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Wage councils, product markups and wage markdowns: Evidence from Uruguay ☆

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ABSTRACT

In 2005, after a leftist coalition won the national election for the first time, Uruguay returned to sector-level wage bargaining councils with active government participation. We estimate product markups and wage markdowns using firm-level data for the period 2002–2016, and report decreasing wage markdowns and increasing -to a lesser extent- firm-level product markups. We find statistically significant impacts of minimum mandated wages on product markups and wage markdowns, and additional effects of unions on wage markdowns. The evidence suggests that firms operate in monopsonistic labor markets. Though their bargaining power in the labor market was reduced over time as a result of wage councils, firms were able to pass a sizable part of the increases in labor costs to consumers.

1. Introduction

A broad discussion on measurement and changes in firm-level product markups is presently developing in the economics literature. This has important implications since market competition is the main driver of firm selection, productivity growth and welfare. It affects resource allocation between consumers and firms, and its evolution has profound macroeconomic consequences apart from the obvious effects on antitrust and tax policy design. The recent literature also emphasizes empirically disentangling firm-level product markup estimations from wage markdowns, *i.e.*, the ratio between price and marginal cost from the ratio of marginal labor revenue to wages. We contribute to this literature by considering the impact of sector-level wage negotiation on firm performance along such dimensions.

De Loecker and Warzynski (2012) and De Loecker et al. (2020) document a generalized rise in product markups in developed countries since the 1980s. Using data from the United States, they report that though product markups do not increase for most firms, there is a sharp increase for those in the upper tail of the markup distribution, which also gain market share.

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This generates an average product markup increase. They argue this leads to a declining labor share. Autor et al. (2017) discuss explanations for the fall in the labor share in the US economy in line with the findings of De Loecker and Warzynski (2012) about the impact of very large firms.

The magnitude and evolution of product markups in developing countries, and their relationship to the labor share and wage markdowns, have been less studied. We explore data for 2002–2016 from Uruguay, a small Latin American economy in which wage councils were set up to handle sector-level, centralized negotiations, with active government participation, and sector-level minimum wages increased substantially.

Methodologically, this paper is based on the Mertens (2022) extension of the De Loecker and Warzysnki (2012) approach.[1] They propose a suitable alternative to obtain firm-level indicators of product markup over marginal cost. A set of recent papers (Mertens 2020, 2022; Yeh et al., 2022; Morlacco, 2019; Dobbelaere and Kiyota, 2018, among others) stress the importance of not considering wages as exogenous, but rather that firms have some degree of monopsonistic power in the labor market, hence facing an upward sloping labor supply schedule.

We contribute to the literature in several ways. First, we document the evolution of firm-level product markups and wage markdowns for a less-developed country. We go beyond manufacturing and include service sectors. The results show a different pattern than for many developed countries, including evidence of an increasing labor share of revenue. These trends are common to all firms and not exclusive to large ones.

Second, we distinguish between market power in output product markup and labor market wage markdown. We show that labor market wage markdown decreases are not compensated by increases of similar magnitude in firm's markups in their final goods markets. This suggests that although firms operated in monopsonistic labor markets, their overall market power was significantly eroded along the period.

Third, we show that changes in the institutional settings of wage negotiation *per se* decreased wage markdowns and increased product markups. We address the main channel through which wage councils operate: setting minimum sectoral wages. The estimated elasticities imply that for every 1% increase in wage council mandated wages, firm wage markdown decreased by 0.10 to 0.15% while product markups increased by 0.10 to 0.14%. Thus, firms were able to pass a sizable portion of their increased labor costs to consumers. Given the change in mandated wages these elasticities account for the whole variation in product markups, but only for a quarter to a third of the variation in wage markdowns.

Fourth, we estimate the impact of union density on wage markdowns and find a statistically significant negative effect, with a wage markdowns elasticity to unionization of −0.08. Given the observed changes in union densities, this estimation implies that the direct effect is not economically meaningful and whatever effect unions produced on wage markdowns, it was mostly channeled through wage councils.

2. Wage councils and the Uruguayan labor market

2.1. The return of wage councils

Act 10,449 of 1943 created wage councils as tripartite wage negotiations to be held periodically between employers, workers representatives and government delegates. They established worker categories and minimum wages by sector of activity. In addition to sector wages, a national minimum wage was set by decree by the Finance Ministry.

The military dictatorship (1973–1984) did not summon wage councils. The succeeding democratic governments did in 1985–1991 but ceased to since 1992. Collective bargaining became then voluntary and bipartite, without government participation. When it occurred, most of it was at firm level, with exceptions at more aggregate sector level.

The pre-2005 years were marked by a severe economic crisis in which GDP fell an accumulated 20% in the face of a banking crisis, depositors run, debt restructuring and a 100% devaluation of the peso with respect to the US Dollar in 2002. In 2004, the left-leaning heterogeneous coalition *Frente Amplio*, which remained in the political opposition since the return to democracy, won the national elections and a majority in the Senate and Chamber of Representatives. In 2009 and 2014 it won elections again retaining control of both houses. The *Frente Amplio* administrations reinstated wage councils in 2005.[2]

In Uruguay since the 1960s a single trade union central has comprised all public and private sector unions. After 2005, changes in wage bargaining and a supportive stance of the government strengthened incentives for unionization across the board, and affiliation increased. Mazzuchi (2009) cites union leaders reporting the emergence of more than 400 trade unions in firms and an increase in total membership of 150,000. We provide evidence for a more cautious assessment below.

Employer associations representing specific sectors (for instance, Clothing Chamber or Metallurgy Chamber) usually group themselves into larger chambers by major sectors of activity (Chamber of Industry, Chamber of Commerce and Services, Mercantile Chamber of Domestic Products, etc.). Most firms do not participate actively in their respective chambers. Thus, for most firms, whatever is negotiated at the chamber level is exogenous.

[1] Their approach can be traced back to Hall (1988).

[2] Changes related to labor markets included protection of union rights; extension of time period for labor claims; days off work for study, parenting, marriage and mourning; control of outsourcing; creation of National Institute of Employment and Vocational Training; changes to unemployment insurance and its extension to specific sectors and firms; caps to working hours in rural labor, and a hike of the national minimum wage in 2005. A summary of labor market institutions in Uruguay can be found in Pagés (2005), later updated by Gandelman (2009) and Alaimo and Rucci (2009).

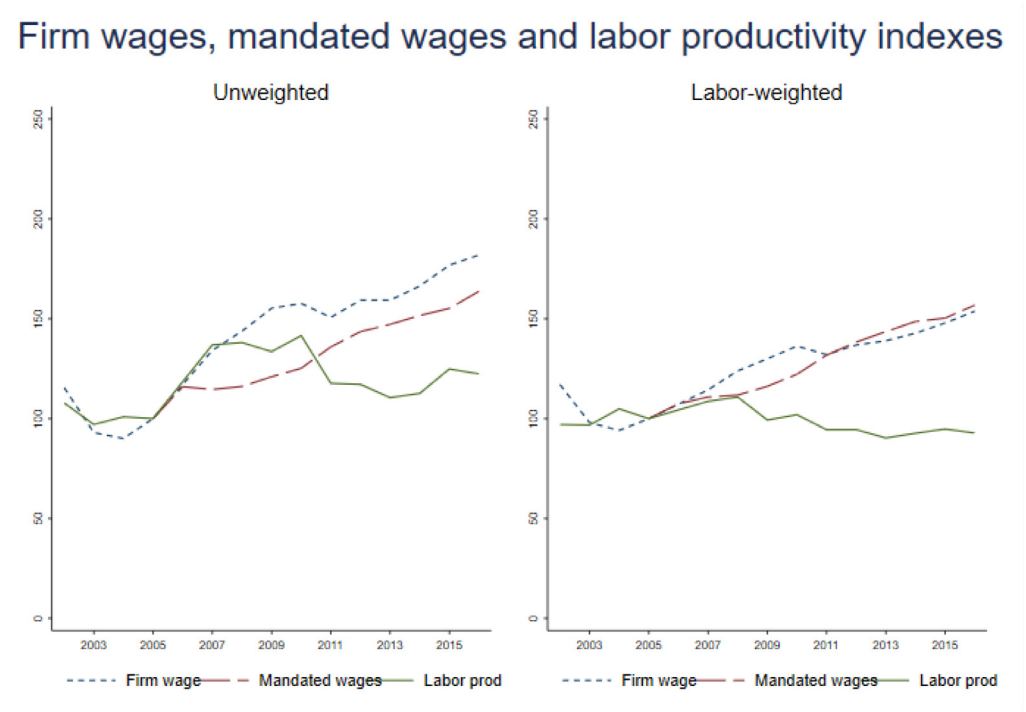

Fig. 1. Firm wages, mandated wages and labor productivity indexes. Index 2005=100.
Source: Based on Annual Economic Activity Survey – INE. Mandated wages from Labor and Social Security Ministry.

Since 2005 negotiations were carried out at sector level, formally coordinated by the union federation and the respective employer chamber. Wage agreements are legally enforced by the government.

There are 20 groups (sectors of activity) summoned for negotiations. Within them, there are about 50 subgroups and more than 200 sub-subgroups at which agreements are struck and minimum wages set.

Before each "negotiation round" the government sets guidelines for wage increases. If workers and employers do not strike a deal, the government may set minimum wages by decree. This happened very rarely. Negotiation rounds were carried out in 2005, 2006, 2008, 2010, 2012 and 2015. Most wage adjustments were scheduled to take place in January and July each year.

The Secretary for Labor and Social Security stated that by the end of 2004, only 20% of workers were covered by labor agreements.[3] By late 2007 all sectors of activity were covered, though not all workers were, due to informality. National Statistics Institute (INE hereafter) household survey data show that 28% of private sector wage employees were not covered by social security in 2005; this proportion fell to 12% in 2016. Also using household surveys, Cabrera et al. (2013) analyze wage council non-compliance at the lower tail of the wage distribution, *i.e.,* workers being paid below mandated sector levels. In 2011, 14% of individual wages were below minimum mandated wage council levels. Firms are penalized in case of not complying with wage councils' minimum wages or other labor regulations.

Finally, mandated sector wages are understood as minimum levels, but wage councils also determine proportional increases in wages throughout the firm's wage structure. Other issues such as benefits, work conditions, conflict solution mechanisms, peace clauses, union activity regulation have been agreed upon in some sectors but there is not a common pattern.

2.2. Labor market overview

To show the policy at work, Fig. 1 plots for our firm database the evolution of average firm real wages and mandated wage indexes in real terms.[4] The same graph also plots revenue per worker as a proxy for apparent labor productivity,[5] as an index equal to 100 in 2005. We report simple and employment-weighted averages.

Although labor productivity remained stagnant for most of the period, average wages increased steadily after 2005 following wage council mandated wage increases. The same pattern is observed by firm size class, sector of activity and for

[3] Dr. Jorge Bruni, cited in Alaimo and Rucci (2009).
[4] Both variables deflated by GDP deflator.
[5] Revenue per worker is not real labor productivity, being affected by price markups and wage markdowns.

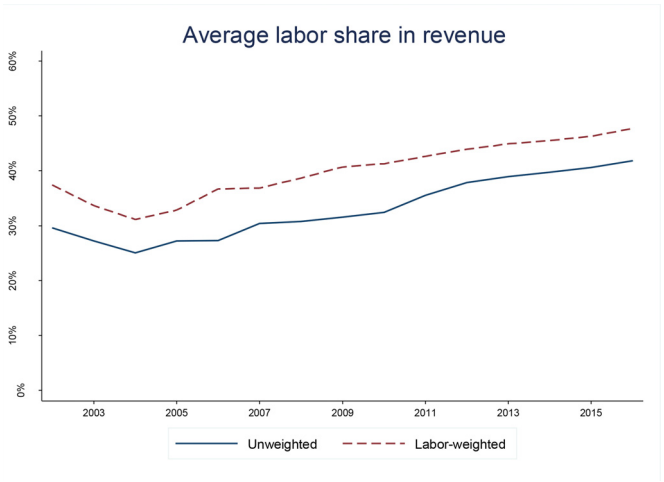

Fig. 2. Average labor share in revenue.
Source: Based on Annual Economic Activity Survey - INE.

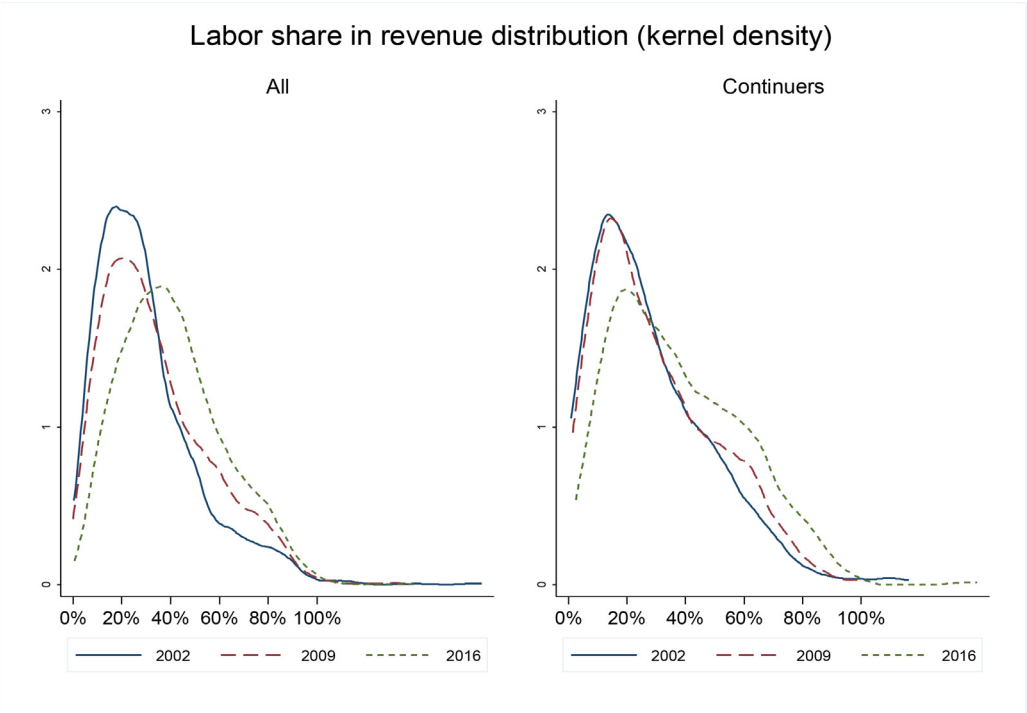

Fig. 3. Labor share in revenue (kernel density).
Source: Based on the Annual Economic Activity Survey - INE.

the subset of continuing firms: wages evolve substantially above labor productivity in line with mandated increases (Figs. A1, A2 and A3 in Appendix).

Differences between growth rates of wages and apparent labor productivity arithmetically translate into an increasing labor share of revenue (labor share wL/Y being the ratio between average wage and labor productivity: $w/(Y/L)$). Fig. 2 shows labor share of revenue increasing steadily after 2005 from 27% to 42% (simple average) and from 33% to 48% (employment-weighted average).

The change in the labor share is pervasive and also present in data by firm size, sector of activity and for the subset of continuing firms (Appendix Figs. A4, A5 and A6). Fig. 3 shows firm labor share distribution for 2002, 2009 and 2016. The kernel density moves to the right, and the higher labor share area increases.

3. Literature review

This paper is based on the production approach to measuring product markups and wage markdowns, and it is linked to the labor share of income analysis literature, the wage bargaining effects and rent sharing literatures, and to research on the Uruguayan labor market.

De Loecker et al. (2020) investigate firms' product markups for the US economy using firm-level data. They document a rise in product markups between 1980 and 2016, mainly driven by sharp increases in the upper tail of the distribution. High product markup firms also gain market share. They conclude that this has an important role is the decline in the labor share. They do not propose an explanation for the observed reallocation of economic activity towards high-product markup firms, though they point to changes in technology and changes in market structure due to weaker antitrust enforcement as possible suspects in a different paper (De Loecker et al. 2018).

De Loecker and Eeckhout (2018) analyze data from 134 countries. They estimate that the average global product markup had risen from around 1.1 in 1980 to 1.6 in 2016. North America and Europe show the largest increases.[6] Although Uruguay is not part of the sample, Latin American countries do not display a clear pattern in their average product markup evolution.

Several papers have related product markups to different dimensions of firm behavior and performance. For instance, De Loecker and Warzynski (2012) analyze the link between product markups and export behavior and find that, on average, larger product markups are observed in exporters, and product markups increase when firms start to export. De Loecker et al. (2016) study trade liberalization in India and conclude that it lowers firm prices and is associated with pro-competitive effects. They find, however, that price declines are small when compared to falls in marginal costs due to input tariff liberalization.

The literature on the labor share in GDP is summarized in Autor et al. (2017). After decades-long stability of the labor share, a noticeable decline began in the United States in the 1980s. The explanations advanced include trade shocks and changes in relative factor costs. Autor et al. (2017) propose the superstar firm explanation. If production uses a fixed amount of overhead labor plus a size-dependent variable labor input, or if product markups in the final goods market correlate positively with firm size, then larger firms would have lower labor shares.

Autor et al. (2020) analyze US micropanel data to conclude that the fall in the labor share is explained by reallocation between firms and not by a fall in the unweighted mean labor share across all firms. They also claim that the aggregate product markup increases more than the typical firm's product markup, and that such patterns are present not only in US firms, but also in other developed economies. Elsby et al. (2013) find that offsetting industry changes are behind the aggregate stability of the labor share before the 1980s. They find that data do not support explanations based on substitution of capital for unskilled labor, and that there is no conclusive evidence on the influence of institutional factors such as the decline in unionization. However, they find that offshoring of the labor-intensive component of firms' supply chains could be a potential explanation.

Closely related to our approach, Mertens (2022) using data for German manufacturing (1995–2014) reports a strong fall in the labor share of revenue and a stable product markup. It measures and analyzes separately wage markdowns and finds an increase in firm's labor market power. Based on the production side approach to markup estimation, it measures and analyzes wage markdowns. Mertens (2022) finds that output elasticities are not stable, and reports that 70% of the change in the labor share is due to firms moving towards less labor-intensive production technologies, and only the remaining 30% to changes in product or labor market power.

A recent literature analyzes market power in the labor market. Dobbelaere and Kiyota (2018) argue that the reduced form estimation strategy followed by Mertens (2020, 2022), Morlacco (2019), Yeh et al. (2022) and themselves among others can nest different labor market models in which market power can be on the side of workers or employers. Their estimated product markups and wage markdowns are used to classify firms according to product and labor market functioning types. They conclude that pricing in product and labor markets differs across firms according to their links to the international economy. In turn, Mertens (2020) studies the impact of international trade on firm's labor market power. Using data on German trade with China, he concludes that increases in export demand expand labor market power of firms, and final product import competition conversely strengthens employees' labor market power.

Most of the older wage bargaining literature is focused on monopoly power on the part of workers, or at least envision a negotiated outcome in which workers and firms exercise some bargaining power. Recent literature stresses monopsony power on the part of firms, *i.e.,* they do not face an infinitely elastic labor supply curve. In competitive models, equilibrium is reached through perfect mobility, so wage gaps between firms vanish. Monopsonistic models can be justified on the basis that workers facing substantial costs when moving between firms: searching, moving, training, etc. Manning (2020) provides a review of the literature on estimation of the labor supply elasticity and the potential of the monopsony perspective for understanding labor market issues and inequality.

Our research is also related to the rent sharing literature, of which Card et al. (2018) provides a useful summary. Product market competition limits the ability of unions to obtain gains for their members. Wage differentials across firms have been found to be correlated with differences in productivity. However, they may not be interpreted directly as rent sharing

[6] Exceptions to this increase include De Loecker et al. (2018) for Belgium, Mertens (2022) for Germany and van Heuvelen et al. (2021) for the Netherlands.

Table 1
Firms per year.

2002	1,835
2003	1,900
2004	1,955
2005	1,992
2006	732
2007	1,067
2008	1,408
2009	1,379
2010	1,709
2011	3,011
2012	3,409
2013	3,064
2014	2,884
2015	2,990
2016	2,871
Total	32,206

Source: Annual Economic Activity Survey.

because of difficulties in measuring worker quality. Several papers have tried to measure the extent to which quasi rents and product market conditions influence wages paid by firms.[7]

Finally, several authors analyzed Uruguayan labor markets. Allen et al. (1996) and Cassoni et al. (2000) study a previous re-unionization experience 20 years before our analysis, when wage councils were reinstated in 1985 by the first democratic government after more than a decade of military dictatorship. They find that wages increased more in union than in non-union sectors, but that employment grew more in non-union sectors. The economic environment mirrors the period analyzed in our paper, since Uruguay went through a deep contraction after a financial crisis and devaluation in 1982, and centralized wage council negotiation was reinstated coincidently with economic recovery. Regarding the 2005 general minimum wage increase, Borraz and González-Pampillón (2017) find that it contributed to the reduction of wage inequality for formal workers. It also had a small negative impact on employment outside the capital of Montevideo and on working hours.

4. Data

We assemble a unique data set by matching multiple sources of information, including firm-level surveys from INE, sector-level mandated minimum wages of wage councils -from the Ministry of Labor and Social Security- and sector unionization rates computed by merging data from the Uruguayan trade union central and household surveys.

4.1. Annual economic activity survey

Firm-level data for manufacturing and services sectors from the Annual Economic Activity Survey by the INE were available for 2002–2016. The survey aims to represent firms in the local economy, though some sectors as agriculture, banking, construction, household work and extraterritorial organizations are not included.

Within each four-digit ISIC sector, all firms above given employment or sales thresholds (compulsory range) are included in the sample, while a probabilistic sample is drawn from the set of firms below. The INE periodically revises sample coverage and includes new firms using listings from the social security institute and tax authority.[8] The resulting unbalanced panel includes consistent annual data on sales, production, labor (number of workers), capital and intermediate inputs (such as electricity, fuel, water and materials). Firm prices are not available and nominal values are deflated using sector-level price indexes.

Following Mertens (2022) we restrict our analysis to firms with non-negative estimated labor and material output elasticities. As shown in Table 1, there are non-negligible variations in sample sizes by year. We report the number of observations with complete information to estimate firm price markups according to our methodology. Overall, there are 32,206 observations corresponding to 4,613 different firms. Each firm was observed on average 7 times within the 15-year window of this study.

295 firms for which a price markup can be estimated are present in the sample every year. We refer to them as “continuers”. They survived the whole period, were always in the INE sample and have non-negative estimated labor and materials elasticities of output. Non-continuation does not necessarily mean exiting the market. In order to check that results are not driven by composition effects due to resampling, they are presented both for all firms and the set of continuers (in the paper text or in the Appendix).

[7] Abowd and Lemieux (1993), Barth et al. (2016), Crépon et al. (2005), Hildreth and Oswald (1997), Estevão and Tevlin (2003) and Van Reenen (1996), for instance.

[8] Changes in sampling sizes and strata thresholds are observed through our sample period. A detailed report is provided in Appendix C.

4.2. Wage council data

The Labor and Social Security Ministry of Uruguay compiles a database (MTSS-CSA) recording wage increases set in sector agreements by negotiation group at various levels. Broader wage councils differentiate between private and public sectors, rural activities and domestic work. Within the private sector, groups are divided in subgroups, for instance the "leather clothing and footwear industry" subgroup comprises sub-subgroups for tanneries, footwear, leather clothing, etc.

Wage councils agree on wage levels and increases for a broad set of job categories within each group, according to working conditions and technology. The MTSS-CSA database calculates wage indexes, with base January 2005=100, for different worker categories, including workers in job-categories earning the minimum level. From Blanchard et al. (2021) we have access to minimum wage levels for July 2005. Applying variations in the MTSS-CSA index to these data we compute a panel of minimum wages with time and cross section variation by negotiation group.

There is not a one-to-one correspondence between ISIC sectors and wage council negotiation groups. If a given firm has differentiated activities, it may be subject to agreements of more than one group. For instance, supermarket workers' wages are determined in the retail group, but if the supermarket bakes bread, wages of bakery workers are set in a different group.

To match wage council negotiation groups and ISIC Rev 4 classification (4 digits) we use correspondence tables provided by IECON.[9] In general, more than one wage council group is matched to a single ISIC code, and a single group may set wages for firms in many ISIC codes. For each ISIC code in the database, we compute the average of mandated wage minimums for all groups linked to that ISIC code. Then each firm in the Annual Economic Activity Survey is matched to the mandated minimum wage corresponding to the 4-digit ISIC sector in which it is classified.

From 2005 onward, our working database has 26,516 firms. We can match a minimum mandated wage to 79% of them.

4.3. Unionization data

We construct sector unionization density measures as the affiliates/employment ratio of a given broad sector. Unions do not always correspond to precisely defined ISIC codes, though they are organized within economic activities in a broad sense. Sometimes a union covers workers from more than one ISIC code. In other cases, more than one union has affiliates in the same ISIC category. We consider broad sectors that do not exactly match two digit or three-digit ISIC codes and are consistent over time.

INE included in the household survey, in 2007 only, a specific question on individual affiliation to unions. We obtain a cross section indicator of unionization density by broad sector (ratio of unionized to total employees) at this point in time.

Using data on the number of delegates to central congresses of the trade union central PIT-CNT, we estimate sector trade union affiliates (delegates being allocated proportionally to number of dues-paying members). Congresses were held every two to three years. Membership figures were linearly interpolated to obtain an annual measure of sector union membership. Union density figures by sector and year were obtained as the ratio of membership to yearly employment from the Uruguayan Household Survey.

We updated the 2007 INE cross section indicators of union density by applying variations in the ratio of union affiliates to employment. We obtained sector union density estimates for 61% of the firms in our sample after 2005.

Union density measures are affected by changes in affiliation and in employment. Periods of stagnant union membership with decreasing employment such as years after the 2002 crisis are associated with increases in unionization rates. When employment recovers after 2004 we observe the opposite effect. After 2005, there is a clear increase in membership. However, this does not translate to an increase in unionization density after the initial years, since employment growth may have exceeded the increase in affiliations. Over the whole period there is considerable sector-level variation in both the level of unionization and its changes over time (See Figs. A7 and A8 in the Appendix).

5. Methodology

5.1. Product markup and wage markdown measurement

Our approach is based on Mertens (2022), which extends DeLoecker and Warzynski (2012) to obtain expressions for firm product market markups and labor market wage markdowns.

For firm i at period t we assume a production technology given by:

$$Q_{it} = Q_{it}(L_{it}, M_{it}, K_{it}, \omega_{it}) \tag{1}$$

where L_{it} and M_{it} are labor and materials, respectively, K_{it} is capital, ω_{it} is a scalar productivity term and Q_{it} is gross output. For cost minimization, the following Lagrangian can be written:

$$\mathcal{L} = w_{it}(L_{it})L_{it} + pm_{it}M_{it} + r_{it}K_{it} + \lambda_{it}(Q_{it} - Q_{it}(\cdot)) \tag{2}$$

where w_{it}, pm_{it} and r_{it} are prices for labor, materials and capital, respectively.

[9] IECON stands for Instituto de Economía of the Universidad de la República. Among others, Blanchard et al (2021) and Brum and Perazzo (2020) used this correspondence tables.

We assume that material inputs prices are exogenous to firms, and they are flexible in the sense of not being subject to adjustment costs. Labor is also assumed to be flexible, but instead of an exogenously given wage, firm monopsony power is assumed in the labor market, *i.e.*, $w_{it}(L_{it})$ is a positively sloped function.

First order conditions for material inputs and labor are:

$$\frac{\partial \mathcal{L}}{\partial M_{it}} = pm_{it} - \lambda_{it}\frac{\partial Q_{it}}{\partial M_{it}} = 0 \tag{3}$$

$$\frac{\partial \mathcal{L}}{\partial L_{it}} = \frac{\partial w_{it}}{\partial L_{it}}L_{it} + w_{it} - \lambda_{it}\frac{\partial Q_{it}}{\partial L_{it}} = 0 \tag{4}$$

where λ_{it} represents marginal cost at a given level of output.

Rearranging, we obtain a relation between output elasticity of materials (θ_{it}^{M}) and product markup over marginal cost μ_{it}:

$$\theta_{it}^{M} = \frac{\partial Q_{it}/\partial M_{it}}{Q_{it}/M_{it}} = \frac{P_{it}}{\lambda_{it}}\frac{pm_{it}M_{it}}{P_{it}Q_{it}} = \mu_{it}\frac{pm_{it}M_{it}}{P_{it}Q_{it}}. \tag{5}$$

where P_{it} is firm's output price. In other words, the expression for the product markup can be written as:

$$\mu_{it} = \theta_{it}^{M}\left[\alpha_{it}^{M}\right]^{-1} \tag{6}$$

where α_{it}^{M} is the materials share of revenue.

For labor, we obtain:

$$\theta_{it}^{L} = \frac{\partial Q_{it}/\partial L_{it}}{Q_{it}/L_{it}} = \left[\frac{\partial w_{it}}{\partial L_{it}}\frac{L_{it}}{w_{it}} + 1\right]\frac{P_{it}}{\lambda_{it}}\frac{w_{it}L_{it}}{P_{it}Q_{it}} = \left[\varepsilon_{Sit}^{-1} + 1\right]\mu_{it}\alpha_{it}^{L} \tag{7}$$

where ε_{S}^{-1} is the inverse elasticity of labor supply. Profit maximization in a monopolistic labor market implies that $[\varepsilon_{Sit}^{-1} + 1]$ equals the wage markdown (υ_{it}) defined as the ratio between marginal revenue of labor $MRPL_{it}$ and the wage w_{it}.

Then,

$$\upsilon_{it}\mu_{it} = \theta_{it}^{L}\left[\alpha_{it}^{L}\right]^{-1} \tag{8}$$

where α_{it}^{L} is the labor share of revenue.

If the labor market is assumed in perfect competition, as in DeLoecker and Warzynski (2012), then $\theta_{it}^{L}[\alpha_{it}^{L}]^{-1}$ would be interpreted as a measure of product markup. Instead, in our setting this term is the interaction of product market markup and wage markdown, $\upsilon_{it}\mu_{it}$. Thus, assuming perfect competition would lead to estimates confounding two possible sources of firm power: in the final goods market and in the labor market.

A flexible input can be adjusted without extra costs. With adjustment costs, minimization of dynamic costs is more relevant, and deviations can no longer be interpreted as a pure measure of market power. Morlacco (2019) investigates market power in input trade. Using French data, she finds support to the assumption of competitive intermediate input markets, insofar the imported share of intermediates is not too high. Similarly, De Loecker et al. (2018) in its estimation of product markups in Belgium stresses the importance of using a flexible input and distinguishes between service and goods material inputs, arguing that the former is more likely to be fixed or costly adjusted and the latter more likely to be flexible.

Casacuberta and Gandelman (2012) address adjustment costs in Uruguayan manufacturing in a context of trade liberalization. Sectors experiencing stronger tariff reductions adjust a lower fraction of the gap between desired input levels and actual usage in the creation side (reducing shortages—i.e., hiring employees or investing) but a larger fraction in the destruction side (reducing surpluses—i.e., firing employees, scrapping or letting capital depreciate).

As Mertens (2020) argues, adjustment frictions are a precondition for market imperfections to arise: on "monopsonistic labor markets, firms exploit worker-side adjustment costs like moving costs or other local preferences to pay wages below marginal revenue products of labor. On the other hand, worker-side labor market power can only exist if firms cannot freely adjust wages and employment."

In Uruguay, we do not claim that the only source of market imperfections is market power on the firm side, particularly when we are assessing the impact of wage bargaining and unions on firm outcomes. In that sense, we adopt Mertens (2022) interpretation according to which the gap between marginal revenue of labor $MRPL_{it}$ and wage w_{it} can respond to rents towards the firm (ratio $\upsilon_{it} > 1$) or towards its employees ($\upsilon_{it} < 1$) and, although we refer to it as wage markdown, we acknowledge that it may condense influences from different sources.

Thus, our wage markdown measure incorporates sources of adjustment frictions that are related to the labor market power, consistently with Casacuberta and Gandelman (2012). On the other hand, in the period under study changes in adjustment frictions that are unrelated to labor market power were not clearly present. For instance, there were no changes in institutional employment protection policies. In Uruguay labor contracts are not subject to duration restrictions nor dismissal previous notices are required, while firing costs remained constant during the period.

In what follows our measure of firms' product markup is:

$$\mu_{it} = \theta_{it}^{M}\left[\alpha_{it}^{M}\right]^{-1} \tag{9}$$

and our measure of firms' wage markdown is:

$$\upsilon_{it} = \theta_{it}^{L}\left[\alpha_{it}^{L}\right]^{-1}\alpha_{it}^{M}\left[\theta_{it}^{M}\right]^{-1} \tag{10}$$

where material and labor shares of revenue are directly observable in the data and output elasticities are estimated.

5.2. Production function estimation

Output elasticities estimation requires a consistent estimate of production function parameters. It is usual to use control function-based estimators to correct for biases from endogeneity of factor use choices by firms.

The Cobb-Douglas specification assumes labor, materials and capital elasticities of output to be constant across firms in a given sector and time. For this paper, this assumption is problematic since changes in labor and materials shares of revenue ($\alpha_{it}^{L},\ \alpha_{it}^{M}$) would be automatically passed on to changes in market power. Thus, we adopt a translog specification in which elasticities are allowed to vary by firm and period. Our production function is then defined as:

$$y_{it} = \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + \beta_{ll} l_{it}^2 + \beta_{mm} m_{it}^2 + \beta_{kk} k_{it}^2 + \beta_{lk} l_{it} k_{it} + \beta_{lm} l_{it} m_{it} + \beta_{km} k_{it} m_{it} + \beta_{lkm} l_{it} k_{it} m_{it} + \omega_{it} + \varepsilon_{it} \tag{11}$$

where y_{it} is gross output, $l_{it},\ m_{it}$ and k_{it} are labor, materials and capital inputs respectively (all in logs), while ω_{it} is unobserved Hicks-neutral productivity, assumed to follow a first-order Markov process, and ε_{it} a sequence of shocks conditional mean independent of current and past input choices. As Wooldridge (2009) (and also Levinsohn and Petrin, 2003 and Olley and Pakes,1996) we assume a productivity control function such that

$$\omega_{it} = g(k_{it},\ m_{it}). \tag{12}$$

Wooldridge (2009) proposes a single stage method based on the following assumptions:

$$E(\varepsilon_{it}|\ l_{it},\ k_{it},\ m_{it},\ l_{it-1},\ k_{it-1},\ m_{it-1}, \ldots,\ l_{i1},\ k_{i1},\ m_{i1}) = 0;\ t = 1, .., T \tag{13}$$

$$E(\omega_{it}|\ k_{it},\ l_{it-1},\ k_{it-1},\ m_{it-1}, \ldots,\ l_{i1},\ k_{i1},\ m_{i1}) = E(\omega_{it}|\ \omega_{it-1}) \tag{14}$$

along with k_{it} being uncorrelated with the productivity innovation

$$\psi_{it} = \omega_{it} - E(\omega_{it}|\ \omega_{it-1}). \tag{15}$$

The additional condition is imposed that:

$$E(\omega_{it}|\ \omega_{it-1}, \ldots, \omega_{i1}) = E(\omega_{it}|\ \omega_{it-1}) = f[g(k_{it-1},\ m_{it-1})]. \tag{16}$$

The two equations that identify the production function parameters are:

$$y_{it} = h(l_{it}, m_{it}, k_{it}) + g(k_{it},\ m_{it}) + \varepsilon_{it} \tag{17}$$

$$y_{it} = h(l_{it}, m_{it}, k_{it}) + f[g(k_{it-1},\ m_{it-1})] + \zeta_{it} \tag{18}$$

where $\zeta_{it} = \varepsilon_{it} + \psi_{it}$. Following Petrin et al. (2004), functions $g(k_{it},\ m_{it})$ and $f[g(k_{it-1},\ m_{it-1})]$ are approximated by third degree polynomials in $k_{it},\ m_{it}$, including all terms of the form $k_{it}^{p} m_{it}^{q}$, where $p \geq 0$, $q \geq 0$ and $p + q \leq 3$. Instruments for the first equation are contemporaneous capital k_{it}, labor l_{it}, material inputs m_{it} and the polynomial approximating $g(k_{it},\ m_{it})$ but excluding k_{it}. For the second equation contemporaneous capital k_{it}, lags of labor and materials inputs l_{it-1} and m_{it-1}, lags of the polynomial and functions of them can be used as instruments. We estimate both equations simultaneously using the generalized method of moments.[10]

We recover labor and materials elasticities of output for firm i in period t by computing

$$\theta_{it}^{L} = \hat{\beta}_l + 2\hat{\beta}_{ll} l_{it} + \hat{\beta}_{lm} m_{it} + \hat{\beta}_{lk} k_{it} + \hat{\beta}_{lkm} k_{it} m_{it} \tag{19}$$

$$\theta_{it}^{M} = \hat{\beta}_m + 2\hat{\beta}_{mm} m_{it} + \hat{\beta}_{lm} l_{it} + \hat{\beta}_{km} k_{it} + \hat{\beta}_{lkm} k_{it} l_{it} \tag{20}$$

In these estimates, production function coefficients are taken to be common within each sector. As they depend not only on estimated coefficients but also on factor levels, output elasticities for each factor differ by firm and time.

[10] The Ackerberg et al. (2015) critique of the Levinsohn and Petrin (2003) and Olley and Pakes (1996) methods refers to an identification problem for the labor coefficient in their first estimation stage if labor inputs are chosen at the same time as intermediate inputs. The Wooldridge (2009) single step method addresses this problem, but still maintains the scalar productivity assumption. This paper assumes there is markdown and markup variation across firms, so scalar productivity may not be the only source of serially correlated variation in input demand. High labor usage does not necessarily indicate a firm is more productive, since it could also have low product or input market power. Estimations must be interpreted cautiously (See Doraszelski and Jaumandreu, 2021).

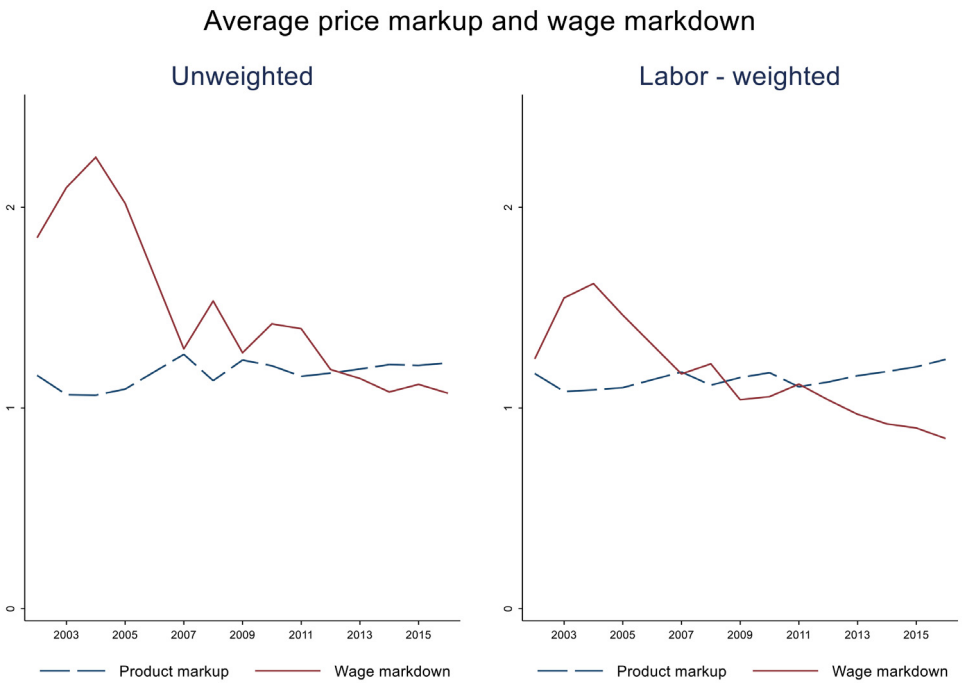

Fig. 4. Average price markup and wage markdown.
Source: Based on the Annual Economic Activity Survey of the INE.

ISIC aggregation level for production function estimation is, as usual in the literature, defined for manufacturing sectors at the 2-digit ISIC code level. Services are less often included in empirical work. Our sector definition for services is more aggregate, being defined at ISIC section levels, encompassing several 2-digit codes. We include: Water supply; sewerage, waste management and remediation activities (section E), Wholesale and retail trade (section G), Transportation and storage (section H), Information and communication (section J), Professional, scientific and technical activities (section M), Administrative and support service activities (section N), Education (section P), Human health and social work activities (section Q), Arts, entertainment and recreation (section R) and Other service activities (section S). For each of these sectors we estimate a translog production function.

Table B1 in the Appendix reports average, median and standard deviations for estimated firm-level elasticities and implied returns to scale, by sector. These estimates use all observations in the sample. In turn, Table B2 in the Appendix reports the same statistics by year for the set of continuers.[11] Results are remarkably stable over time. This is interesting in the light of the results in Mertens (2022) pointing to changes in output factor elasticities as an important factor in explaining changes in the labor share. Though this is not the main focus of our paper, we find that we do not have a *prima facie* case for technology to be a main factor in changes in the labor share.

6. Results

6.1. Product markups and wage markdowns

Fig. 4 reports annual averages for firm-level price markups and wage markdowns. They correspond to the sample counterparts of parameters μ_{it} in Eq. (9) and υ_{it} in (10). We present unweighted statistics (left-hand panel) and employment-weighted averages (right-hand side) as it is customary in the markup literature.

A magnitude of 1.2 for μ_{it} indicates that a firm is able to price its product 20% above marginal cost. Our average product markup estimates are procyclical, tending to follow output recovery after the 2002 crisis. Though initially they decrease, they are on average 26% higher in 2016 than in 2005, according to unweighted results.

We also find a decreasing path of wage markdowns along the sample period (*i. e.* workers' wages get closer to the value of marginal product of labor). Initial average wage markdowns υ_{it} above 2 imply workers earned less than 50 cents of each peso they generated. By the end of the sample period, unweighted mean wage markdown estimates of 1.07 imply that on

[11] Translog production function coefficients are the same as in table B1 but the elasticities are evaluated only for continuing firms.

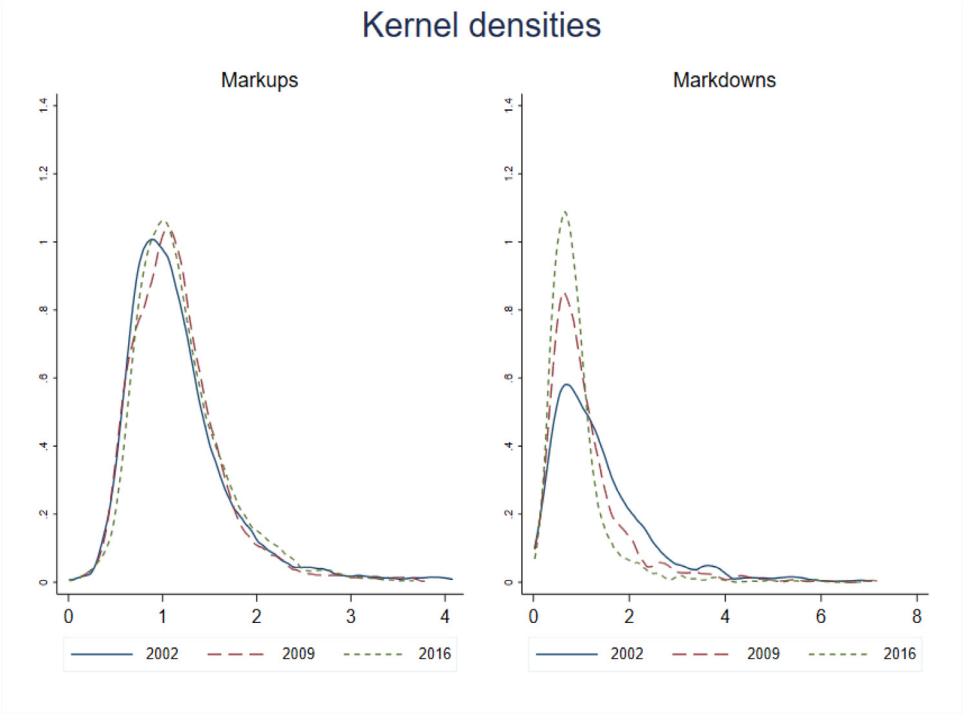

Fig. 5. Kernel densities.
Source: Based on the Annual Economic Activity Survey by the INE.

average they earned 83 cents of each peso they marginally generate. Yeh et al. (2022) for instance estimate an average wage markdown of 1.53 for US firms close to our whole period (simple) average of 1.48.

Labor weighted estimates reported in the right-hand side of Fig. 6 show lower average wage markdowns. In the last years of our study, they are actually below 1, implying that, marginally, workers started to earn above the value of their marginal productivity.

The diverging paths of estimated wage markdowns and product market markups also indicate that had we used labor to recover markups -as for instance DeLoecker and Warzynski (2012) does-, our results would indeed have been very different. Our product markup estimates would in fact have been the product of our actual product markup times our wage markdown, and closer to our wage markdowns than to our product markup estimates.

For robustness, Fig. A9 in the Appendix reports a similar picture based on median values, while Fig. A10 does it for average values of the set of continuing firms. The Appendix also includes separate plots of average price markups and wage markdowns by firm size and sector, showing a consistent picture (Figs. A11 and A12).

Tables B3 and B4 in the Appendix report average, median and standard deviation of price markups and wage markdowns by sector of activity (unweighted and labor-weighted respectively).

6.2. Heterogeneity

De Loecker et al. (2020) find that a relatively small number of large firms drive the increase in the average product markup, while there are no changes in median firm-level product markups. Fig. 5 plots the kernel density of estimates for 2002, 2009 and 2016. We find that there are no significant shifts in the distribution of product markups across firms, but a clear leftward shift in wage markdowns. Thus, it is not a matter of only a set of large (or small) firms. Fig. A13 in the Appendix reports a similar pattern for the set of continuing firms.

To further explore wage markdown heterogeneity, following Yeh et al. (2022) we compute a simple decomposition of wage markdown variance based on the multiplicative expression for υ_{it}, which can be expressed in logarithms as:

$$ln\upsilon_{it} = ln\theta_j^L - ln\alpha_{it}^L - ln\mu_{it} \tag{21}$$

This gives:

$$V(ln\upsilon_{it}) = V\left(ln\theta_j^L\right) + V\left(ln\alpha_{it}^L\right) + V(ln\mu_{it}) - 2\left[COV\left(ln\theta_j^L, ln\alpha_{it}^L\right) - COV\left(ln\alpha_{it}^L,\ ln\mu_{it}\right) + COV\left(ln\theta_j^L,\ ln\mu_{it}\right)\right] \tag{22}$$

From Table 2 we obtain that both variance in output elasticities and in labor shares contribute significantly to the overall wage markdowns variance, while variance in product markups contributes to a lower extent. Very small covariances between product market power and both labor share and output labor elasticity are also observed.

Table 2
Variance in firm-level wage markdowns.

	Variance	Contribution
Var (υ)	0.635	1.000
Var (θ^L)	0.546	0.860
Var (α^L)	0.578	0.910
Var (μ)	0.218	0.343
Cov (θ^L, α^L)	0.320	−1.006
Cov (α^L, μ)	−0.007	−0.021
Cov (θ^L, μ)	0.027	−0.085

Table 3
Product markups and wage markdowns over time.

	ln product markup	ln wage markdown
d2002	0.067***	−0.094***
	(0.009)	(0.013)
d2003	−0.004	0.111***
	(0.009)	(0.013)
d2004	−0.003	0.110***
	(0.009)	(0.013)
d2006	0.056***	−0.126***
	(0.012)	(0.019)
d2007	0.086***	−0.159***
	(0.011)	(0.016)
d2008	0.055***	−0.141***
	(0.010)	(0.015)
d2009	0.084***	−0.266***
	(0.010)	(0.015)
d2010	0.099***	−0.351***
	(0.009)	(0.014)
d2011	0.045***	−0.306***
	(0.008)	(0.013)
d2012	0.057***	−0.368***
	(0.008)	(0.013)
d2013	0.070***	−0.439***
	(0.008)	(0.013)
d2014	0.087***	−0.470***
	(0.008)	(0.013)
d2015	0.078***	−0.501***
	(0.008)	(0.013)
d2016	0.091***	−0.559***
	(0.008)	(0.013)
Constant	0.004	0.213***
	(0.006)	(0.010)
Firms fixed effects	Yes	Yes
R-squared	0.012	0.153
Observations	32,206	32,199
Number of firms	4,613	4,613

Note: Standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

6.3. The impact of sectoral bargaining

To analyze the time path of our market power indicators, we separately regress firms' product markups and wage markdowns on a set of year dummies and fixed effects with the 2005-year dummy as the omitted category.[12] Results are shown in Table 3.

The decreasing pattern in wage markdowns is revealed by the increasing coefficients (in absolute value) of time dummy variables in wage markdowns' estimates across the years. By 2016, firm wage markdowns had decreased 43 percent on average with respect to their 2005 values.[13]

On the other hand, the estimated year dummy coefficients in the firm product markups equation increase. In 2005–2010 after initial growth, they seem to stabilize. Over 2005–2016, the increase in product markups is 10%.

Therefore, while firms bargaining power in the labor markets weakened, they seemed to have been able to pass part of their increased labor costs to consumers.

[12] Table B5 in the Appendix presents results (very similar in magnitude and sign) for the subset of firms for which wage council data is available.

[13] Since the dependent variable is in logs, the estimated coefficient for the 2016 dummy indicates markdown decreased by $e^{-0.559} - 1$.

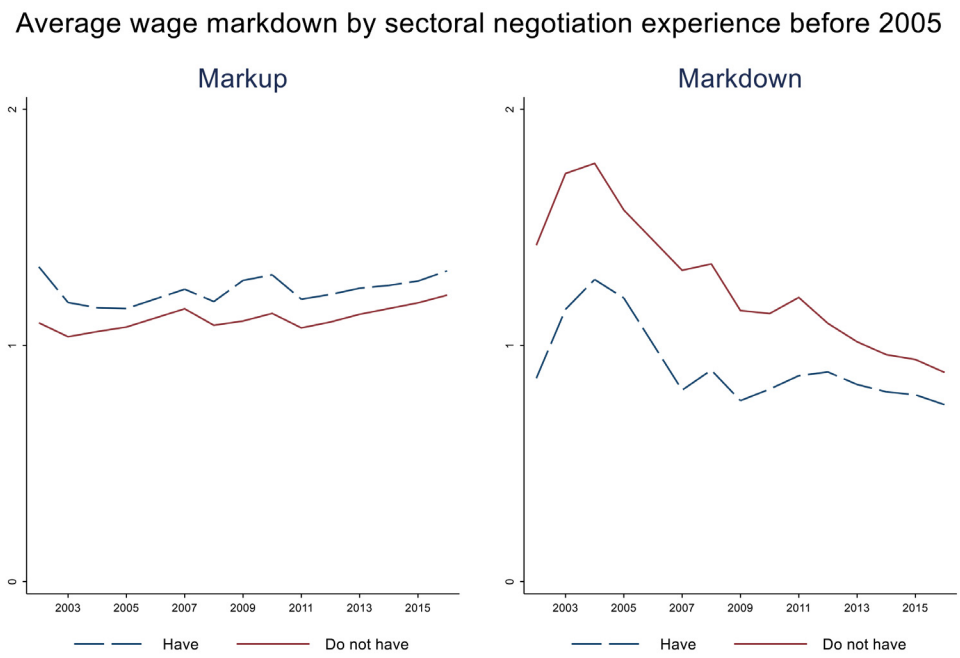

Fig. 6. Labor-weighted.

Even without considering mandated minimum wages, we can analyze whether the introduction of a centralized bargain setting may *per se* have had an effect on wage markdowns, for instance by improving the relative bargaining strength of workers. Also, it may have impacted product market markups, for example if workers and employers aligned to promote a coordinated passthrough of wages to prices.

While before 2005 there were no wage councils with government participation, some sectors had other forms of sectoral bargaining (Mazzuchi, 2009).[14] We can distinguish two types of firms: those in sectors that had some sort of sectoral collective bargain before 2005, and those in sectors that did not have sectoral bargain before 2005 (we cannot distinguish whether there was collective bargaining at the firm level or no wage bargaining at all). Sectors without sector-level pre-2005 bargaining would have been more exposed to the effects of council reinstatement and somehow subject to a treatment (compulsory collective three-part sector bargaining) while the rest can be considered the control group subject only to change of their sectoral two-part bargaining to three-part now including the government.[15]

We show in Fig. 6 markups and markdowns for sectors with sector-level bargaining experience prior to 2005 ("have") and for sectors without sectoral bargaining before 2005 (“do not have”). It can be observed that despite the time trends look parallel in general, initial markdowns values are visibly larger for sectors without sector bargaining, and the gap tends to close post 2005. Unweighted results are shown in Fig. A14 in the Appendix.

To formally check significance of the results, we run a diff-in-diff estimation as:

$$ln\mu_{it} = \beta_1 Treat + \beta_2 Treat * After + \rho_1 X_{it} + \delta_i + \delta_t + \zeta_{it} \tag{23}$$

$$ln\upsilon_{it} = \beta_1 Treat + \beta_2 Treat * After + \rho_2 X_{it} + \delta_i + \delta_t + \zeta_{it} \tag{24}$$

where $ln\upsilon_{it}$ and $ln\mu_{it}$ are firm-level indicators of wage markdown and firm product markup respectively (in logs). Variables in X are dummies for firm size (taking value of 1 if the firm belongs to the second, third or fourth quartiles of output respectively) and the log of the firm's capital-labor ratio. δ_i and δ_t are firm fixed effects and year effects respectively. *Treat* is an indicator variable taking value 1 if the firm is in sectors without sector bargaining prior to 2005, and After a dummy variable taking value 1 for observations in which year is greater or equal to 2005. The parameter of interest is β_2 and can be interpreted as the differential in product markups (wage markdowns) induced by the wage negotiation setting change.

Table 4 reports a difference-in-difference statistically significant effect. Even without considering differences in mandated wages, the sole implementation of wage councils is associated with decreases in wage markdowns of −0.14 log points and increases in price markups of 0.13 log points.

[14] We than G. Mazzuchi for this list, which we include in appendix D.

[15] We thank an anonymous referee for suggesting this approach. We acknowledge this is not a bullet-proof identification strategy due to endogeneity concerns about sectors with and without sectoral bargaining conditions before 2005.

Table 4
Impact of sectoral bargaining on product markups and wage markdowns.

	Product markups	Wage markdowns
After	−0.179***	0.230***
	(0.016)	(0.025)
Treat	0.223	−0.675**
	(0.197)	(0.310)
After*Treat	0.131***	−0.142***
	(0.015)	(0.024)
Dummy size (second quartile)	0.122***	−0.099***
	(0.008)	(0.012)
Dummy size (third quartile)	0.196***	−0.197***
	(0.010)	(0.016)
Dummy size (fourth quartile)	0.234***	−0.235***
	(0.013)	(0.021)
ln capital to labor ratio	−0.014***	0.066***
	(0.002)	(0.004)
Constant	−0.115	0.095
	(0.173)	(0.271)
Firms fixed effects	Yes	Yes
Time effects	Yes	Yes
R-squared	0.037	0.179
Observations	25,702	25,695
Number of firms	3,639	3,639

Note: After is a dummy for 2005–2016, Treat is a dummy for sectors with no sectorial bargaining before 2005. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

6.4. The impact of minimum mandated wages

Now we turn to the impact of minimum mandated wages set at the wage councils. Identification is based on the assumption that mandated wages are exogenous to firms. We estimate the following models:[16]

$$ln\mu_{it} = \alpha + \beta_1 ln(mw)_{jt} + \rho_1 X_{it} + \delta_i + \delta_t + \zeta_{it} \quad (25)$$

$$ln\upsilon_{it} = \alpha + \beta_2 ln(mw)_{jt} + \rho_2 X_{it} + \delta_i + \delta_t + \varepsilon_{it} \quad (26)$$

where $ln(mw)_{jt}$ is log sector mandated minimum wage, and the other terms are defined as above.

Our impact variable of interest is measured at the sector level. Hence exogeneity requires that individual firms are not able to influence the sector level outcome. Lack of representativeness of wage councils has been widely discussed in Uruguay. The law indicates that elections of representatives must be held and the Social Security and Labor Ministry should maintain a voter registry for active workers and employers. This however was not feasible, and the government summoned "the most representative organizations" of employers and workers to set negotiations (Decree 105/05). Fernández et al. (2008) indicates that it is generally understood that representatives of both sides are disproportionally recruited among employers and workers of the largest firms in each sector.

Hence, we regress our firm level product markups and wage markdowns on mandated wages defined at the sector level. In principle we do that for the whole set of firms, though, as a robustness check, we also perform our estimations in a reduced database where we drop the largest firm of each 4-digit ISIC code.

In Table 5, column (A) for the whole sample shows an elasticity of wage councils mandated wages to markups of 0.139. Column (B) reproduces the previous estimation excluding the largest firm of each 4-digit ISIC sector. The elasticity is 0.129 (confidence intervals in both estimates overlap). Given that average real mandated wages doubled after 2005, the reported elasticity accounts for all the 10% increase in product markups reported in Table 3.

Column (C) reports an elasticity of minimum mandated wage of −0.149, almost the same magnitude shown by column (D) for the estimation excluding the largest firm for each 4-digit sector. Given real mandated wages doubling, this implies that wage markdowns were reduced by wage councils between 15% and 10%. This is between a third and a quarter of overall wage markdown reduction previously reported.

The reported control variables imply that larger firms tend to have higher markups and lower markdowns, while more capital-intensive firms tend to have lower markups and higher markdowns.

6.5. The impact of unionization on wage markdowns

Wage councils and mandated wages can be expected to interact with unionization in complex ways. Wage councils might have channeled wage increases above labor productivity increases, inducing a shift in the distribution of revenue between

[16] Regressions in this spirit are included for instance in Mertens (2020) and Dobbelaere and Kiyota (2018).

Table 5
Impact of wage council mandated wages on product markups and wage markdowns.

	Product markups		Wage markdowns	
	(A)	(B)	(C)	(C)
ln council mandated minimum wage	0.139***	0.129***	−0.098**	−0.109***
	(0.026)	(0.026)	(0.040)	(0.041)
Dummy size (second quartile)	0.107***	0.107***	−0.093***	−0.089***
	(0.008)	(0.008)	(0.014)	(0.014)
Dummy size (third quartile)	0.176***	0.173***	−0.167***	−0.157***
	(0.011)	(0.011)	(0.018)	(0.018)
Dummy size (fourth quartile)	0.211***	0.206***	−0.196***	−0.164***
	(0.015)	(0.015)	(0.024)	(0.025)
ln capital to labor ratio	−0.012***	−0.012***	0.061***	0.059***
	(0.003)	(0.003)	(0.005)	(0.005)
Constant	−1.156***	−1.062***	0.481	0.592*
	(0.213)	(0.214)	(0.336)	(0.342)
Firms fixed effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes
R-squared	0.029	0.031	0.155	0.160
Observations	21,026	19,863	18,04	17,075
Number of firms	3,639	3,498	3,114	2,996

Note: column A and C refers to the whole sample, column B and D exclude the largest firm at ISIC four digits. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

labor and other production factors. In sectors in with already strong unions, workers had extra bargaining power in wage negotiations. We extend the analysis of Sections (6.3) and (6.4). to control for sector unionization level. Identification is also based on assuming that sector unionization is exogenous to any individual firm.

We estimate the following extended version of Eqs. (24) and (26):

$$ln\upsilon_{it} = \alpha + \beta_3 ln(mw)_{jt} + \gamma_3 ln(union)_{jt} + \rho_3 X_{it} + \delta_i + \delta_t + \varepsilon_{it} \tag{27}$$

$$ln\upsilon_{it} = \alpha + \beta_3 ln(mw)_{jt} + \gamma\, ln(union)_{jt} + \rho_3 X_{it} + \delta_i + \delta_t + \eta_{it} \tag{28}$$

where $ln(union)_{jt}$ is the log sector union density measured at a broad sector of activity level. By definition it is a sector effect. Unions are more likely to be active in the largest firms. For instance, Arim and Salas (2007) show that in 2006 in Uruguay the probability of a given private sector worker to be employed in a firm without a union was 0.90 or more for firms with 9 employees or less, 0.77 for firms with 10 to 49 employees and 0.47 for firms with 50 or more employees. Thus, we present results for the whole sample and as a robustness check we exclude the largest firm within each 4-digit ISIC code.

We have more missing information about unionization than about mandated wages so results must be taken with caution. In Table 6 Column (A) reproduces the estimation for all the sample of Table 4 (the difference in difference exercise). In column (B) we run the same regression but for the set of firms for which we have unionization data (the number of observations drops from 25,695 to 19,986). In this restricted sample the interaction between After and Treat (the effect of imposing sectoral bargaining) is of a similar magnitude than in the whole sample. Column (C) includes the unionization control, and an elasticity of −0.058 is obtained.

Then we replicate the same estimations of the minimum mandated wage impact. In column (D) we reproduce the estimations of Table 5 for the whole sample. In column (E) we restrict the sample to firms with unionization data (number of observations drops from 21,019 to 16,216). In the restricted sample the wage markdown to mandated wage elasticity is still statistically significant but of a lesser magnitude, −0.082 instead of −0.149. Column (F) jointly estimates the effect of mandated wages on wage markdowns, controlling for unionization. We find a mandated wage impact on wage markdowns of very similar magnitude and significance to that of the uncontrolled estimation, and an additional impact of unionization. Larger unionization is associated with lower wage markdowns, the estimated elasticity being −0.084.

After 2005, our unionization measure increases about 12%. This implies that unionization had a small direct impact on wage markdowns beyond the impact that had been channeled through wage councils.

Summing up, we have presented evidence that after 2005 price markups increased about 10%, while wage markdowns decreased by more than 40%. Wage councils seem responsible for a sizeable part of this change. The estimated elasticities for wage council mandated wages are statistically significant in all of our modeling alternatives. Given the overall variation in mandated wages, they account for the whole change in markups, and between a quarter to a third of variation in markdowns. Thus, there is still room for complementary explanations of the wage markdowns dynamics that could include globalization effects like increases in exposure to China's imports, or changes in export demand, for instance related to regional changes related to shifts in the relative importance of trade with Argentina and Brazil.

Table 6
Impact of unionization and sectoral bargaining on markdowns.

	(A)	(B)	(C)	(D)	(E)	(F)
After	0.230***	0.204***	0.210***			
	(0.025)	(0.026)	(0.026)			
Treat	−0.675**	−0.687**	−0.790***			
	(0.310)	(0.303)	(0.305)			
After*Treat	−0.142***	−0.135***	−0.122***			
	(0.024)	(0.024)	(0.024)			
ln council mandated min wage				−0.149***	−0.082*	−0.089**
				(0.040)	(0.044)	(0.044)
ln unionization density			−0.058***			−0.084***
			(0.018)			(0.020)
Dummy size (second quartile)	−0.099***	−0.097***	−0.097***	−0.091***	−0.093***	−0.095***
	(0.012)	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)
Dummy size (third quartile)	−0.197***	−0.193***	−0.194***	−0.169***	−0.169***	−0.172***
	(0.016)	(0.018)	(0.018)	(0.017)	(0.020)	(0.020)
Dummy size (fourth quartile)	−0.235***	−0.216***	−0.216***	−0.188***	−0.177***	−0.180***
	(0.021)	(0.023)	(0.023)	(0.023)	(0.026)	(0.026)
ln capital to labor ratio	0.066***	0.072***	0.072***	0.066***	0.070***	0.070***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Constant	0.095	0.013	−0.081	0.846**	0.245	0.068
	(0.271)	(0.257)	(0.259)	(0.334)	(0.371)	(0.373)
Firms fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.179	0.180	0.180	0.138	0.132	0.133
Observations	25,695	19,986	19,986	21,019	16,216	16,216
Number of firms	3,639	2,849	2,849	3,638	2,848	2,848

Note: column A and *D*=whole sample, column *B*= and E only for sectors with unionization data, column C and *F*=only for sectors with unionization data. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

7. Conclusions

This paper used Uruguay's Annual Activity Survey to obtain firm-level product markup estimates along with wage markdown measurements for a sample of firms from manufacturing and service sectors in 2002–2016. Firm data were matched to sector-level wage mandated increases from wage councils summoned by the government since 2005 and sector-level unionization data.

We analyze a period in which sector-level wage negotiation was introduced, there is a *prima facie* case to study changes in wage markdowns. Recent approaches explore the setting in which firms do not take wages as given and face a positively sloped labor supply curve, hence they exercise some degree of monopsony power in the labor market. Older literature on wage negotiation was more focused on market power on the part of workers or unions (in the context of a bargaining process). Both aspects of labor market functioning may coexist. Empirically, we interpret our estimation as a measure of the wedge between marginal revenue productivity of labor and the wage.

In our sample, average firm wages closely follow the increases set in wage councils. This evolution is matched by an increasing labor share of gross output driven by a drift of average wages above labor productivity increases. Since we find stable output elasticities to factors along the period, technological change does not seem a plausible explanation for changes in the labor share. The evidence we find points to changes in relative market power of employers and employees.

Following Mertens (2022) we obtained product markups and wage markdowns estimations separately, and they follow clearly different paths. Product markups increase over the period, though much less dramatically than the sharp raises reported in the literature for some developed economies. At the same time, there is a markedly decreasing trend of average firm-level wage markdowns.

Institutional changes help us to devise a strategy to identify how wages set in centralized negotiation and union density can have effects on firm-level product markups and wage markdowns. First, we compare firms in sectors that before 2005 had sectoral bargaining experience with sectors that did not. The second group of firms were more exposed to the wage council's reinstallation of 2005. In a difference in difference estimation we find that for those without previous sectoral bargaining experience price markups increased more and wage markdowns also decreased more.

We acknowledge that this first estimation strategy might be subject to an endogeneity problem, thus we provide additional evidence less likely affected by this criticism. Since most firms are not represented in wage councils, we argue that minimum sectoral mandated wages are exogenous to them. We make a similar argument for our sectoral union density measure.

We find a significant positive effect of council mandated wages on firm product markups, while they have a negative effect on firm wage markdowns. Our interpretation is that wage councils reduced firm bargaining power in labor markets, but nevertheless firms succeeded in passing to consumers a sizeable part of the increased labor costs.

Union density at the sector level reinforces the effect of mandated wages in depressing wage markdowns at the firm level but the effect in the estimations is quantitatively small. This means that most of unions' effects on wages was channeled through wage councils.

In applied research like ours the question of external validity is always relevant. The worst-case scenario would be that the impact of wage councils reported could be particular to the specific environment described, where a leftist "pro-worker" government is in power. Wage council law gives the government considerable power to impose mandated increases by decree in case unions and employers do not reach an agreement. Then, agreements in wage councils may be thought of as conditional on government goals. In that scenario wage councils can be effective to reduce wage markdowns if the government has that policy objective and acts accordingly in sector bargaining, not necessarily otherwise. Similar reasoning could be applied to the small impact of union density on wage markdowns. In a "pro-worker" government the effect of unionization might be small, but there might be larger differences between more and less unionized sectors in a different political environment.

Finally, our results indicate that reductions in average firm wage markdowns have pushed wages closer to competitive levels. This could indicate less scope for attempting to attain redistributive objectives further through labor market policies.

Credit statement

The authors contributed equally to all steps of the research process.

Data availability

Data will be made available on request.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.ijindorg.2022.102916.

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