5037.9	29.5	162.5	1757.1	6633.2	158.4	4.1	1752.4	18122.2
Observations	3307	3307	3307	3307	3307	3307	3307	3307	3307
Number of firms	1800	1800	1800	1800	1800	1800	1800	1800	1800

NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects. "Weak unions" are unionized firms with initial union density below the 75th percentile, "Strong unions" are firms with initial union density above the 75th percentile. The interaction terms capture the difference from the reference group, that is firms with no unions. The mean wage bill and total wage are in thousands Euros, mean wage components are in Euros. First-stage F test: 26.62 in Panel A, 24,58 in Panel B. Bootstrapped standard errors clustered at the firm level in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table A5 – Estimates by firm-level bargaining
IV-FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Wage components:		Employment:		Working hours:	
	wage bill	wage	employ-ment	base wage (monthly)	wage cushion (annual)	permanent	temporary	anual per-capita hours	short-time work
log(sales)	0.257**	0.045	0.223**	0.070**	-0.185	0.197**	0.743**	0.015	-2.958**
	[0.113]	[0.038]	[0.094]	[0.034]	[0.619]	[0.093]	[0.338]	[0.018]	[1.264]
log(sales)*firm agreement	-0.043	0.017	-0.149**	0.014	-0.236	-0.151**	-0.138	0.011	-0.573
	[0.095]	[0.024]	[0.073]	[0.025]	[0.413]	[0.073]	[0.235]	[0.012]	[1.051]
Mean Y	5552.9	30.0	181.5	1784.7	6817.4	176.3	5.2	1756.0	15945.8
Observations	6733	6733	6733	6733	6733	6733	6733	6733	6733
Number of firms	2300	2300	2300	2300	2300	2300	2300	2300	2300

NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects. The interaction term captures the difference from the reference group, that is firms without a firm-level agreement. The mean wage bill and total wage are in thousands Euros, mean wage components are in Euros. First-stage estimates are in Table A2. Bootstrapped standard errors clustered at the firm level in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table A6 – Estimates by union density and investment strategy
IV-FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Wage components:		Employment:		Working hours:	
	wage bill	wage	employ-ment	base wage (monthly)	wage cushion (annual)	permanent	tempo-rary	anual per-capita hours	short-time work
				A) INVESTING FIRMS					
log(sales)	0.453***	0.030	0.448***	0.075	0.179	0.452***	0.575	0.004	1.219
	[0.157]	[0.057]	[0.126]	[0.051]	[1.206]	[0.128]	[0.385]	[0.030]	[1.811]
log(sales)*investor	0.003	0.005***	-0.004	0.002	-0.029	-0.001	0.004	-0.002*	0.042
	[0.005]	[0.002]	[0.005]	[0.002]	[0.038]	[0.004]	[0.018]	[0.001]	[0.090]
log(sales)*union	-0.245*	0.018	-0.340***	0.004	-0.525	-0.372***	0.040	0.021	-4.749***
	[0.134]	[0.045]	[0.107]	[0.042]	[0.953]	[0.111]	[0.271]	[0.024]	[1.655]
log(sales)*union*investor	-0.002	-0.001	-0.001	-0.001	0.020	-0.002	0.007	0.000	-0.017
	[0.002]	[0.001]	[0.002]	[0.001]	[0.019]	[0.002]	[0.006]	[0.000]	[0.032]
				B) INNOVATING FIRMS					
log(sales)	0.456***	0.036	0.442***	0.077	0.113	0.444***	0.607	0.004	1.013
	[0.158]	[0.058]	[0.127]	[0.051]	[1.201]	[0.128]	[0.392]	[0.028]	[1.825]
log(sales)*innovator	0.006	0.003	0.007	-0.001	0.043	0.007	0.042	0.001	0.107
	[0.010]	[0.002]	[0.009]	[0.003]	[0.058]	[0.009]	[0.026]	[0.002]	[0.145]
log(sales)*union	-0.246*	0.018	-0.340***	0.003	-0.504	-0.373***	0.048	0.021	-4.768***
	[0.136]	[0.046]	[0.107]	[0.042]	[0.945]	[0.111]	[0.277]	[0.024]	[1.671]
log(sales)*union*innovator	-0.003	0.000	-0.002	0.001	-0.019	-0.001	-0.018**	-0.002***	0.002
	[0.004]	[0.001]	[0.004]	[0.001]	[0.034]	[0.004]	[0.009]	[0.001]	[0.065]
Mean Y	5552.9	30.0	181.5	1784.7	6817.4	176.3	5.2	1756.0	15945.8
Observations	6733	6733	6733	6733	6733	6733	6733	6733	6733
Number of firms	2300	2300	2300	2300	2300	2300	2300	2300	2300

NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects. Union is a dummy equal to one for unionized firms in the initial period. Investor is a dummy equal to 1 for firms increasing total investments from t-1 to t; innovator is a dummy equal to 1 for firms increasing revenues from patents rights from t-1 to t. Interaction terms identify the difference from the reference category, that is firms with no unions and no increasing investments or no increasing revenues from patent rights.
The mean wage bill and total wage are in thousands Euros, mean wage components are in Euros. First-stage estimates are in Table A2. Bootstrapped standard errors clustered at the firm level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Table A7 – Estimates of the effect of changes in sales, union density and firm-level bargaining on firm exit and temporary non-participation in the survey
IV-FE estimates

	(1)	(2)	(3)	(4)
VARIABLES	exit	exit	no response	no response
log(sales)	-0.042	-0.042	-0.270	-0.267
	[0.083]	[0.083]	[0.206]	[0.206]
union density		0.008		-0.004
		[0.044]		[0.037]
firm-level bargaining		-0.001		0.014
		[0.011]		[0.023]
Meany Y	0.056		0.418	
Observations	6733	6733	6733	6733
Number of firms	2300	2300	2300	2300

NOTE: all dependent variables are logarithms; models include also industry-specific time fixed effects. First-stage estimates are in Table A2 in Appendix. Firm exit is a dummy equal to 1 when a firm is observed in year t, but she is missing in t+1 and in all the following years. "No response" is a dummy equal to 1 when a firm is observed in year t, she is missing in t+1 but is observed again in later years in the sample.
Bootstrapped standard errors clustered at the firm level in brackets
*** p<0.01, ** p<0.05, * p<0.1