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Concentration and mergers: evidence from Italian labor markets

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Concentration and Mergers: Evidence from Italian Labor Markets*

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Abstract

This paper combines INPS workers' records and the *Zephyr* archive to investigate the effects of horizontal mergers on labor market concentration and, in turn, the effect of concentration on wages, job security, and employment. By constructing a flow-based concentration index, I find substantial heterogeneity in concentration levels across different industries. I then employ a TSLS strategy based on the different exposures of industries to horizontal mergers. First-stage results confirm that mergers raise concentration, while the elasticity estimates range between -0.14 and -0.07 percentage points for wages and between -0.77 and -0.68 for hires. Overall, job security is not affected. However, the impact on both wages and job security is limited to women, with the magnitude of the estimates increasing in concentration levels. Overall, the paper provides evidence that horizontal mergers increase concentration, which in turn harms workers and employment.

Keywords: Monopsony; Labor Market Concentration; Mergers; Wages; Hires; Job security.
JEL codes: J31; J42; J71; L13; L41.

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1 Introduction

Over the last four decades, global labor share has declined and the growth of wages for typical workers has been slow, with stagnation observed in some cases. Meanwhile, measures of corporate valuations have risen, and real markups have increased. Before the financial crisis, unemployment had reached record lows, while inflation remained low. Some of these trends are illustrated in Figure B1. These phenomena have sparked interest among researchers, who are exploring non-competitive, country-specific explanations. Recently, several articles have proposed that increasing monopsonistic power can explain these trends (De Loecker, Eeckhout, and Unger 2020; Eggertsson et al. 2019; Farhi and Gourio 2018; Gutierrez and Philippon 2017; Philippon 2020; Mertens and Mottironi 2023; Amodio and Roux 2021).

The term *monopsony* refers to the case in which a small number of buyers dominate a specific upstream market, and to maximize profits, fix input purchases and prices below the level that maximizes social welfare (Manning 2003; Manning 2011). There is ample evidence that monopsony can explain various labor market dynamics, including wages and employment (*e.g.*, I. Marinescu, Ouss, and Pape 2021; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022; Dodini et al. 2023a; Azkarate-Askasua and Zerecero 2023; Gregor Schubert and Taska 2020; Fanfani 2022; Berger et al. 2023), self-employment (Amodio, Medina, and Morlacco 2022), wages inequality (Mertens 2021), firms' behavior (Stansbury and Summers 2020), gender wage gap (Dodini et al. 2023a; Manning 2021; Fanfani 2022), and migration dynamics (Manning 2021).

Deb et al. 2022 analyze establishment data from the US Census Bureau between 1997 and 2016 and found that monopsonistic dynamics can explain 25% of wage stagnation. Similarly, Luccioletti 2022, using granular administrative Spanish microdata, finds that 20-30% of the city-size wage premium and 6-15% of the employment gap between small and large cities can be attributed to differences in labor market power exerted by firms across locations. Lamadon, Mogstad, and Setzler 2022 similarly find that the US labor market is characterized by a high degree of imperfect competition, which causes workers' misallocation, leading to overall welfare reduction.

Monopsony has traditionally been measured by the degree of concentration in product markets. D. Autor, Patterson, and Van Reenen 2023 find that the market shares of the top firms in the US have risen significantly across different industries, and so has product market concentration within[1]. However, monopsonistic dynamics can also arise in the labor market. Cali and Presidente 2023 argue that labour market power is an important source of market distortions in modern economies[2]. They find that higher barriers to entry in product markets translate into higher employers' labor market power in Indonesia. In such a scenario, firms have the power to employ fewer workers and pay lower wages than would be observed in a competitive mar-

[1] They discuss their findings and the policy implications in *Local concentration and structural transformation*, David Autor, Christina Patterson, and John Van Reenen, 12 April 2023, VoxEU.org.

[2] They discuss their findings and the policy implications in *Product market monopolies and labour market monopsonies*, 18 April 2023, VoxEU.org.

ket. There is a body of literature focused on estimating micro-labor supply elasticities of workers and firms (*e.g.,* Bachmann, Demir, and Frings 2022; Langella and Manning 2021; Datta 2021; Sulis 2011; Sokolova and Sorensen 2021; Fanfani 2022; Amodio and Roux 2021; Amodio, Medina, and Morlacco 2022) to assess the presence of monopsonistic dynamics. Positive labor supply elasticities imply that workers' labor supply increases with wages and firm size, which is interpreted as a sign that competitive dynamics are unlikely to develop (Manning 2003; Manning 2011).

More recently, labor market concentration has been identified as a potential factor contributing to the emergence of monopsony (*e.g.*, Luccioletti 2022; Arnold 2021; Azkarate-Askasua and Zerecero 2023; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022; Dodini et al. 2023b), especially when workers have limited bargaining power and there are significant labor market frictions (*e.g.,* Boeri, Garnero, and Luisetto 2023, Amodio, Medina, and Morlacco 2022). Several papers prove theoretically that, when concentration rises, we should expect wages and employment to fall (Manning 2021; Azar, I. Marinescu, and Steinbaum 2019). Although European labor markets have stronger institutions than those in the US, they are not necessarily more competitive (Araki et al. 2022). Only two studies have addressed monopsony in Italy (Sulis 2011; Fanfani 2022), while one has examined labor market concentration (Bassanini, Bovini, et al. 2022), even though not specifically on Italy.

This paper aims to address this gap in the literature by estimating the effects of labor market concentration on daily wages, hirings, and job security, and implementing an novel identification strategy. Traditionally, the literature has addressed the issue of endogeneity induced by local labor market threats through a leave-one-out instrument that relies on a national-level shock in concentration. There is indeed a need to investigate alternative channels that enhance concentration. For instance, Dodini et al. 2023b shows that concentration is driven by the concentration of skills demanded by firms. An additional driver could be the heterogeneity of local labor markets to merger exposure: this is the channel I investigate in this paper.

I calculate a measure of labor market concentration, using the Herfindahl-Hirschman Index (HHI so on), across Italian Local Labor Markets (LLMs also). Only the working spells of entrants' workers from *LoSaI*, a matched employer-employee database drawn from the *INPS* archive, between 2005 and 2018 are selected. Markets are defined as the interaction of regions, industries, and occupations. The definition slightly differs from the standard one in the literature, which usually defines a market as an interaction of a commuting zone and an occupation, for two reasons. First, I want to obtain a more granular definition of an LLM than the standard one. Second, I want to investigate whether concentration differs across industries, as Fanfani 2022 finds that industry heterogeneity drives monopsonistic dynamics. I find that this is also the case.

I use a flow-based measure of concentration rather than a standard stock-based one because it provides a more precise and dynamic picture of how concentration evolves if new hires accurately measure available job opportunities for workers. The majority of the 5,008 Labor Markets identified in my data are not concentrated, as the median value is below the low-concentration threshold. However, a few LLMs are driving the average value upward. In contrast, Martins 2018 found that approximately 9% of Portuguese workers are subject to a level of concentration that raises concerns (*i.e.*, HHI higher than 0.15). In my context, the corresponding share is approximately 3%. Next, I estimate the correlation between labor market concentration, wages, and employment using a worker and market fixed effects specification, similar to *e.g.*, I. Marinescu, Ouss, and Pape 2021, Azkarate-Askasua and Zerecero 2023, Bassanini, Batut, and Caroli 2023, and Bassanini, Bovini, et al. 2022, finding negative but weak correlations.

I exploit the Bureau Van Djik-provided *Zephyr* database, which contains the universe of mergers and acquisitions worldwide, to obtain a measure of industries' exposure to horizontal mergers in Italy. I merge the two data sources by industry and years and I set up a quasi-experimental framework to predict a variation in concentration where markets are treated or not depending on whether they experience horizontal mergers. Mergers seem to target markets randomly, affecting concentration in the following years, as Figure 2 indicates. An industry-level variation is presumably orthogonal to all the threats at the market-level that simultaneously affect wages and concentration, allowing me to identify the true effects of concentration on the outcomes of interest. Figure B7 indicates that concentration is positively correlated with merger exposure across industries over time. The TSLS estimates suggest that the instruments predict an upward shock in concentration at the market-level ranging between 15 and 21 percentage points, which in turn reduces wages by approximately 0.09 to 0.14 percentage points and hires by 0.7 to 0.8 percentage points (p.p. henceforth). In addition, estimates exhibit relevant heterogeneities by sex and concentration levels, as I show in Section 5.6.

I then try to identify the mechanisms. On the extensive margin, employers may reduce the remuneration with labor supply fixed, while on the intensive margin, they can force the labor supply to increase while keeping the overall remuneration fixed. Qiu and Sojourner 2022 find that workers' human capital decreases in firms with greater market power, and Bachmann, Demir, and Frings 2022 that non-routinary jobs are exposed to a higher degree of monopsony than routinary jobs because of workers' on-the-job specific human capital and preferences for non-pecuniary jobs characteristics. Amodio, Medina, and Morlacco 2022 find that higher concentration pushes workers towards self-employment which is used by firms as a strategy to lower wages. I contribute by shedding light on the channels through which concentration reduces wages, disentangling the effect on the extensive margin, represented by the number of worked days, and the intensive margin, represented by the overall remuneration, in Section 5.4.1[3].

Another dimension of workers' welfare that concentration may impact is job security, which I measure as the likelihood of being hired with an open-ended contract (OEC henceforth). Bassanini, Bovini, et al.

[3]Unfortunately, the INPS data does not allow me to explore whether the reduction in wages is due to employers' exercise of power or a worsening of human capital, as I do not observe workers' skills, the tasks they perform on-the-job, or their educational attainment. Furthermore, investigating the effect of concentration on human capital is an exercise for a separate paper.

2022 found no effect of concentration on job security in Italy. In this paper, I examine whether this result holds using a different identification strategy. Overall, I find similar results to previous research, but when estimating separate elasticities by sex and concentration levels, I discover that only women are affected by concentration. In summary, this paper makes two contributions: *i.* It implements a novel empirical strategy to identify the effect of concentration on several labor market outcomes, exploring different transmission channels and heterogeneity; *ii.* It addresses mostly unexplored concerns in Europe and Italy regarding the labor market spillovers of mergers.

To the best of my knowledge, only two prior studies, Arnold 2021 and Guanziroli 2022, used mergers to identify the exogenous variation in concentration and then evaluate the effect on labor market outcomes. However, these studies rely on a difference-in-difference strategy, while my approach uses an instrumental variable regression. Furthermore, Arnold 2021 uses a slightly different definition of labor market concentration, and Guanziroli 2022 focuses on a specific event, a large Retail Pharmacy Merger in 2012 in Brazil, and both studies do not fully explore all the transmission channels and outcomes that could be affected by concentration.

Lastly, economists from various fields have increasingly turned their attention to antitrust issues. A growing body of academic literature among US antitrust scholars calls for more stringent antitrust enforcement, especially concerning horizontal mergers (Berger et al. 2023; E. A. Posner and I. E. Marinescu 2020; Jarosch, Nimczik, and Sorkin 2019; Shapiro 2019; Suresh, E. Posner, and Wey 2018). These scholars argue that product markets in the US have become more concentrated in the past decades, due in part to weak legislation on mergers enforced by antitrust agencies. More recently, labor markets have also become a subject of scrutiny. Cali and Presidente 2023 argue that extending antitrust measures from product to labour markets is needed in modern economies, while Berger et al. 2023 come to the same conclusion while providing, in addition, a data-driven guideline to evaluate mergers. This debate is also gaining attention in Europe, but empirical evidence is still scarce. My work contributes to this policy-driven literature by identifying the effects of mergers on labor market concentration and, in turn, their spillovers on various labor market outcomes, both at the worker and the market-level. As far as I know, my study is one of the first to address these concerns in Italy, adding to the ongoing debate on whether more stringent antitrust enforcement is necessary.

The rest of the paper is structured as follows: Section 2 describes the institutional background; Section 3 introduces the theoretical context; while Section 4 describes the data and the evidence on labor market concentration. In Section 5 I present the empirical equations; Sections 5.3 and 5.4 present the identification strategy and the exogenous estimates, respectively; and in Section 5.6 I explore the underlying mechanisms and the heterogeneities. I conclude with Section 6.

2 Institutional Background

This section provides a brief overview of the Italian labor market, highlighting the collective bargaining system, minimum wages, and the deregulation process that occurred during the 1990s. These factors are important for contextualizing and motivating my analysis.

Collective bargaining in Italy is structured across two levels. The first level, known as *Contratti Collettivi Nazionali di Lavoro* or *CCNL*, establishes minimum wage schedules and working conditions at the industry and local levels. The second level, which takes place at the firm or local level, negotiates additional wage components and other details. The *CCNL* involves unions and employer associations, while firm-level bargaining is conducted by employee representative bodies. However, the complexity of the situation has been exacerbated by a decentralization process whereby larger firms with bargaining power can opt out of industry-wide collective agreements and establish their favorable terms. Consequently, the labor market in Italy is fragmented, making it difficult to map all the different contracts[4].

According to Garnero 2018, there are approximately 860 industry-wide collective agreements covering practically all private-sector employees in Italy. Trade union density, which is defined as the number of members over the total number of employees, is below 30% in the private sector, and employers' organizations' density is slightly lower than 50%. The author also finds that the minimum wages, established through *CCNL* and industry-specific, are relatively high compared to industry-specific medians and that when computed concerning regional median wages, they are higher in southern regions compared to northern ones. This suggests that firms would likely opt out of collective agreements to reduce labor costs, especially in regions where real minimum wages are significantly high, such as in southern Italy.

Furthermore, several labor market reforms have been promulgated in the last few decades, reducing Employment Protection Legislation (EPL) on open-ended contracts to promote employment and firms' resilience. Figure B2 illustrates this pattern of de-regularization, which began with law No.108 approved in 1990, continued with the *Biagi* Reform in 2003, was followed by the *Fornero* Reform in 2012, and ended with the *Jobs Act* in 2015, which abolished the *Article 18*. This article of the *Statuto dei Lavoratori*[5] essentially prohibited firms from dismissing workers covered by an open-ended contract for economic reasons. The *Jobs Act* introduced the change for firms to dismiss workers for economic reasons in the Italian labor market.

To summarize, the reforms implemented increased the likelihood of economic dismissals and a decrease in costs, both in monetary terms and in terms of the probability of reinstatement. These changes applied only to larger firms. Until 2018, the overall cost of uncertainty, the possibility of firing, and monetary compensation were low for open-ended contracts and even lower for fixed-term contracts, with decreasing union coverage

[4]For an extended discussion on the Italian bargaining system and its effect on labor market outcomes read Fanfani, Lucifora, and Vigani 2021; Devicienti and Fanfani 2021; Fanfani 2020.

[5]Also known as *Legge 300*, was introduced in 1970 as it represented the main pillar defining workers' rights in the Italian labor market.

and differential impacts of minimum wages across LLMs.

Last, one of the potential sources of monopsonistic power is the presence of legal provisions limiting workers' mobility, such as non-compete agreements[6]. This source is particularly relevant in the US (Sarfati 2020; OECD 2020). However, Boeri, Garnero, and Luisetto 2023 find that in Italy about 16% of private sector employees are currently bound by a non-compete agreement, which corresponds to approximately 2 million employees. They are more common among highly educated and higher-earning employees, but they are also relatively frequent among employees in manual and elementary occupations and low-educated and lower-earning ones. The authors also find that the probability of being bound by a non-compete clause is negatively correlated with labor market concentration. They interpret this as a sign that these agreements matter less in more concentrated local labour markets because there are already fewer competitors. Overall, in such a scenario, firms, especially the largest ones, could exert their market power over workers.

3 Conceptual Framework

The underlying hypothesis of my paper is that labor market concentration, as measured with the HH index, is a proxy of monopsonistic dynamics. The key intuition for monopsony power is analogous to that of monopoly power: profit-maximizing employers with monopsony power keep both wages and employment below the competitive equilibrium. Manning 2011 and Azar, I. Marinescu, and Steinbaum 2019 predict that both employment and wages should fall when labor market concentration rises. While recent literature has also explored this relationship, in this section, I will further examine the link between labor market concentration and monopsony power. Although this relationship may seem obvious, it is not always the case.

Therefore, it is essential to explain why an index of labor market concentration can efficiently proxy the degree of power of employers across labor markets, as I define them. Amodio, Medina, and Morlacco 2022 derive an oligopsony model, estimated on Peruvian firm and worker-level survey data, in which, when concentration increases, earnings from wage work decrease. They additionally demonstrate that, across local labor markets, the average markdown is an exact function of the HHI. Arnold 2021 argues that there are no reasons to unambiguously believe that monopsonistic dynamics are proxied by labor market concentration. Other drivers, such as declining unionization rates and increases in non-competes and no-poaching agreements (Boeri, Garnero, and Luisetto 2023), could lead to rising monopsony power, even in the presence of falling local concentration.

Given this, there is no reason to unequivocally believe that estimating a wage elasticity concerning labor concentration is appropriate to capture the effect of increasing labor power on workers. However, Arnold

[6] Boeri, Garnero, and Luisetto 2023 define them in short as a contract in which an employee agrees to not compete with her employer after the employment period is ended.

2021 highlights the importance of the source of variation of concentration. According to the model, mergers can be the source of such variation, and the results demonstrate this beyond a reasonable doubt. This is because mergers do not affect monopsony power through channels other than concentration. Therefore, in this context, it can be inferred that there are no monopsony effects when there are zero changes in local labor market concentration. Therefore, a merger that increases concentration raises firms' power, which in turn results in a wage loss. Arnold 2021, assuming that firms compete à *la Cournot*, obtains the following equation[7]:

$$w_m = \underbrace{\left(\frac{\eta_m}{HHI + \eta_m}\right)}_{\text{worker fraction } = \gamma_m} \underbrace{\theta_m}_{\text{AMRPL}} \tag{1}$$

, in which w_m is the average market wage, η_m is the elasticity of labor supply in market m, $\text{HHI} = \sum_j (s_j)^2$ is the HHI based on employment shares, θ_m is the average value of the marginal product of labor, and γ_m is the fraction of the average marginal revenue product that goes to wages. This equation implies that all else equal, the higher concentration, the lower the wage[8]. The empirical challenge is to isolate an exogenous variation in HHI to identify the effect of employers' power on wages. In Section 5.3.2, I will elaborate on how I use horizontal mergers to achieve this.

4 Data, Concentration Index and Evidence

4.1 *LoSaI*

LoSaI is a longitudinal sample of workers extracted from the *INPS* universe based on their date of birth. For these workers, it provides information on all their working spells, including the gross overall remuneration, the days and weeks worked, the occupation in 5 discrete brackets, the type of contract (OEC, FTC, and seasonal), the time schedule (part time or full time), and the region of residence, from 1985 to 2018. For each spell, it provides a unique firm code that can be matched with a different firm-level dataset to obtain a matched employer-employee dataset. This dataset provides several firms' information, mainly the industry (two-digit NACE Rev.2), and the size class, which varies over years and is classified in 14 brackets from 1-5

[7]Equation 1 is taken from Arnold 2021, Section 2.3, page 7.

[8]An analogous prediction is drawn from the model of Luccioletti 2022, in which, as HHI and wages are directly related in the FOC, there is perfect mobility, and the elasticity of housing is perfectly elastic, labor market power affects wages only through variation in HHI_{mt}. He derives the following FOC:

$$\text{W}_{mt} = \left(1 + \eta^{-1}\text{HHI}_{mt}\right)^{-1} \text{AMRPL}_{mt} \ (2)$$

, where η is workers' labor supply elasticity and $AMRPL_{mt}$ is the average marginal revenue product of labor.

to over 500 employees[9]. I select only new hires between 2005 and 2018, as I want to calculate a flow-based concentration index.

Moreover, both theoretical predictions and empirical evidence indicate that employers' power compresses entrants' wages rather than long-term incumbents, who are more experienced and protected by higher EPL (Bassanini, Batut, and Caroli 2023). New hires are defined as the spells activated for each individual in a given year in which the firm does not match the one for which the same individual worked in the previous year (Bassanini, Bovini, et al. 2022). I also exclude transformations, keeping only newly activated contracts. For each worker, I delete repeated observations within the same year keeping the longest spell (Macis and Schivardi 2016), obtaining thus an unbalanced worker-level panel dataset.

I compute the main dependent variable, daily wages, by dividing the total gross remuneration of each employment contract by the number of days actually worked as provided by *LoSaI*, thus ruling out the presence of any measurement errors. The number of records with a value of 0 for wages is less than 50,000 and, as they likely represent a measurement error, they are dropped. The final sample is made up of approximately 3,600,00 newly activated employment contracts and 1,400,000 entrant workers[10].

4.2 Herfindahl-Hirschman Index

A labor market is defined as an interaction between an industry s, an occupation o and a region r. Industries are classified as two-digit NACE Rev.2 industries, occupations are 5 discrete brackets (employees, managers, middle managers, apprentices, and standard workers), while regions, the 20 Italians, are those of residence of workers. The measure of labor market concentration is the standard one in the literature, the Herfindahl-Hirschman Index, whose formula is:

$$\mathrm{HHI}_{m,t} = \sum_{i=1}^{N_m} s_{imt}^2 \tag{3}$$

, where N_m is the total number of firms within the market m and s_{im} is the labor market share of the firm i in market m at time t, defined as the number of hires of the firms in that market in t divided by total hires of all firms belonging to the same market in t. However, *LoSaI* follows workers' careers, and hence firms' population is presumably not representative. Hence I cannot calculate firms' shares and the HHI as in Equation 1. However, firms' distribution within and across class sizes is similar to the Italian one, as indicated in Table A2 in the Appendix. I, therefore, calculate concentration by modifying the previous formula:

[9] I provide further details in Appendix 6.

[10] I provide more evidence in Table A1 in the Appendix.

$$\text{HHI}_{m,t} = \sum_{N_{dm}} s_{d,t}^2 \tag{4}$$

, where N_{dm} represents the number of class sizes in each market m and s is the ratio of the number of new hires for the representative firm in class d in m in t to the total number of hires in m and t. The representative firm's hires for each size class are computed by dividing the number of hires for each year within that size class by the number of firms hiring in the same year within that size class. The idea underlying the construction of this index is that firms within the same class size pay similar wages and that market concentration depends on the heterogeneity of hires across firms' sizes within it. The fact that larger firms or plants pay higher wages, and vice-versa, is widely documented in the US, in Europe (Ramaswamy and Rowthorn 1991; Brown and Medoff 1989; Idson and Oi 1999, Abowd, Kramarz, and Margolis 1999), and in Italy as well (Bertola and P. Garibaldi 2001; Mion and Naticchioni 2009).

4.3 Descriptive Evidence

Concentration. I compute the HHI for approximately 6,000 LLMs. However, several markets have only one spell, which inevitably induces an upward bias in the HHI[11]. To address it, I deleted all those market-year tuples with one spell only. I finally obtained an almost perfectly-balanced panel of 47,727 market-year tuples regarding 5,008 markets in Italy between 2005 and 2018. I describe market concentration in Table 1 and Figure 1, while across industries and regions in Figures B3 and B5 in the Appendix. On average, concentration across Italian labor markets is mild: the median value is far lower than the standard threshold, indicating a medium level, and only a few markets are concentrated. However, the average value is approximately 0.14, indicating a medium level of concentration. Figure 1 indicates that the distribution is right-skewed: most of the markets are not concentrated, while a few are.

As a robustness exercise, I compute the market HHI distribution on the worker-level panel. In this case, I substantially weight each market's HHI by its numerosity, thus getting rid of the potential bias induced by those markets with fewer spells and higher HHI. Results are displayed in Figure B4 and indicate that, when each market is weighted by the number of spells, the levels of concentration sharply decrease and so the spikes. This confirms previous interpretations: spikes are not a concern, and on average concentration levels across Italian LLMs are low.

[11]With one spell only the index, for a mechanical bias induced by the HHI formula in Equation 3, is equal to 1.

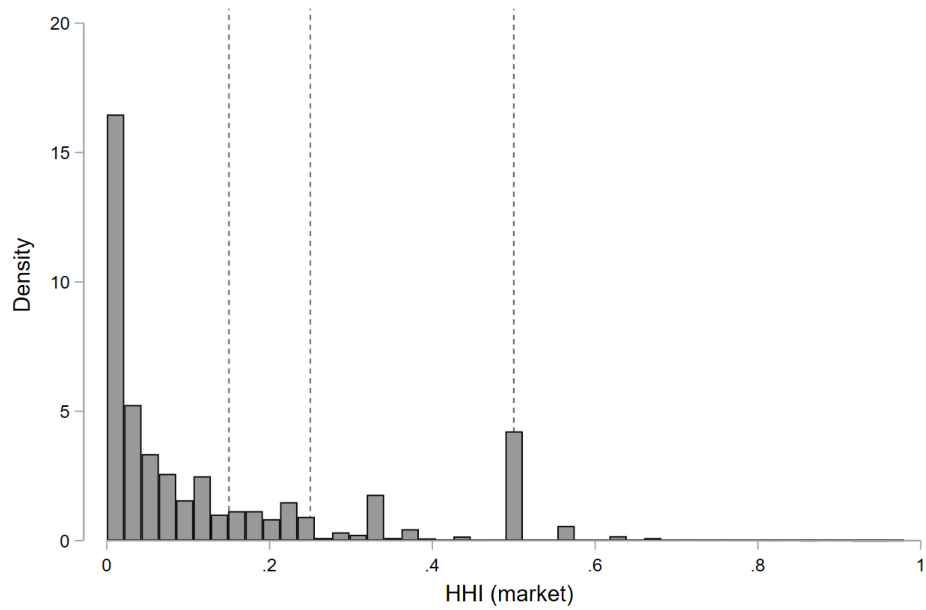


Figure 1: **HHI distribution across markets and years**.

Notes: Observations are 47,727 market-year tuples associated with 5,008 labor markets in Italy. A market is defined as an interaction of a region, an occupation, and an industry. Industries are 76 two-digit NACE Rev.2 cells, regions are the 20 Italians, and occupations are classified in 5 brackets. The dotted lines represent the standard thresholds for defining, respectively, low, medium, high, and high levels of concentration. Market HHI's are calculated as the squared sum of class size - 14 discrete brackets - shares, where the share is calculated as the ratio between hires by market-year tuples of the representative firm in each size class and the total number of hires in that market, following the formula in Equation 1.

Martins 2018 studies labor market concentration in Portugal finding that approximately 9% of Portuguese workers are subjected to a level of concentration that, according to the US antitrust agency, can be classified as medium. Moreover, as he relies on a stock-based index, he likely underestimates the true level of concentration across LLMs. In the Italian case, according to my estimates, the percentage is definitely lower, as the median value in the market distribution is 0.05 points. I find that approximately 95,000 spells over more than 3,500,000 entrants' spells happen in markets with an HHI higher than 0.15. They represent approximately 3%, so definitely less than 9% in Portugal. This share is lower than expected, according to various concerns, and thus should not worry competition authorities and policy makers.

When computing the measure across regions, industries, and occupations separately, concentration increases, as displayed in Table 1 and Figure B3. Although the distributions tend to shift toward normality it emerges that concentration largely differs across industries. I believe that my findings are coherent with those of Fanfani 2022, who finds that industry heterogeneity in monopsonistic dynamics in the labor market explains a relevant portion of the gender wage gap. This suggests that monopsonistic dynamics are driven by local and industry factors. I take advantage of this fact to set up my identification strategy.

Index	Observations	Mean	St. Dev	Min	$1^{st}Perc.$	Median	$99^{th}Perc.$	Max
HHI_m	47,727	0.136	0.174	0.000	0.000	0.054	0.625	0.979
HHI_s	1,064	0.155	0.087	0.006	0.027	0.141	0.424	0.642
HHI_r	280	0.148	0.040	0.088	0.094	0.138	0.269	0.291
HHI_o	84	0.211	0.094	0.091	0.091	0.206	0.363	0.363

Source: Author's calculation on *LoSaI*.

Table 1: **Summary statistics of concentration indexes across markets (m), industries (s), regions (r) and occupations (o).**

Notes: Labor markets are 5,008. The indexes are calculated according to formula (2) relying on entrants' spells, *i.e.,* those newly activated for each individual who was not working in the same firm the previous year. The indexes are calculated as averages of markets HHI's within respectively each occupation, industry, and region. Markets-year tuples are 47,727, industry-year tuples are 1,064, region-year tuples are 280 and occupation-year tuples are 84. The time span goes from 2005 to 2018.

Financial Turmoils. Another concern is that concentration could vary over time, peaking during periods of recession, exacerbating the damage that financial shocks can inflict on workers. In fact, there is evidence of this phenomenon, as financial turmoil can amplify labor market volatility (Boeri, Garibaldi, and Moen 2013; D. H. Autor, Dorn, and Hanson 2016) and shrink firms' access to credit markets, which in turn reduces hirings (Berton et al. 2018). However, my results point in a different direction. As proved by Figures B5 and B6 in Appendix 6, concentration does not change over time and even during the peak of the financial crisis[12] it does not differ from the whole period. Therefore, it does not appear that labor market concentration is an additional channel through which financial turmoil affects the labor market that, in turn, damages employment and workers.

Summary. *i.* Concentration in Italy is weak, but it is heterogeneously distributed; *ii.* Most of the markets have low value while a few are highly concentrated, driving the average value upward; *iii.* Concentration increases when computed across industries, which show relevant heterogeneity; *iv.* There is no evidence that financial turmoil affects concentration. My index of concentration suffers of several limitations, mainly due to data issues, that I discuss extensively in Section 6 in the Appendix. However, I believe that it still paints a reliable picture of the levels of concentration and how they have changed across Italian LLMs over time.

[12]Approximately, the financial crisis displays its effects in Italy between 2009 and 2014, peaking in 2011 and 2012.

5 Empirical Strategy

5.1 Wages Specification

To identify the correlation between concentration and entrants' wages, I estimate the following Equation:

$$log(Y_{i,m,t}) = \delta_i + \mu_m + \gamma_s + \Gamma_{r,t} + \Lambda_{d,t} + \Phi_{o,t} + \beta_t + \theta log(HHI_{m,t}) + \Gamma Z_{i,t} + v_{i,m,dj,t} \quad (5)$$

, where i indexes workers, r regions, o occupations, j firms, d class sizes, s industries, and t years. Y is the gross daily remuneration for each yearly spell of worker i in region r, with occupation o, in firm j of class size d and industry s in year t. The matrix Z contains worker-level covariates, as a quadratic polynomial for age and spells length to proxy individuals' working experience and on-the-job working experience. $v_{i,m,t}$ is an error term clustered at the market-year-level (Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022). I relax this assumption in a robustness exercise, which I describe in Section 5.4.1.

The model is specified in a log-log form and hence θ should be interpreted as the elasticity of entrants' daily wages with respect to labor market concentration. Equation 5 is estimated with Ordinary Least Squares (OLS so on). I exploit, hence, both cross-sectional and over-time variation in concentration to address its effect on workers' wages, controlling for a full set of time-varying covariates at a worker and market-level as well as for market and worker fixed effects. I instead control for occupation-year, region-year, and size-year fixed effects to take into account potential time-varying confounding effects jointly influencing concentration and wages.

Workers Mobility. The main assumption of my empirical strategy is that individuals change employers and markets within the time window. I need to test if it holds to rule out the confounding effects jointly influencing wages and concentration at the market-level. In my sample, on average, individuals appear 3-4 times, while more than 50% change at least once in the period of analysis market and the same share approximately also employers. As the industry depends on the firm where the worker is employed, it means that also the industry changes at least once for more than 50% of the population. Approximately also half of the workers switch firms' size class.

This evidence suggests that a considerable amount of wage dispersion in my sample can be explained by controlling for time-varying firms' size. Individuals do not change their occupations frequently, 20% between 2005 and 2018. The region of residence does not change for each worker. For this reason, in the regressions, I control for time-varying fixed effects for occupation and regions. All in all, worker and market fixed effects plus all the sets of controls, both for workers and markets, capture a relevant amount of wage variation. Many workers appear only once and are dropped when including individual fixed effect. The final sample on which equations are estimated is made up of approximately 3,000,000 newly spells and 900,000 workers.

Dependent variable: ln(Daily Wages)				
	(1)	(2)	(3)	(4)
ln(HHI)	.00209**	-.00152**	.00115	-.0014*
	(.00075)	(.00068)	(.0011)	(.00081)
Observations	2,928,818	2,928,818	2,928,474	2,928,474
spell length & age (squared)	√	√	√	√
part-time dummies	√	√	√	√
worker FE	√	√	√	√
year FE	√	√	√	√
industry FE	-	√	√	√
region FE	-	√	√	-
occupation FE	-	√	√	-
size FE	-	√	√	-
reg-ind-occ FE	-	-	√	√
occupation-year FE	-	-	-	√
size-year FE	-	-	-	√
region-year FE	-	-	-	√

SE clustered at the market-year-level.
Daily wages are the ratio of overall remuneration and the number of worked days
*** p<0.01, ** p<0.05, * p<0.1

Table 2: **Equation 5 estimates**.

Notes: Observations are 3,573,677 yearly spells between 2005 and 2018. Clusters are 47,727. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

Results. Estimates in Table 2 indicate that the relationship between concentration and wages exists, but is weak. The sign switches when adding market fixed effects, suggesting indeed that time-invariant unobserved heterogeneity at the market-level explains a considerable amount of variation in both wages and concentration. In the latest specification, the elasticity becomes negative, even though slightly significant. Furthermore, there might be a heterogeneous effect of concentration for contracts protected by different levels of EPL. Differences over time might also be due to variations in the composition of the workforce in terms of sex, nationality, and age[13].

Estimates suffer from endogeneity for several reasons. First, there is a reverse causality mechanism between concentration and wages as where wages are higher concentration is likely lower, second, concentration and wages jointly realize and, third, there is an omitted variable bias issue triggered by market tightness, industry, and firm-specific shocks that I do not control for. Because of the sign of these correlations, which is always positive, endogenous estimates are shrunk towards zero. I discuss all the determinants of endogeneity

[13] There is indeed empirical evidence that monopsonistic power is more harmful for women, parents, and immigrants (Fanfani 2022; Sulis 2011; Detilleux and Deschacht 2021; Qiu and Sojourner 2022).

in Section 6 in the Appendix[14].

5.2 Employment Specification

Both theory and evidence (Manning 2003; Azar, I. Marinescu, and Steinbaum 2019; Luccioletti 2022; I. Marinescu, Ouss, and Pape 2021, Arnold 2021, Berger et al. 2023) indicate that monopsonistic competition entails, beyond lower remuneration, also lower use of the labor input, namely employment. The effect might go through two channels: on the extensive margin, a highly concentrated market prevents firms from entering the competition and reducing employment, while on the intensive margin, firms holding power have the incentives to reduce labor input to maximize profits. To test this, I measure employment as the number of labor contracts signed in a market during a year (I. Marinescu, Ouss, and Pape 2021), which I denote as $F_{m,t}$, and estimate the following equation:

$$log(F_{m,t}) = \delta_m + \Phi_s + \gamma_{o,t} + \Theta_{r,t} + \beta_t + \theta log(HH_{m,t}) + \phi X_{m,t} + v_{m,t} \tag{6}$$

, where m indexes markets, δ and β represent market and year fixed effects and γ, Φ and Θ are occupation-year, industry and region-year fixed effects. $v_{m,t}$ is a standard error term clustered at the market-level to allow records belonging to the same market to be correlated across time as the shocks can be time-persistent within each market. As the model is specified in a log-log form, should be interpreted as the elasticity of market employment with respect to labor market concentration. X contains the average age and share of men for each market m in year t (I. Marinescu, Ouss, and Pape 2021), while I rely on a full set of occupation-year, industry and region-year fixed effects to control for value-added and employment levels specific to each market and year.

Results. Table 3 shows that there is a negative and significant correlation between market concentration and employment flows: when (and where) concentration increases, hires diminish. The coefficients are similar in magnitude across all different specifications and are precisely estimated as the standard errors are very close and small. Estimates again suffer from endogeneity, arising from different mechanisms with respect to those of the wages specification. Due to the HHI formula, markets with higher spells tend mechanically to have lower levels of concentration, whereas the opposite holds for markets with fewer spells. This mechanism induces a negative relationship between the two variables which biases towards zero the estimates. To address all these threats, I need to identify a shock affecting concentration but not the outcomes. This variation should rule out the joint effect of any labor demand and offer shocks at the market-level influencing concentration and the outcomes contemporaneously. Furthermore, it should also be orthogonal with respect to the mechanism

[14] For an extended discussion of all feasible threats, and of the sign and magnitude of the bias, read I. Marinescu, Ouss, and Pape 2021 and Azkarate-Askasua and Zerecero 2023.

inducing a positive correlation between concentration and wages across markets.

	(1)	(2)	(3)
Dependent variable:	ln(Hires)	ln(Hires)	ln(Hires)
ln(HHI)	-.1166***	-.1167***	-.0948***
	(.00445)	(.00446)	(.00332)
Observations	47,180	47,180	47,180
(mean) sex & age	$\surd$	$\surd$	$\surd$
reg-ind-occ FE	$\surd$	$\surd$	$\surd$
year FE	$\surd$	$\surd$	$\surd$
occupation FE	-	$\surd$	-
region FE	-	$\surd$	-
industry FE	-	$\surd$	$\surd$
region-year FE	-	-	$\surd$
occupation-year FE	-	-	$\surd$

SE clustered at the market-level
*** p<0.01, ** p<0.05, * p<0.1

Table 3: **Equation 6 estimates**.

Notes: Employment is measured as the number of newly activated working spells within each market and year. Specification and controls are displayed in Equation 5. Clusters are 5,008. The sample is made of 47,727 market-year tuples, associated to 5,008 markets.

5.3 Mergers and Concentration

To obtain this exogenous variation, I rely on an instrumental variable strategy exploiting horizontal mergers. A wide body of literature has focused on mergers, but mostly in different fields of economics with respect to labor. Mergers have been widely discussed in the antitrust literature, while, regarding monopsony, there are several works finding that they could raise product market concentration[15]. However, growing evidence and concerns among competition authorities and policy makers in the US and Europe suggest that mergers also have consequences in the labor market (E. A. Posner and I. E. Marinescu 2020; I. Marinescu and Hovenkamp 2019; Manning 2003; Manning 2021; Suresh, E. Posner, and Wey 2018). Mertens and Mottironi 2023 found a negative correlation between the markups of firms and their size within the same industry. To explain this evidence, the authors suggest that firms maintain low markups and increase their market shares in order to establish a dominant labor market position, which allows shrinking wages. This implies that even in the absence of standard product market spillovers, a horizontal merger could have adverse effects on workers.

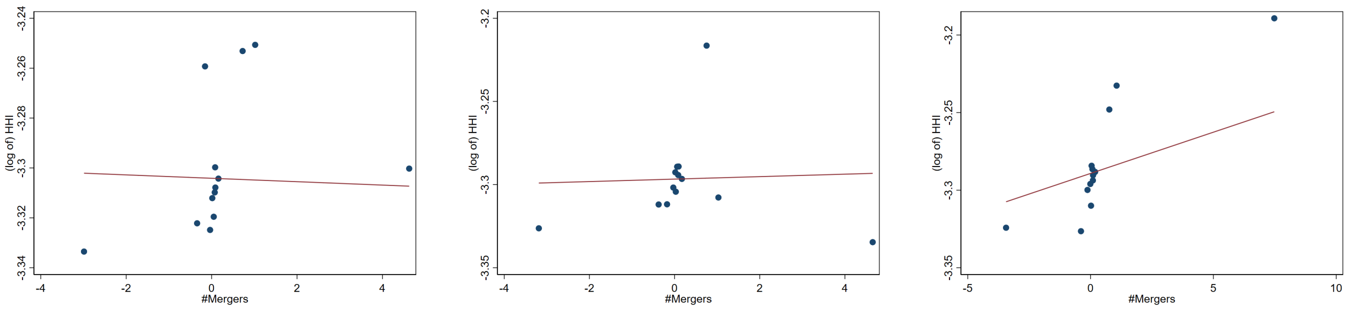
[15] For further details and discussions on this topic read *e.g.*, Saidi and Streitz 2021, Affeldt et al. 2021, Götz and Gugler 2006, Cosnita-Langlais 2008, and Benkard, Yurukoglu, and Zhang 2021.

Berger et al. 2023 find that mergers decrease employment and wages with higher effects in concentrated markets. Given Empirical evidence supports this claim, as there are studies (Shapiro 2019; Dodini et al. 2023a; Arnold 2021; Guanziroli 2022) that show that horizontal mergers increase labor market concentration in both reduced-form and in more structural estimates (I. Marinescu, Ouss, and Pape 2021; Jarosch, Nimczik, and Sorkin 2019). In Section 6 of the Appendix, I provide an extensive discussion of the literature. I exploit this mechanism in my identification strategy.

5.3.1 Data on Mergers

I exploit the *Zephyr* database provided by the *Bureau Van Dijk* archive, which contains times series of worldwide rumoured, announced, and completed mergers and acquisitions operations of all types (partial or full acquisitions, mergers, etc.) from 1997 to nowadays. I select all completed mergers and acquisitions operations whose target country is Italy from 2005 to 2018. For a sub-sample of these events, I also have information on the number of workers involved, as well as the vendor and acquiror sizes. The final sample contains 5,932 events, associated with 4,237 different acquiror firms and approximately the same number of vendors[16]. On average, approximately 423 events happen per year. I provide further evidence in Appendix 6. I select only *horizontal mergers*, so those operations between firms that operate in the same industry. The final number of events in the entire analysis period decreases to 184. *Zephyr* provides a lot of information for each record, such as all the industries (up to 6 digits) associated, the number of employees involved (with many missing unfortunately), the location involved through the plants, and the firms' names. Ideally, one can match these firms to other data sources through their names, which can be used to recover the fiscal code with virtually no errors. However, the same information is absent in *LoSaI*, which instead provides only a 2-digit NACE code and no fiscal code. Therefore, I can only match this data with my workers' dataset through the industry associated with each firm and the year, exploiting a national-industry shock in horizontal mergers and not a firm-specific one. As I define a market as a region-industry-occupation tuple, mergers between firms across different industries do not raise market concentration and thus are not relevant to the identification strategy.

[16]In France and Germany, for example, approximately the same number of domestic operations occurred between 2014 and 2018 (Source: *Oxford Economics*). Hence, Italian labor market exposure to this phenomenon is relatively weak with respect to other countries.



(a) Current mergers.

(b) 1 year lagged mergers.

(c) 2 years lagged mergers.

Figure 2: **Binscatters between (log of) market concentration and the number of industries' horizontal mergers over time.**

Notes: The red line in each panel represents a linear fit on the data. In each panel, market fixed effects are included. The total number of horizontal mergers between 2005 and 2018 is 184. The sample is made up of 48,219 market-year tuples and 4,874 markets. Mergers are defined as horizontal mergers at the industry-level between 2005 and 2018.

5.3.2 Identification Strategy

The underlying idea of the identification strategy is that markets that experience mergers become more concentrated over time. Concentration can vary depending on shocks coming through different channels[17]. An industry-level shock (*e.g.*, two large firms merging in an industry at a national level) could raise concentration at the market-level, as the shock would translate to different extents to all those LLMs associated with that industry. Furthermore, only workers belonging to the two merged firms would be directly hit, while all others would not.

The channel I exploit is a *national-industry-level shock* in concentration. There is evidence that industry heterogeneity is a driver of monopsonistic dynamics in Italy (Fanfani 2022). There is also evidence that industries' heterogeneous exposure to horizontal mergers explains different increases in concentration and in turn the heterogeneous effects on wages (Jarosch, Nimczik, and Sorkin 2019; I. Marinescu, Ouss, and Pape 2021). Figure B7 in the Appendix indicates that the most hit industries are *Financial Activities, Information and IT Services Activities, Editorial Activities, Electric and Gas Furniture, Manufacture of Machinery and Equipment*, and *Satellite Telecommunication*. I hence exploit the fact that the more an industry experiences mergers over time, the more it will become concentrated with respect to other industries that do not experience mergers but also with respect to itself. Figure 2 confirms this relationship for lagged measures of horizontal mergers, with the two-year lagged ones being the most relevant. Panel (a) of Figure 2 indicates instead that current mergers are not correlated with respect to concentration, suggesting that the

[17]For instance, it may be the case that some occupations become more concentrated over time. Matsudaira 2014 and Azar, I. Marinescu, Steinbaum, and Taska 2020 study concentration across occupations, based on vacancies posted on *Careerbuilder.com*, finding that some occupations, especially those in the low-wage segment of the labor market, have become more concentrated over time.

"treatment" can be considered as good as random across labor markets. Summing up, horizontal mergers seem to target industries randomly, while the positive correlation with labor market concentration shows up only in the following periods.

To the extent to which I am able to control for any source of variation in concentration, mergers would hence represent a shock in concentration targeting only a sub-sample of markets, and consequentially, workers belong to these markets, allowing me to set up a quasi-experimental framework. The intuition behind the identification strategy is illustrated in Figure 3. Relying on mergers in the same market, I would not rule out the direct effect of mergers on wages and hires and the reverse causality triggered by the positive correlation between productivity and mergers within markets. Instead, an industry-national level shock affects concentration but has no direct effect on the outcomes of interest as income and employment dynamics depend on the region and on the occupation of each market. To illustrate this, consider, for instance, a merger between two competitors at the national level, whose plants are located in one region of Italy. It reasonably does not directly influence the outcomes of the workers employed by other competitors whose plants are mostly located in different regions. The same applies to hires.

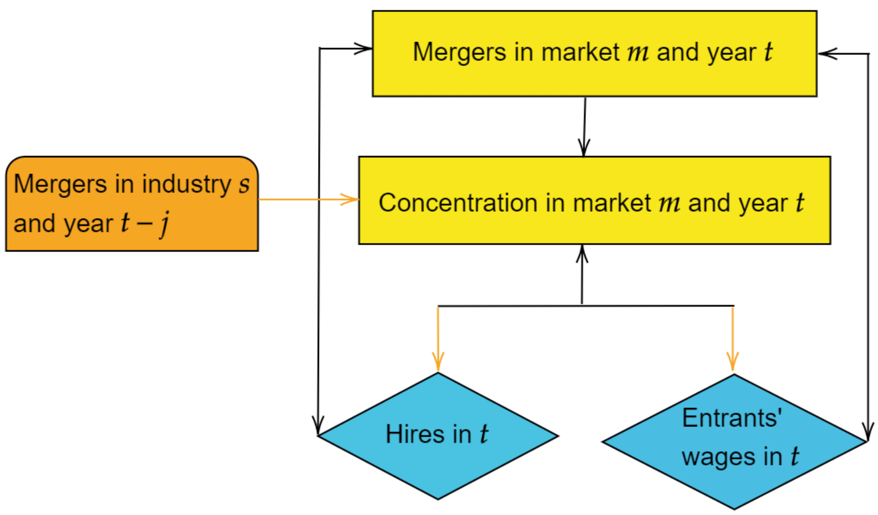


Figure 3: **Sketch of the Identification Strategy**.

Notes: The thick arrows indicate a relationship of any kind between two variables, in the boxes. The yellow boxes indicate the endogenous variables, the orange boxes those exogenous, while the blue color indicates an outcome of interest. The black arrows represent correlations, that by definition are symmetric, while those orange indicate a unidirectional relationship, that I believe based on the assumptions to be a causal effect. What my strategy aim to estimate are the orange arrows. *Source:* Author's realization.

Furthermore, I rely on lagged measures of mergers to (i) Ensure exogeneity with respect to LLMs dynamics simultaneously influencing the realization of the mergers and the outcome; (ii) Take into account that merged firms need some time to consolidate and display their increased labor market power. Figure 2 seems to suggest that this is the case as only lagged mergers are positively correlated to concentration, with the two-year ones being the most relevant.

Instruments. I define two binary instruments as:

$$\text{IV}_t^1\text{: } \forall \text{ t in [2005,2018], I}_t\{Mergers_{s,t-1} > 0\} = 1 \Rightarrow \text{HHI}_{m,t}; \tag{7}$$

$$\text{IV}_t^2\text{: } \forall \text{ t in [2005,2018], I}_t\{Mergers_{s,t-2} > 0\} = 1 \Rightarrow \text{HHI}_{m,t} \tag{8}$$

, where t-1 and t-2 indicate 1 and 2 lagged years and I is a dummy variable taking value 1 if an industry s experiences at least one merger in t-1 and t-2, 0 otherwise. In other words, I instrument ($\Rightarrow$ in my notation) concentration in each labor market and year (*i.e.*, $HHI_{m,t}$) with a dummy variable indicating whether the industry s identifying those markets has experienced at least a merger in the previous one or two years. I define a Wald Instrument[18] as:

$$\beta_{IV} = \frac{\operatorname{cov}\left(Y_{it}, IV_t^k\right)}{\operatorname{cov}\left(HHI_{i,m}, IV_t^k\right)} = \frac{E\left[Y_{it} \mid IV_t^k = 1\right] - E\left[Y_{it} \mid IV_t^k = 0\right]}{E\left[HHI_{i,m} \mid IV_t^k = 1\right] - E\left[HHI_{i,m} \mid IV_t^k = 0\right]} \tag{10}$$

,

Substituting all the expected values with their corresponding averages in the sample, β_{IV} becomes $\widehat{\beta}_{\text{IV}}$ which should be hence interpreted as the difference in the average outcome Y (*i.e.*, log of daily wages; probability of being hired with an OEC; log of hires) across workers belonging to markets whose industry has experienced in the past one or two years at least one merger[19] divided by the difference in average HHI between treated and non-treated markets as predicted by IV_t^k with $k = \{1, 2\}$. Estimates can be interpreted as causal effects as long as the standard conditions of the instrumental variable regression are met, namely the absence of a direct effect of the instruments on the outcomes of interest (*i.e.*, exogeneity), and the correlation with the endogenous covariate (*i.e.*, relevance). I discuss exogeneity in detail in Section 6 in the Appendix.

Relevance. Relevance implies that the instruments must be strongly correlated with the endogenous covariate. First-stage estimates, displayed in Figure B8 in the Appendix, show positive and always significant coefficients, both when the instruments are considered separately and jointly. The IVs additionally satisfy the rule-of-thumb check: All F statistics are far greater than the standard threshold of ten (Stock and Yogo

[18]Formally, a generic Wald instrument is defined as follows:

$$\widehat{\beta}_{\text{Wald}} = \frac{(\bar{y}_1 - \bar{y}_0)}{(\bar{x}_1 - \bar{x}_0)} \tag{9}$$

, where the subscript $_1$ indicates the treated group, while $_0$ the control group. β_{IV} in Equation 10 estimates the difference in average outcome across the two groups divided by the difference in average concentration across the two groups.

[19]It is being assumed that it is possible to define two groups such that the instrument does not directly determine the outcomes, though it does affect the level of concentration and hence indirectly affects the outcomes.

2005). Instruments, although correlated, capture different sources of variation in concentration, as the third model shows that when they are considered jointly, they both remain significant and sizeable.

Results indicate that the coefficients associated with the IVs, indexed by the letter j, range between 0.123 and 0.211 points. As the outcome is a log, coefficients are semi-elasticities, which means that the IVs predict an increase in concentration that ranges between $e^{\beta_j}=(2.718^{0.123}-1)*100=13$ p.p. and $e^{\beta_j}=(2.718^{0.211}-1)*100=23$ p.p.. Those of the two instruments specifications together indicate instead that workers belonging to treated markets on average experience an increase in concentration of 30-38 p.p. with respect to those belonging to non-treated markets. The coefficients do not differ significantly from those estimated in a DiD specification in Arnold 2021. With additional controls, as he manages to include industry-commuting zone-year fixed effects, he finds that the treatment effect ranges between 0.175 and 0.239 points, which translates into an increase in concentration for treated markets from 19 to 27 p.p. with respect to controls.

I. Marinescu, Ouss, and Pape 2021 find that the average percentage points of change in labor market concentration per employee induced by a merger range between 1 and 4, depending on the industry, while Jarosch, Nimczik, and Sorkin 2019 find that the simulated mergers would, on average, increase the HHI by 0.05 points from an average of 0.12. Thus, as I consider in my reduced-form specification all horizontal mergers, the aggregated effect in the treated markets is a multiple of those estimated in previous exercises. Hypothetically, according to my estimates, an industry experiencing five to ten mergers in a given year would experience an increase in line with the results of the previous works.

What I estimate is hence an Average Treatment effect on the Treated (ATT), expressed as an elasticity or a semi-elasticity depending on the outcome, as long as the identification strategy holds. To clarify the notation, the previous parameter of interest, θ, the correlation between the outcome of interest and LLM concentration, now becomes $\widehat{\beta}_{\text{IV}}$, which, under certain assumptions, takes a (previously described) casual interpretation as an ATT. My empirical strategy is similar to those of Guanziroli 2022 and Arnold 2021 when they compare in a DiD framework the outcomes in markets/workers that are "treated" (*i.e.*, that are exposed to mergers) with those that are not. Essentially, that is also what the Wald instrument of Equation 10 does with the only difference that it weights the difference in outcome between the two groups by the change in the endogenous covariate predicated by the instrument.

5.4 TSLS Estimates

Wages. I display results for the three different specifications: in Panel (a) I use the instrument defined in Equation 7, in (b) the instrument defined in Equation 6, while in (c) I use both. Errors are clustered at the market-year-level, addressing the correlation between workers and spells hit by the same shock in a given market and year. The results are shown in Table 4 and indicate that concentration has a sizeable negative impact on entrants' wages. The estimates are larger in magnitude as the different confounders, which are positively correlated with respect to both concentration and wages, induce a downward bias in the endogenous estimates, suggesting in addition that the IVs predict an exogenous variation in concentration.

Dependent variable: ln(Daily Wages)				
Panel (a)	(1)	(2)	(3)	(4)
$\widehat{\beta}_{IV}$	-.319**	-.114**	-.1258**	-.134***
	(.1354)	(.0471)	(.04514)	(.03803)
Panel (b)	(1)	(2)	(3)	(4)
$\widehat{\beta}_{IV}$	-.282**	-.0525	-.0684*	-.209
	(.1189)	(.04419)	(.0404)	(.1754)
Panel (c)	(1)	(2)	(3)	(4)
$\widehat{\beta}_{IV}$	-.300**	-.0920**	-.1052**	-.1393***
	(.0890)	(.0326)	(.0315)	(.0375)
Observations	2,928,818	2,928,818	2,928,474	2,928,474
spell length & age (squared)	√	√	√	√
part time dummies	√	√	√	√
worker FE	√	√	√	√
year FE	√	√	√	√
industry FE	-	√	√	√
region FE	-	√	√	-
occupation FE	-	√	√	-
size FE	-	√	√	-
reg-ind-occ FE	-	-	√	√
occupation-year FE	-	-	-	√
size-year FE	-	-	-	√
region-year FE	-	-	-	√

SE clustered at the market-year-level.
Daily wages are the ratio of overall remuneration and the number of worked days
*** p<0.01, ** p<0.05, * p<0.1

Table 4: **IV estimates of Equation 5.**

Notes: Observations are 3,573,677 yearly spells between 2005 and 2018. $\widehat{\beta}_{\text{IV}}$ is formally displayed in Equation 10. Panel indicate different instruments: (a) 2-years lagged mergers as in Equation 7; (b) 1-year lagged mergers as in Equation 6 and (c) both jointly. *Notes*: observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and market fixed effects.

Estimates magnitude and significance differ little across specifications, but the instrument of Equation 7 seems to be the most relevant. Estimates range between -0.14 and -0.068 p.p., while the preferred ones, those clearly significant, are between -0.14 and -0.09. It follows that a 10 p.p. increase in market concentration reduces new hires' wages by approximately 0.9-1.4 p.p.. Estimates differ from those of the literature: I. Marinescu, Ouss, and Pape 2021 reduced-form elasticities range between -0.067 and -0.052 points, which indicate a reduction in wages following a 10% increase in market HHI of 0.67 and 0.52 p.p..

My estimates are in line with those obtained simulating an *horizontal merger* between two top-employing firms raising concentration by 10 points in I. Marinescu, Ouss, and Pape 2021 as they find a reduction in new-firm wage bill of approximately 7 p.p.. Arnold 2021 finds elasticities that range between -0.3 and -0.08 p.p., and his findings are quantitatively confirmed by simulating different mergers according to the structural model developed in Berger et al. 2023[20]. Jarosch, Nimczik, and Sorkin 2019 find reduced-form elasticities for wages ranging between -0.18 and -0.09 p.p., while, simulating a horizontal merger shifting a market from average to high concentration (*i.e.*, from the 25^{th} to the 75^{th} percentile in HHI distribution), they find that wages decrease by 1 p.p.. These values are higher than those on average estimated in the literature and more similar to mine.

In summary, my estimates stand in the middle between those obtained with the leave-one-out IV (I. Marinescu, Ouss, and Pape 2021; Azkarate-Askasua and Zerecero 2023; Dodini et al. 2023a; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022) and those obtained instead relying on mergers in reduced-form estimates (Arnold 2021; Guanziroli 2022), structural models (Jarosch, Nimczik, and Sorkin 2019; Berger et al. 2023), and simulations (I. Marinescu, Ouss, and Pape 2021), while being very close to those obtained with an alternative IV by Luccioletti 2022[21].

Robustness Even though I control for a large set of fixed effects, there could still be different factors, affected by mergers, that might influence the outcomes as well. In this case, the exclusion restriction would be violated and the TSLS strategy would be invalidated. These threats are mainly two:

Merger-induced gains. Mergers might entail productivity and profitability gains and higher investment, which leads to higher growth in general. This mechanism also relates to the absence of product market concentration, which can be correlated to all these gains and, in turn, to the outcomes. In my setting, these threats would affect the outcomes across the industries and over time. To take this threat into account, I add to the regressions a large set of time-varying industry-level controls that capture profitability, revenues, growth, profits, and investment dynamics across industries and over time. I select 54 variables, drawn from *Eurostat*, to be included in the regressions. I provide the full list in Appendix 6. To rule out the fear of collinearity, as these variables are all highly correlated, I perform an additional exercise: I run a Principal Components Analysis on the 54 variables, saving the first 10 components that roughly explain the 77% of the total variance (the first two alone explain approximately the 35%). The advantage of this strategy is that the factors produced by the PCA are, by construction, orthogonal to each other. Thus, I can largely reduce the number of controls, additionally ruling out the collinearity concern. Hereafter, I report the results

[20] The authors estimate elasticity ranging between -0.44 and -0.11 percentage points.

[21] He uses an IV based on the changes in the local size of the public sector in Spain estimating elasticities ranging between -0.14 and -0.07 p.p..

for daily wages obtained with the full set of controls (Column (4) of Table 4) for the three different sets of instruments. Using the first 10 more explanatory components and IV1, the estimated elasticity is still not statistically significant. With IV2 and both instead, the significance is still high, and the coefficients overall are unchanged. In the former, the coefficient is -.132, the SE is .0375 and the t-statistic is equal to -3.53. In the latter instead, the coefficient is -.138, with a standard error equal to .0372 and a t-statistic equal to -3.71. Both coefficients are still significant at the 99% confidence level, therefore suggesting that the bias induced by the side effects of mergers is not of concern for the TSLS strategy.

Merger-induced layoffs. Mergers might also entail large layoffs that, in turn, affect wages and employment. To take this concern into account, I control for the number of layoffs that happen in each market and year. I calculate layoff from *LoSaI*, which provides for each spell the reasons why it was terminated. Among these reasons, there are different kinds of firings. Therefore, in each market and year, I select only the spells terminated due to the firm's decisions. Hereafter, I report the results for daily wages obtained with the full set of controls (Column (4) of Table 4) for the three different sets of instruments. With IV1, the estimated elasticity is still not statistically significant. With IV2 and both instead, the significance is still high, and the coefficients overall do not change significantly. In the former, the coefficient is -.1386, the SE is .04024 and the t-statistic is equal to -3.44. In the latter instead, the coefficient is -.1444, with a standard error equal to .04022, and a t-statistic equal to -3.59. Both coefficients are still significant at a 99% confidence level, therefore suggesting that the bias induced by the side effects of mergers is not of concern for the TSLS strategy. In all specifications, interestingly but as expected, the coefficient associated with the layoff variable is always negative and highly statistically significant. However, the estimated elasticities are still significant. Therefore, even though layoffs seem to play a role, the TSLS strategy is still robust[22].

So far, I clustered the standard errors within markets and years. The motivation is that the covariate varies across markets and over time. However, in the identification strategy, the variation arises at an industry-level and flows through markets afterwards. Moreover, the instruments I built rely on lagged measures of market exposure to mergers, thus exploiting the time persistence of mergers' effect within each market on concentration. This indicates that the implicit assumption on which my framework is based is that, at least within the same market and year, observations could be correlated as they are exposed to the same shock. To check the validity of the results, I relax this assumption. I allow observations to be correlated within the same market over time, defining the clusters at the market-level. Results are displayed in Appendix 6. In short, the significance of all estimates slightly decreases, but the null hypothesis is always rejected. This exercise proves that the significance of the results is not driven by the cluster level. Henceforth, I cluster standard errors at the market-level to ensure the robustness of the estimates.

[22]For both robustness exercises, the do files to replicate the analysis are available.

Dependent variable:	ln(Hires)	ln(Hires)	ln(Hires)
Panel (a)	(1)	(2)	(3)
$\widehat{\beta}_{IV}$	-.681**	-.681**	-.692**
	(.2819)	(.2821)	(.2867)
Panel (b)	(1)	(2)	(3)
$\widehat{\beta}_{IV}$	-.771*	-.771*	-.747*
	(.4689)	(.4694)	(.4402)
Panel (c)	(1)	(2)	(3)
$\widehat{\beta}_{IV}$	-.699**	-.699**	-.704**
	(.2791)	(.2794)	(.2792)
Observations	47,180	47,180	47,180
(mean) sex & age	√	√	√
reg-ind-occ FE	√	√	√
year FE	√	√	√
occupation FE	-	√	-
region FE	-	√	-
industry FE	-	√	√
region-year FE	-	-	√
occupation-year FE	-	-	√

SE clustered at the market-level
*** p<0.01, ** p<0.05, * p<0.1

Table 5: **IV estimates of Equation 6**.

Notes: Employment is measured as the number of newly activated working spells within each market and year. Full sample is made of 47,727 market-year tuples. Markets are 5,008. $\widehat{\beta}_{\text{IV}}$ is formally displayed in Equation 10. Panel indicate different instruments: (a) 2-years lagged mergers as in Equation 7; (b) 1-year lagged mergers as in Equation 6 and (c) both jointly.

Employment. The exogenous estimates are larger in magnitude than the endogenous ones as the different confounders, which are positively correlated with both concentration and hires, induce a downward bias. Results are shown in Table 5 and indicate that the estimates are stable across panels, with elasticities ranging between -0.68 and -0.77 p.p.. Estimates are slightly greater than those estimated by I. Marinescu, Ouss, and Pape 2021, which range between -0.31 and -0.585 p.p., Arnold 2021, whose elasticities range between -0.9 and -1.4 p.p., and Luccioletti 2022 which, using employment levels, estimates elasticities between -1.5 and 1.7 p.p.. Results indicate that a standard 10 points increase in HHI would reduce hires by approximately 3-6%. The distance in magnitude might be due to the different framework, but most likely to the different identification strategy or the definition of new hires[23]. The more conservative definition of new hires in my

[23]They define new hires as those whose employment spell starts in each quarter, deleting those observations whose job

framework could explain the higher magnitude of my estimates. Results indicate that when a market shifts from low to high concentration, hires reduce by 7-8 p.p..

5.4.1 Extensive and Intensive margin

I now move to explore the mechanisms behind the estimates. The literature on concentration has mostly focused on wages as the main worker-level outcome that is affected by monopsonistic power. However, we know thanks to the whole literature, both theoretical and applied, on labor economics that wages are determined at different levels and are affected by different drivers. In different frameworks, where different forces operate, the final effect can be the same even though the effects might have different signs and magnitudes. For instance, wages could shrink as employers push workers to work more days or, equally, could reduce if employers exert their power to compress only the remuneration. The effect is the same: wages reduce, but, clearly, the mechanisms are different.

Throughout the paper, I referred to these two channels as *extensive* and *intensive* margins. I aim to open the black box of concentration effect on wages in order to shed light on which channels monopsonistic power goes through and how they differ in magnitude, an exercise only slightly performed in the literature[24]. In order to answer these questions, I estimate the following equation on two separate outcomes, namely the number of worked days and overall remuneration:

$$log(O_{i,m,t}) = \delta_i + \mu_m + \gamma_s + \Gamma_{r,t} + \Lambda_{d,t} + \Phi_{o,t} + \beta_t + \theta log(HHI_{m,t}) + \Gamma Z_{i,t} + u_{i,m,dj,t} \tag{11}$$

, where i indexes workers, r regions, o occupations, j firms, d class sizes, s industries, and t years. O indicates an outcome. The model is still specified in a log-log form. θ should thus be interpreted as the elasticity of the outcome with respect to labor market concentration. I present the results only for the two IVs specifications, that of Panel (a) of Table 4, with the full set of controls displayed in Equation 10. Errors are clustered at the market-year-level.

Results are displayed in Figure B9 in the Appendix. The estimated elasticity for days is not significantly different from zero (=0.001, SE=0.049), while for overall remuneration it is slightly significant and equal to -0.12 (SE=0.059). They suggest that employers exert their monopsonistic power only through one channel, the extensive margin one. The intensive margin, instead, is not affected at all by concentration. It seems

spells start on January 1^{st} for each year. I have additionally deleted all transformations keeping only new activations and all observations for an individual that was working in the same firm the previous year.

[24] Unfortunately, my data does not allow me to disentangle, in the spirit of Qiu and Sojourner 2022 and Dodini et al. 2023b, the effect on the intensive margin side between a feasible shift of performed tasks, from more to less productive, and/or a decrease of workers' skills. This exercise requires the use of different data, such as the *Communicazioni Obbligatorie* provided by the Italian Ministry of Labor, which contain detailed data on workers' performed tasks on the job, skills, and educational levels. However, it is an analysis for a separate paper.

thus that employers in more concentrated markets simply can, and thus do, reduce the wages of entrants' workers. They are able to do it presumably due to the high search and matching costs and frictions that characterize Italian LLMs, which prevent workers from easily switching jobs and markets, and due to the few skills acquired by workers over time. I now move on to perform a robustness exercise.

5.5 Job Security

Wages are not the only worker-level outcome affected by concentration. Amodio, Medina, and Morlacco 2022 find that, the higher concentration, the higher the rate of self-employment in Peru. Lamadon, Mogstad, and Setzler 2022 find that monopsonistic labor markets increase workers' misallocation to firms, while Dodini et al. 2023b find that workers exposed to higher levels of concentration have substantially worse subsequent labor market outcomes. There is evidence of an analogous shift in the Italian context regarding two labor market reforms reducing EPL in Italy in 2012 and 2015 (Ardito et al. 2022, Bottasso et al. 2023). To the extent to which an EPL reduction can be potentially considered an increase in employers' power, a similar mechanism might apply to concentration as well.

There is instead a dimension of worker's welfare that could be affected by employers' power that is not captured at all by the wage, namely job security. In a highly dual labor market such as the Italian one, where the costs in terms of uncertainty, firing possibilities, and monetary compensation for workers are definitely lower for fixed-term with respect to open-ended contracts, firms can overcome workers along different dimensions. They might, for instance, decide to hire but on a temporary basis to secure their possibilities in the near future to dismiss these workers with little or no costs at all. Avoiding looking at the types of contracts that are activated among the new hires would thus result in overlooking potentially relevant mechanisms[25].

Practically, I test whether concentration affects workers' likelihood to be hired with open-ended contracts. It could be the case that, in markets characterized by higher concentration, firms employ more workers with FT contracts with respect to OEC. Bassanini, Bovini, et al. 2022 find no effect in Italy and Spain on this outcome, while they find a small and negative one in France and Germany. Qiu and Sojourner 2022 find a similar effect in the US as well[26]. I estimate the following equation:

$$P_{i,m,t} = \delta_i + \mu_m + \gamma_s + \Gamma_{r,t} + +\Lambda_{d,t} + \Phi_{o,t} + \beta_t + \theta log(HHI_{m,t}) + \Gamma Z_{i,t} + u_{i,m,t} \quad (12)$$

[25] This exercise can be seen as a piece of a broader picture portraying the effects of employers' power. This is implied by the definition of labor market monopsony, which is a situation in which employers, in order to maximize profits, decide to reduce labor use, remuneration, or both.

[26] The different effect could be attributed to the fact that Italy and Spain are well known for largely relying on fixed-term contracts, while other labor markets are not.

with $P_{i,m,t} = \begin{cases} 1 & \text{if worker } i \text{ in market } m \text{ is hired with an OE contract in year } t \\ 0 & \text{if worker } i \text{ in market } m \text{ is hired with a FT contract in year } t \end{cases}$

, and i denotes the worker, r the region, o the occupation, j the firm, d the class size, s the industry, and t the year. P is a dummy variable taking value equal to 1 if worker i in region r, with occupation o, in firm j of class size d and industry s in year t is hired with an OEC, 0 otherwise. Z contains a cubic polynomial in age and spells length to proxy individuals' specific on-the-job working experience. $u_{i,m,t}$ is an error term. Standard errors are clustered at the market-year-level. The equation is specified in a linear-log form, and thus θ should be interpreted as a semi-elasticity of the probability of being hired permanently and concentration. The equation is estimated with OLS, thus as a Linear Probability Model. Estimates are displayed in Figure B10 in Appendix 6 and refer to the two instruments' specification with the full set of controls displayed in Equation 13[27].

Overall, I find a null effect on the probability of being hired on a permanent basis, coherently with Bassanini, Bovini, et al. 2022. I find a semi-elasticity of 0.0044 (SE=0.0315; t-statistic=0.14; p-value=0.888), with standard errors clustered at the market-level. Estimates remain not significant, even clustering at the market-year-level (SE=0.0224; t-statistic=0.2; p-value=0.842). Overall, my results indicate that concentration does not affect the probability of being hired on a permanent basis. Employers' power does not seem to damage job security in the Italian labor market. It thus seems that employers, when they have power and the chance to exert it, simply do not hire some workers rather than hire them on a temporary basis. This is likely due to the fact that in Italy most workers are anyway hired at the beginning with fixed-term contracts, as firms want to have the chance to evaluate workers and keep them only when they prove their value. Therefore, there is little room to increase the probability of hirings on a temporary basis[28]. I now move on to investigate the heterogeneity.

5.6 Heterogeneity

Sex. Dodini et al. 2023a and Manning 2021 find that monopsony explains gender wage gap dynamics in the UK and Norwegian labor markets. Sulis 2011 and Fanfani 2022 find similar evidence in Italy. I can test the same predictions in my setting, hence exploring whether, and to what extent, merger-induced shocks in concentration hurt differently the wages of men and women. I additionally deepen whether they affect job security of men and women differently. To my knowledge, I am the first to explore this potential dimension of merger spillovers. The estimates are shown in Figures B12 and B11 in Appendix 6.

[27] I do not attach the estimates across all the specifications as it would be redundant, but they have been performed. Results are coherent with those displayed.

[28] This does not mean, however, that concentration in the labor market does not affect job security. It might indeed be the case that higher employers' power reduces the likelihood of conversion.

Wages. Estimates are significant for women only, not for men. This is very interesting as the joint estimates, displayed in Table 4, are significant regardless of the cluster choice. The significance of the estimates is driven by women. Men are not affected by concentration, as the coefficient remains not significant even clustering the standard errors within markets and years. Although not statistically significant, men's coefficient drives the magnitude of the baseline one upward. The elasticity of women is approximately -0.036 p.p. (p-value=0.012, standard errors clustered at the market-level) which is in line with that estimated in the literature[29]. This finding explains the greater magnitude of the baseline results of Section 5.4 that I have previously attributed to the different identification strategies.

Job security. For men there is no effect at all, while for women the effect shows up and it is slightly significant (t-statistic=-1.81; p-value=0.07) when clustering the standard errors at the market-level, and even more when clustering them at the market-year-level (coefficients become significant a the standard 95% confidence level). Our results indicate that concentration damages workers along different dimensions at the intensive and extensive margin, but mostly for women. Overall, job security of men is not affected by monopsonistic dynamics.

Concentration levels. I split concentration into 4 brackets: from 0 to .15 in HHI, from .15 to .25, from .25 to .5, and above .5, which indicate respectively a weak, medium, high and very high concentrated market according to the US antitrust recommendations (I. Marinescu, Ouss, and Pape 2021). I aim to test whether concentration shocks, induced by mergers, have a higher detrimental effect on workers' wages in markets that start at different levels of concentration. Arnold 2021 and Berger et al. 2023 find indeed that mergers have greater detrimental effects on wages and employment in more concentrated markets. According to their estimates, in highly concentrated markets, the elasticity is almost 4 times higher than in medium-concentrated ones[30]. The same mechanism could be in place for job security.

Wages. Results are displayed in Figures B13 and B14 in Appendix 6. Mergers-induced concentration shocks do have different effects depending on the pre-treatment level of concentration. However, the effect is well identified for women only, while even in high-concentrated markets men do not seem to be affected by concentration, although estimates show an increasing trend. This is surprising and sheds light on the mechanisms driving gender wage gap dynamics in the Italian labor market. Overall, shifting from a weakly to medium and then to a highly concentrated market, the elasticity becomes twice (-0.073 vs -0.038) and four times (-0.122 vs -0.038) as negative. Estimates remain always significant. These findings are relevant along different dimensions: first, they indicate a striking difference in how monopsonistic dynamics affect

[29] I mainly refer to I. Marinescu, Ouss, and Pape 2021; Dodini et al. 2023a; Gregor Schubert and Taska 2020; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022.

[30] Arnold 2021's elasticities range between -0.31 and -0.08 percentage points, while those of Berger et al. 2023 between -0.44 and -0.11.

individuals by gender, with several policy implications, and, second, the baseline estimates displayed in Table 4 are to some extent biased as they hide a relevant heterogeneity driving estimates magnitude and significance.

Job security. Estimates are displayed in Figure B15 in Appendix 6. There is no effect at all for men, although the coefficients display an increasing trend, while it is relevant and monotonically increasing for women. The estimated semi-elasticity for women is always significant and equal to -0.03 p.p. in weakly concentrated markets, -0.078 in medium-concentrated markets, -0.093 in high-concentrated markets, and -0.272 in very high-concentrated ones. Magnitude therefore triplicates shifting from the bottom to the top of the concentration distribution. These results highlight one simple but very powerful fact: concentration damages job security for women only, with an intensity depending on the starting levels of concentration of the market. What matters thus are not only the variations but also the levels of concentration.

6 Conclusions

In this paper, I investigate monopsonistic dynamics across Italian labor markets by analyzing labor market concentration and its effects on new hires' wages, job security, and employment. On average, I find that the level of concentration in Italian labor markets is weaker than expected, with a median of 0.05 points and a mean of 0.14. This suggests that the majority of labor markets are weakly concentrated, while only a small proportion are highly concentrated. Specifically, approximately only 3% of new hires occur in markets with high enough levels of concentration to raise concerns for competition authorities. I find that concentration increases when it is calculated across industries, suggesting that industry heterogeneity, as also found by Fanfani 2022, drives monopsonistic dynamics. To address endogeneity issues, I exploit this mechanism to implement a novel IV strategy based on horizontal mergers, in the spirit of Arnold 2021 and Guanziroli 2022. I use lagged measures of mergers happening within the same industries to instrument for changes in concentration. This approach exploits a source of variation in markets' exposure to concentration that is exogenous to the determinants of wages and employment outcomes, additionally answering policy concerns.

I find that mergers enhance concentration, which in turn reduces wages and employment, but only through the intensive margin. The estimated elasticities range between -0.14 and -0.09 percentage points for daily wages and between -0.77 and -0.68 percentage points for hires. I find no effect on job security. In a simulation in which a market at an average level of concentration becomes 10 points more concentrated, wages would decrease by 0.9-1.4 percentage points while hires by 7-8 over the following two years. This implies a loss of 9-19 euros per month, or 108–205 euros per year for a full-time worker with an average wage. The estimates hide relevant heterogeneity. Specifically, the elasticity of wages is precisely estimated only for women and equals -0.036 p.p., while for men, although bigger in magnitude, it is not statistically significant. A similar pattern is observed for job security, with estimates becoming increasingly larger in magnitude as the level of concentration increases. This suggests that the levels of concentration, not just the shocks, are crucial in identifying the most problematic labor markets.

Policy implications suggest that competition authorities should be attentive to labor market spillovers resulting from mergers, in addition to the well-known product market ones, coherently with Suresh, E. Posner, and Wey 2018, I. Marinescu, Ouss, and Pape 2021, Cali and Presidente 2023 and Berger et al. 2023. Policy makers should assess mergers on an industry-specific basis, taking into account the targeted industries and their concentration levels. Based on these criteria, I identify six industries as the riskiest. I thus believe that stronger enforcement of antitrust laws, particularly in these specific industries and following a data-driven approach such as the one developed in Berger et al. 2023, might be necessary in Italy as well. To end, future research could explore additional channels through which monopsony might affect workers' welfare. One such channel could be the impact of concentration on workers' human capital, which could be examined by investigating how concentration affects job content and the tasks performed on the job (Bachmann, Demir, and Frings 2022). It would also be worth investigating the relationship between employers' power and the spread of precarious employment forms such as Atypical Work Arrangements (Datta, Giupponi, and Machin 2019).

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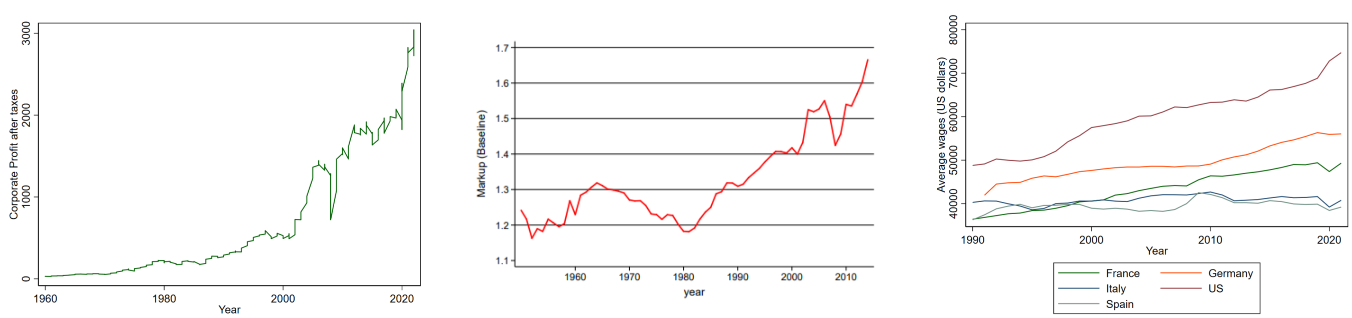
Contents

Appendix

A Introduction



(a) Corporate profits after taxes.

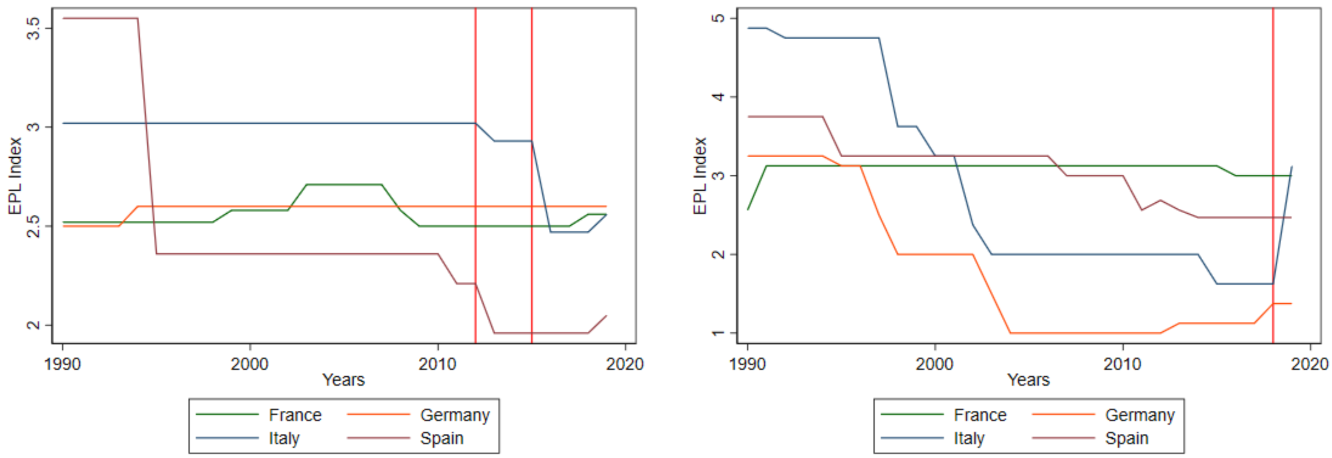
(b) Average Markups.

(c) Average yearly income.

Figure B1: **Emerging trends in worldwide economy.**

Notes: Corporate profits refer to the US, are expressed in billions of dollars and are seasonally adjusted. It is an author's realization based on FRED data available here. Raw data are at a quarterly level and aggregated at a yearly level from 1960 to 2022. Data on Markups for the US are taken from De Loecker, Eeckhout, and Unger 2020 and cover the period from 1955 to 2014. Average yearly incomes are an author's realization. Data are taken from OECD and are available here.

B Institutional Framework - Additional material



(a) Open-Ended contracts.

(b) Temporary contracts.

Figure B2: Strictness of EPL in Europe in the last decades.

Source: Author's realization on OECD 2013a and OECD 2013b available here and here. The time span goes from 1990 to 2020. The vertical red dotted lines represent the main labor market reforms promulgated in this period: in Panel (a), the *Fornero Law* in 2012 and the *Jobs Act* in 2015, while in Panel (b) the *Dignity Decree* in 2018.

C LoSaI

To get an employer-employee dataset, I can use *LoSaI*. It contains several datasets, extracted from the *INPS* administrative archive. The first provides a random set of individuals working spells with many information such as gross remuneration, date (d/m/y) of start/end of the spell, type of contract, linked firm to the spell and other standard information from 1990 to 2018. The spells recorded are all those associated to a random sample of individuals born in days 1 and 9 of any month and year from 1990 to 2018, representative of the Italian working population. The second dataset provides instead registry information regarding the same workers - including the region of residence - which can be linked to the first through a unique code. In the last dataset, I obtain firms' information regarding class size and industry (two-digit NACE Rev.2) ranging from 1990 to 2018.

Firms can be linked to those in the first dataset with an additional unique code. By merging all these sources, I can get an employer-employee dataset in which I observe working spells remunerations within and across triples as defined by the interaction of firms size classes, regions and industry sectors. However, the sample of firms is not obtained based on stratified randomization by size class, region and industry, but according to workers' date of birth. Firms' population thus is likely not representative of the Italian one.

C.1 Additional Descriptive Statistics

Variable	Observations	Mean	St. Dev	Min	$1^{st}Perc.$	Median	$99^{th}Perc.$	Max
Age	3,573,677	35.556	11.194	18	18	34	62	67
Daily wage	3,573,677	61.079	42.146	0.000	0.000	56.494	213.462	700.000
Daily wage (real)	3,573,677	64.393	44.380	0.000	0.000	60.122	226.453	704.935

Table A1: **Summary statistics of *LoSaI* new hires.**

Notes: Observations are 3,573,677 entrants' employment contracts defined as those newly activated for each individual who was not working in the same firm the previous year. Real wages are obtained by deflating nominal daily wages with the 2015 Consumer Price Index (Source: Istat).

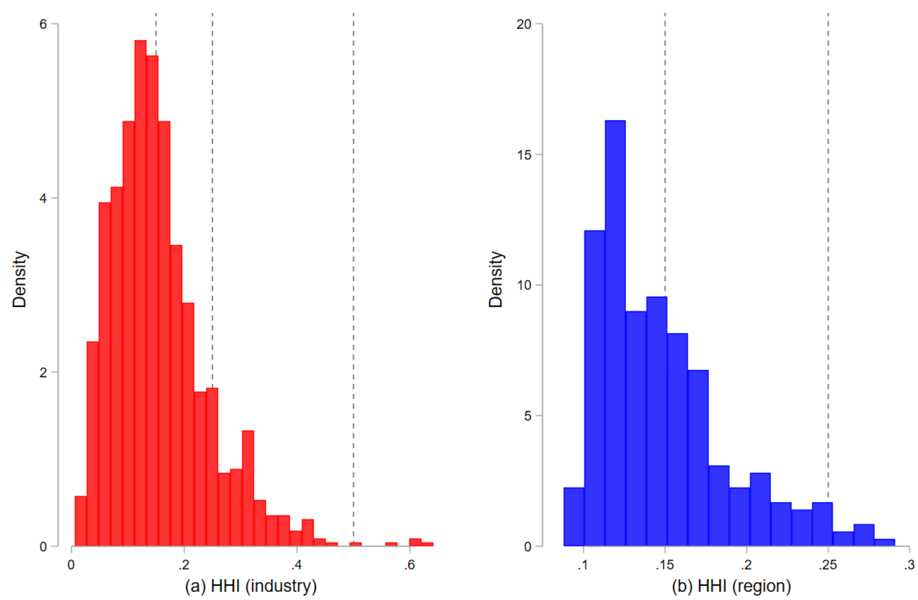


Figure B3: **Distributions of concentration across industries and regions in Italy**.

Notes: Industries are 76 2-digits two-digit NACE Rev.2 cells while regions are the 20 Italians. The time span goes from 2005 to 2018. The dotted lines represent the standard thresholds for defining, respectively, low, medium, high and high levels of concentration. HHI's for industries and regions are calculated as averages of markets HHIs' within a given industry cell or a given regions. Markets HHI's are calculated as the squared sum of class size shares, where the share is calculated as the ratio between hires by market-year tuples of the representative firm in each size class and the total number of hires in that market. Observations are respectively 1,064 industry-year and 280 region-year tuples.

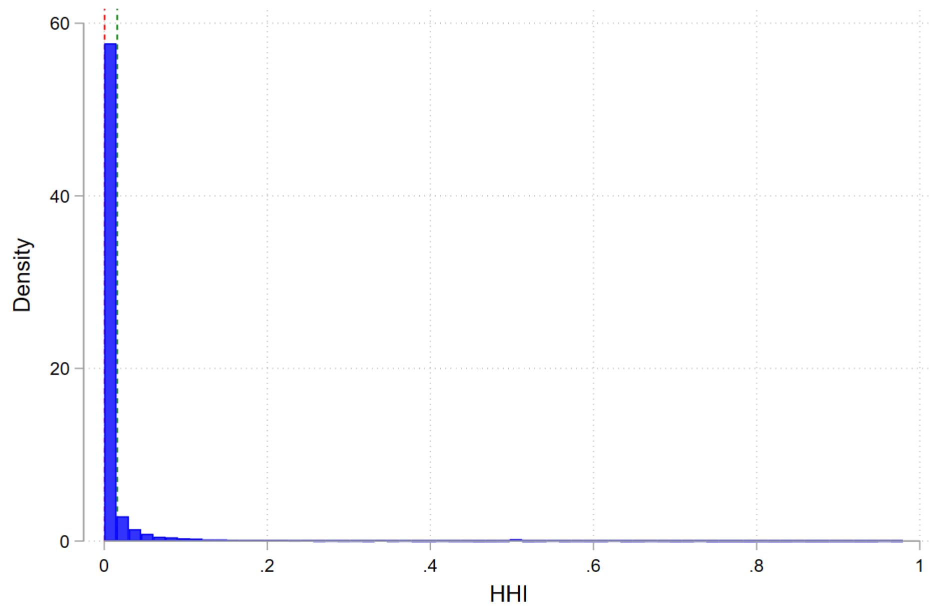


Figure B4: **Distribution of concentration across LLMs in Italy, weighted by each market's spells**.

Notes: Observations are 3,573,677 working spells across 5,008 local labor markets between 2005 and 2018. The two lines represent, respectively, the mean (green dotted, ~ 0.016) and the median (red dotted, ~ 0.005). Markets HHI's are calculated as the squared sum of class size shares, where the share is calculated as the ratio between hires by market-year tuples of the representative firm in each size class and the total number of hires in that market, following the formula in Equation 1.

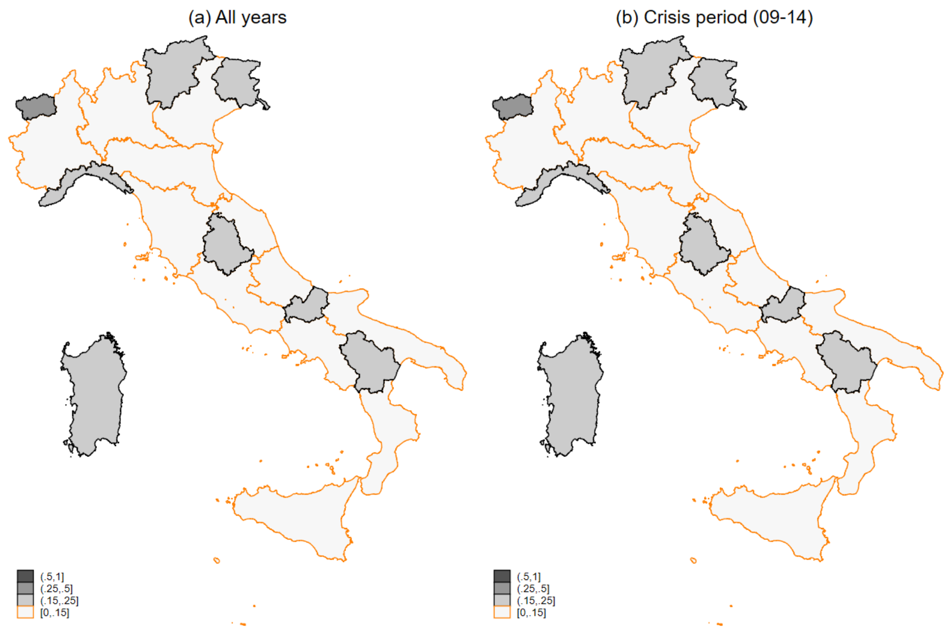


Figure B5: **Map of Italian regions by levels of HHI.**

Notes: Panel (a) indicates all years while Panel (b) considers only the crisis period, which goes from 2009 to 2014. Colours' bandwidths indicate the standard boundaries that define low, medium, high medium, and high levels of concentration. HHI's for regions are calculated as averages of markets HHIs' within each region and across all years in Panel (a) and for 2009-14 in Panel (b). Market HHI's are calculated as the squared sum of class size shares, where the share is calculated as the ratio between hires by market-year tuples of the representative firm in each size class and the total number of hires in that market. Observations are 280 region-year tuples in both panels.

size LoSal	% plants 2018	% plants (2005-2018)	% firm (2005-2018)	size Istat	% firm (2016)
$0-10$	$67,21$	$72,71$	$75,33$	$0-9$	$82,8$
$11-20$	$15,16$	$12,99$	$12,88$	$10-19$	$9,9$
$21-50$	$7,27$	$8,17$	$7,52$	$20-49$	$4,8$
$51-200$	$5,48$	$4,42$	$3,43$	$50-249$	$2,2$
>200	$1,86$	$1,71$	$0,84$	>250	$0,3$

Table A2: **Summary statistics for LoSaI and ASIA firms population, by size class and years.**

Note: Calculations are based on LoSaI, which refers to the period 2005-18 and 2018, and ASIA (ISTAT), which refers to 2016.

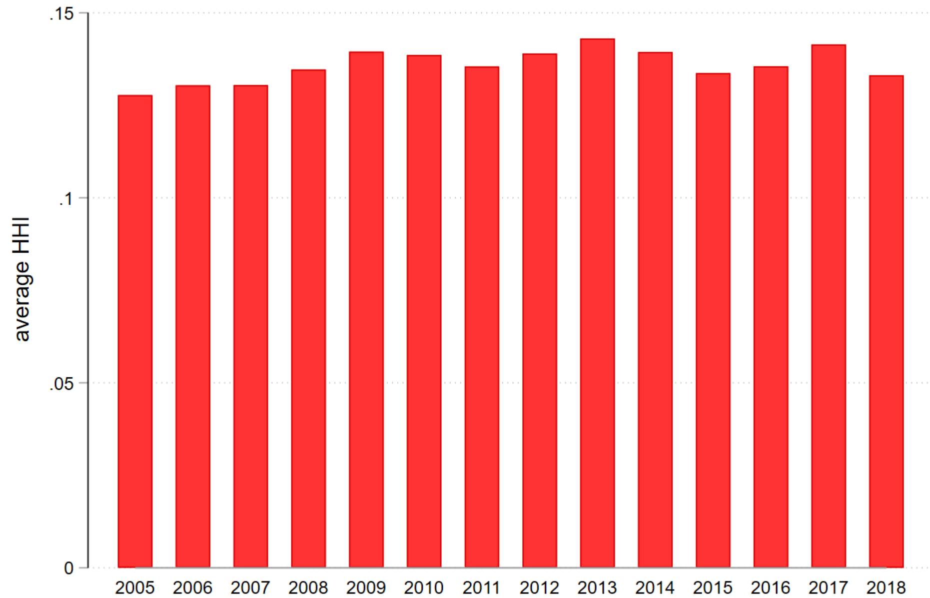


Figure B6: **Average HHI across labor markets, by year, from 2005 to 2018.**

Notes: Markets HHI's are calculated as the squared sum of class size shares, where the share is calculated as the ration between hires by market-year tuples of the representative firm in each size class and the total number of hires in that market. Observations are 47,727 market-year tuples.

C.2 Index limitations

Before proceeding with the empirical strategy and the estimates, it is important to discuss a few limitations of the concentration measure. Although I do not consider these limitations to be too restrictive, there are three noteworthy points. Firstly, I calculate the index within a year, which Azar, I. Marinescu, Steinbaum, and Taska 2020 argue is too long of an interval to capture outside options. However, I have chosen an annual measure as I rely on a sample of workers and a more granular index would yield a different bias due to the small numerosity of workers in each cell. I believe that the latter bias would have a greater impact than the former. It is worth noting that Azar, I. Marinescu, Steinbaum, and Taska 2020's observation applies to the US labor market, where mobility is significantly higher than in the Italian labor market, and thus the bias is less pronounced.

Secondly, it is important to note that the literature on labor market concentration typically uses more detailed measures such as commuting zones, whereas I have used regions due to the unavailability of a more granular measure of worker geo-location. Similarly, the industry is defined at a 2-digit NACE level and occupations are classified into five categories, which may not be precise enough. To account for these limitations, I have interacted the region with industries and occupations jointly, resulting in a substantial number of markets even after applying several cleaning procedures (slightly more than 5,000). However, it is crucial to acknowledge that my concentration index relies on the assumption that firms hire within a region, which is the mechanism through which non-competitive dynamics arise and which I have captured in my data to estimate their effect on workers' wages and employment.

All the aforementioned limitations may induce an upward bias in the concentration index, which could weaken the robustness of the following descriptive analysis. However, when comparing my findings with those of Martins 2018, it further confirms the main implication that Italian workers, on average, are exposed to much weaker levels of concentration than their Portuguese counterparts. Moreover, for the empirical strategy, I rely on a variation in my measure of concentration induced by mergers. The shock is clear of this bias, and thus, it does not affect the empirical strategy, but only the descriptive evidence. The same applies to the paragraph on the role of financial turmoils, as the mechanism is based on a variation and not on the levels.

Finally, it is important to note that the data used in this study only represent a sample of workers, not the entire population. This may introduce an upward bias in the estimates, as not all new hires are captured. To address this, I dropped cells where only one hire occurred, although this adjustment may not completely eliminate the bias[31]. As a result, my index of concentration is likely inflated. Therefore, any interpretation of the levels of concentration should be taken with caution. However, the limitations discussed here do not affect the identification strategy, the comparison with labor markets in Portugal, the role of financial shocks, or most of the descriptive analysis. In conclusion, while there are limitations in the estimated levels of concentration, the identification strategy and the take aways drawn from the descriptive analysis are valid.

D Empirical strategy: Weaknesses and Limitations

I estimate the models including a full set of fixed effects and controls at a worker and market-level, both time-varying and not. Year fixed effects capture macro shocks, homogeneous across regions, industries and occupations, happening at a national level and possibly influencing wages and firms' hires dynamics, such as workers' out-of-work benefits which are set at a national level, macroeconomic fluctuations and trend effects. Occupation-year, size-year, and region-year fixed effects capture instead specific time-varying dynamics across regions - capturing local specific employment dynamics -, firms' size - capturing yearly specific productivity trends for firms of the same size class - and occupations. However, industry-specific time trends, firms' productivity, and market tightness shocks raise concerns about the robustness of Equation 4.

I am already controlling for market, occupation-year and region-year fixed effects but not for industry-year. This means that whether during the period of analysis a yearly-industry-specific shock affecting wages occurs, estimates would be biased. Including firms' fixed effects would solve the former, but as described in the introduction *LoSaI* is not representative at a firm level. *LoSaI* is instead representative across firms'

[31] I additionally dropped those with one and two hires only in a given year and overall concentration levels and the estimates do not change. The results are not attached to the paper.

size classes, and hence I control for size-year FEs. However, the presence of firm-specific characteristics correlated to the outcomes of interest - such as productivity, human capital, employers' attitude and others factors explaining wages heterogeneity - would bias the estimates (Abowd, Kramarz, and Margolis 1999). The proper and easiest way to rule them out is to control for firm fixed effects (*e.g.,* Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022; I. Marinescu, Ouss, and Pape 2021). However, my dataset does not allow to see the employment history of all firms and hence I cannot control for firm fixed effects.

However, to the extent that these characteristics are in common among firms belonging to the same markets, then the inclusions of market fixed effects rules out the bias, otherwise is would be a bias in my specifications. I control for both market and region-year fixed effects as proxies. Ideally, I should build more detailed measure of labor market concentration at a geographical level (*e.g.*, I. Marinescu, Ouss, and Pape 2021; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022). Commuting Zones are the preferred choice as they precisely take into account local employment dynamics, especially in country like Italy which is characterized by a dense presence of the so-called "distretti industriali"[32]. However, I have no access to further information beyond the region and hence I cannot improve the specification.

Another concern is raised by the absence of product market concentration: its omission presumably biases the estimates downward, as it's established in the literature (I. Marinescu, Ouss, and Pape 2021; Dodini et al. 2023a; Bassanini, Batut, and Caroli 2023) that it's correlated positively with concentration and negatively with wages. Unfortunately, I don't have access to firm level information regarding prices and Markups. However, this bias is likely due to the inclusion of market and year fixed effects. The latter issue is reverse causality, which is induced by time-varying market-level shocks influencing simultaneously wages and concentration. The trigger relies primarily in market tightness, which is correlated to both wages and concentration as it depends simultaneously on hires and vacancies. There is no way to properly take this mechanism into account in a reduced-form model, as the proper way is to set up a structural model that simultaneously realize the covariate and the outcome. However, I'll address this threat in the next section relying on a IV strategy.

There might be other confounding effects. Industry-year shocks influencing simultaneously concentration and wages or trade shocks (*e.g.*, china trade shock) targeting specific industries in specific point in time influencing human capital, productivity or revenues. This would bias the estimates as I do not control for industry-year fixed effects. A mass layoff occurring in a given market certainly would increase concentration, but at the same time also has a direct and significant effect on wages and hires. Ideally, I should control for market-year fixed effects, ruling out the presence of all kinds of confounding effects at this level. However, in the literature market-year fixed effects are never included as collinearity likely arises with respect to the

[32] With "distretti industriali" the literature indicates clusters of firms, whose businesses are in general tied one to each other, located in the small geographical area.

remaining set of FEs, resulting in inflated standard errors.

Moreover, there's an additional relationship between wages and concentration: on one hand, everything else equal, higher wages attract more workers and therefore increase markets' concentration. On the other hand, if there is labor market power on the employer side, I expect two workers with the same characteristics to be paid differently depending on the specific local labor market concentration. These two mechanisms cancel out and their interaction does play a relevant role in terms of the magnitude of the bias, as the endogenous estimates contained in the empirical literature are bounded to zero with respect to those exogenous[33]. The specification in Equation 5 additionally suffers of reverse causality because of the mechanical relationship that assigns higher concentration to markets with fewer spells. This bias is inevitable as long as the outcome is measured as a flow. The opposite instead holds for markets with more spells. I again expect the exogenous estimates to be larger in absolute terms because not constrained towards zero.

E Mergers and Concentration: Additional materials

E.1 Extendend Literature

E. A. Posner and I. E. Marinescu 2020 discuss extensively the need for a more intense antitrust regulation, focusing on the US, in order to prevent the birth and the growth of monopsonistic dynamics the in labor market. They explicitly mention mergers and acquisitions as a potential trigger for monopsonistic dynamics, especially when combined with relevant labor markets frictions and anti-competitive behaviors, *e.g.,* non-poaching and non-competitive agreements[34]. I. Marinescu and Hovenkamp 2019 discuss the role played by M&A's in the Labor Market, highlighting the dangers that growing concentration caused by mergers can cause for workers' wages and employment. In fact, they exhort authorities to take into evaluation labor markets spillovers when they evaluate mergers besides those on prices and Markups.

Shapiro 2019 also argues that antitrust law should be enforced. There is indeed convincing evidence that larger, more efficient firms have been growing at the expense of their smaller, less efficient rivals, causing industry concentration in the US economy to increase. He adds that the fundamental challenge for merger control is that it is a predictive exercise: seeking to identify the subset of mergers that "may substantially lessen competition," one must assesses the likely competitive effects of a proposed merger *before* it is consummated.

Jarosch, Nimczik, and Sorkin 2019 simulate the merger between two largest employers in each labor market with Austrian firm level and stock data and re-compute wages at all employers. On average, wages at merging firms decline by seven percent. Mergers have large spillovers also on other workers, whose wages

[33]For a detailed discussion of the feasible channels that this mechanism might take read Azkarate-Askasua and Zerecero 2023 that extensively discusses it.

[34]These mechanisms are discussed extensively in Boeri, Garnero, and Luisetto 2023; Sarfati 2020; OECD 2020.

decreased by 3%. Their model also implies non-linear effects of concentration on wages: large effects are estimated in already highly concentrated markets. From the 25^{th} to the 75^{th} percentile of the concentration distribution, such a merger would depress wages by about 1 p.p..

Suresh, E. Posner, and Wey 2018 discuss mergers that would require more scrutiny by antitrust authorities. They emphasize various thresholds of the change in HHI from the merger that would generate extra scrutiny, indicating that the threshold is when HHI increases by more than 0.2. This happens in about 5% of their events and 40% of those analyzed by Jarosch, Nimczik, and Sorkin 2019. Sarfati 2020; OECD 2020 indicate that mergers are a channel through which concentration enhances. Manning 2003; Manning 2021, providing a list of environments in which monopsony plays a role, urges competition authorities to address the role played by M&A's. The authors' motivations are similar to those of I. Marinescu and Hovenkamp 2019: mergers between large firms, especially in already concentrated and/or small markets, gather employment and increase concentration, which in turn enhances employers power reducing the extensive (wages) and intensive (employment) margin.

Dodini et al. 2023a address the threats posed by mergers to the Norwegian labor market, proving that on average concentration is lower than expected and therefore many relevant M&A's have been denied to safeguard a competitive framework when there was no need to. I. Marinescu, Ouss, and Pape 2021 is one of the few empirical works addressing this topic: they simulate a merger between two top employers in a given industry, finding that it would increase concentration significantly with a sizeable detrimental effect on wages and hires. Mergers are highly heterogeneous across industries and localities. They find that the most vulnerable workers are in disadvantaged areas, both in the North and the South of France.

Arnold 2021 estimates a difference-in-difference specification, on US data, comparing outcomes for entrants' workers in markets experiencing mergers with respect to those which don't. He finds that not all merger events increase concentration and that the effect is not constant along with concentration distribution: it is indeed stronger in higher concentrated markets and negligible for others. Elasticities are significantly higher than those estimated in the literature, ranging between -0.3 and -0.2 p.p.. This result suggests that, beyond ruling out endogeneity, mergers account for a different channel of concentration variation having a more detrimental effect.

Finally, Guanziroli 2022 estimates the effect of labor market concentration on wages leveraging on a large merger in the Brazilian retail pharmacy sector. He finds that increasing market power lowers wages, but less than previously thought, for two reasons. First, failing to account for composition effects biases estimates of the effects of concentration. Second, the negative labor market effects of a merger are offset by competitors' responses. The effect is also heterogeneous depending on workers' characteristics.

E.2 *Zephyr* Archive

The *Bureau Van Djik* is the worldwide leader in providing all sorts of information regarding businesses and industries, across the world. It also has information on an unrivalled number of deals, stored in the *Zephyr* database. *Zephyr* covers over ten years of history for deals around the world and an even longer history for deals with a European counterpart. It also has information on rumours as well as announced and completed deals, from the end of the '90 to today. It covers all types of deals, from standard MAs to joint ventures, de-localizations, or closures. The full database contains more than a billion records. Headline, type, status, value, and details of the target, acquirer, and vendor, including country and activities, plus regulatory bodies, are contained in the database, as well as information regarding target, acquirer, and vendor employment volume. In Table A3 we provide the list of the most targeted industries and the corresponding 2-digit code according to the two-digit NACE Rev.2 grid.

Label	**2-digit NACE code**
Financial Activities	64
Information and IT Services Activities	63
Editorial Activities	58
Electric and Gas Furniture	35
Satellite Telecommunication	61
Manufacture of machinery and equipment	28

Table A3: **Two-digit NACE Rev.2 industries and corresponding label of the six most merger-targeted industries.**

Figure B7: **Scatterplot between average (log of) concentration and the number of mergers across industries and years**.

Notes: Observations are labelled with the corresponding two-digit NACE Rev.2 code. The label associated to each code is displayed in Tabel A3 in the Appendix. Mergers are approximately 200 events between 2005 and 2018. On the y-axis there is the average industry HHI, computed as the average of the HHIs of all markets associated with that industry, while on the x-axis there is the number of mergers that happen for each industry and in each year between 2005 and 2018. Observations are approximately 1000 industry-year tuples associated with 71 two-digit NACE Rev.2 industries.

F Instrumental Variable Regression - Additional Material

In this section, I display the results of a few exercises regarding the exogeneity of the instruments and of the First-stage estimates for the different instruments.

F.1 Instruments properties

Exogeneity To interpret the estimates in a causal manner, I need to assess the validity of the exogeneity assumption. This means ensuring that the instruments do not have a direct influence on the outcomes of interest. One potential concern is that mergers might target specific markets due to their unique characteristics. This implies a correlation between market tightness and the instrument, which would violate the exclusion restriction by being associated with both outcomes. However, it is unlikely that this will occur, as the different data sources are merged by industry and year, not by region. Therefore, I use a national-industry-level channel and do not exploit variation in concentration that occurs through regions.

I assume that a merger between two banks in a given year, controlling for observable characteristics at the market, industry, region, and occupation-level, does not directly affect the wages of all employees or firms' hiring in the financial services industry (NACE code 64) in Italy. Rather, it affects concentration in that market, which in turn affects wages and employment. Furthermore, this shock is also independent of the mechanism previously described, which predicts a positive correlation between concentration and wages resulting from firms raising wages to attract workers with specific skills.

The instruments would only violate the exclusion restriction if mergers persist across time within the same markets. To address this issue, I build the instruments based on lagged mergers. This allows me to account for the fact that increases in concentration induced by mergers take time to display, and I can thus rule out the simultaneous determination between concentration and the outcomes. As a result, any endogeneity arising from local labor dynamics can be ruled out. Previous research (Guanziroli 2022; Arnold 2021) has already used mergers to create variations in concentration. They argue that their events were decided at the national level, and the local-based increase in concentration induced by the event is exogenous.

Similarly, in my framework, I rely on a national and industry-level measure of mergers exposure, except for the small population of workers directly targeted by the merger under examination[35]. The exclusion restriction would be violated if mergers directly affect wages through productivity gains. My objective is to isolate the monopsony power effect while controlling for the potential bias posed by productivity gains hence. Arnold 2021 decomposes the average treatment effect of a merger on wages into three components, namely monopsony power, product market power, and productivity gains[36]. My aim is to isolate the former effect, while the latter is a threat.

Clearly, only mergers that affect concentration and productivity simultaneously are cause for concern. However, I believe that the mechanism highlighted in Equation 13 is not a threat in my framework. Productivity gains pertain solely to the merged firms, whereas my instruments assign the treatment to the industry year in which the merger occurs. In this case, the effect on productivity and, in turn, on wages is concentrated in the firms directly involved in the merger. Additionally, the estimator in Equation 10 compares the outcomes in the treated and control groups, with the bias being the difference in wages between the treated and control groups induced by the productivity gains. Even if the bias were relevant, and I believe this is not the case, the inclusion of year and industry fixed effects is likely to remove it, ensuring exogeneity. Exogeneity is supported by the correlation results presented in Table A4, which shows the correlations between the three measures of individuals' merger exposure and their wages across the full sample. These correlations are very small and negligible, and no relationships of any kind appear between the two, suggesting that there is no

[35] Clearly this mechanism involves only a negligible share of the treated workers, considering how the markets are defined in my context.

[36] Formally, he derives the following equation in Section 2.2 at page 6:

$$E\left[\tilde{w}_j(1) - \tilde{w}_j(0)\right] = \underbrace{E\left[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0)\right]}_{\text{monopsony effect}} + \underbrace{E\left[\tilde{\mu}_j(1) - \tilde{\mu}_j(0)\right]}_{\text{market power effect}} + \underbrace{E\left[\tilde{\psi}_j(1) - \tilde{\psi}_j(0)\right]}_{\text{productivity effect}}. \tag{13}$$

direct relationship between the IVs and the main outcome of interest.

The correlations among the three measures of merger exposure are all significant. This indicates that I can use lagged measures of mergers to predict upward shifts in concentration at time t, while also ruling out concerns regarding reverse causality and simultaneous determination of the outcomes and the covariate of interest. A valid concern is that mergers may target markets that are already highly concentrated, leading to an overestimation of the effect of concentration on the outcomes of interest. However, as shown in Figure B7 in the Appendix, mergers occur across markets with different levels of concentration, thereby dispelling the concern that they only target markets that are already highly concentrated.

Another concern is that mergers often result in a decrease in employment, which may lead to a downward bias in estimating the effect of concentration on employment flows. However, in the employment specification described in Equation 6, I model new hires as the number of newly activated employment contracts in each market-year tuple, without taking layoffs into account, as layoffs affect employment levels rather than employment flows. Therefore, these dynamics should not affect the identification strategy. On the other hand, it's possible that following a layoff, merged firms hire more employees to rebuild their workforce, which may induce an upward bias in the estimates of new hires. That's why I use lagged measures of mergers. Likewise, it's possible that bigger and more efficient companies may raise their employment levels. To account for variations in wages and hiring practices across firms of different sizes, I incorporated class size-year fixed effects into all regression analyses[37]. Additionally, mergers have nothing to do with mechanical bias inducing higher concentration in markets with fewer spells. The identification is thus robust with respect to this bias.

To conclude, I perform a robustness exercise to test whether exogeneity holds. I compute the standardized differences for different moments, *i.e.,* mean, median, and standard deviation, of daily wages[38] between treated and untreated industry-year tuples[39]. Results are displayed in Table A5 in the Appendix. The estimated differences for mean and median daily wages always lay within the standard bandwidths, indicating that they are not statistically significant. It means that mean and median daily wages do not differ significantly between treated and not industries. Regarding the standard deviation instead, the difference is almost significant in Panel (a), and slightly significant in Panel (b). This indicates that the instruments affect the distribution of wages within industries but do not affect their levels, thus strengthening the exogeneity assumption. I interpret this evidence as proof of the fact that the IVs do not directly affect the outcomes.

[37] The regressions encompass 196 cells, as class size brackets and years have 14 levels each.

[38] I do not attach the results of the standardized differences for hires and job security in the paper, even though they hold. I do so because I believe that it is sufficient to test instruments' exogeneity with respect to wages, which likely implies that the same holds for all other outcomes as well.

[39] I first collapse the worker dataset into an industry-year one and compute the mean, the median, and the standard deviation in daily wages for those tuples for which I know the number of current, one-year, and two-year lagged horizontal mergers. Using the IVs of Equations 6 and 7, I compute the standardized differences for the three moments of the main outcome between treated and untreated tuples.

F.2 Threats to Identification - Additional Material

In this subsection, I attach the results of different exercises that I perform to prove that the Exclusion Restriction assumption holds in my empirical strategy. The table and the figure are cited in Section 6 in the main text.

Variables	Daily Wages	Mergers (t-1)	Mergers (t-2)	Mergers (t)
Daily Wages	**1.000**			
Mergers (t-1)	**0.0036**	1.000		
Mergers (t-2)	**0.0086**	0.2542	1.000	
Mergers (t)	**0.0088**	0.2395	0.422	1.000

Table A4: **Correlations between Daily Wages and the different measures of markets' exposure to mergers.**

Notes: t, t-1 and t-2 indicate respectively the number of merger events that occurred in the current year and in one and two years previous to the current one for each market-year tuple. Events are approximately 200 in the period of analysis. Observations are 3,573,677 market-year tuples associated to 5,008 markets between 2005 and 2018.

Panel (a): IV_t^2	Not Treated		Treated		
	Mean	St. Dev.	Mean	St. Dev.	Std Diff
Mean	4.1	.29887	4.11	.25463	−0.03682
Median	4.131	.27277	4.15	.20988	−0.07726
St.Dev.	.5259	.10451	.5073	.069363	0.20957*
Panel (b): IV_t^1	Not Treated		Treated		
	Mean	St. Dev.	Mean	St. Dev	Std Diff
Mean	4.1	.29495	4.103	.24318	−0.01161
Median	4.129	.26837	4.131	.20984	−0.00566
St.Dev.	.5218	.10425	.4988	.069976	0.25905*

Table A5: **Standardized differences in the mean, median and standard deviation of (log of) daily wages between treated and not industry-year tuples.**

Notes: The standard bandwidths to assess whether there are significant differences, denoted by *, in a variable are -.15 (25) and .15 (25), according to Imbens and Rubin 2015 and Bayoumi 2022. IV_t^2 indicate the 2-year lagged Mergers instrument, while IV_t^1 indicate the 1-year lagged Mergers instrument. On the rows, there are the industry-year mean, median and St.Dev. of the outcome, while on the rows there are the mean, median and St.Dev. across all industry-year tuples used to perform the differences. Observations are 1,064 industry-year tuples associated with 76 two-digit NACE Rev.2 industries.

F.3 First-stage estimates

In this Section, I display the results of the First-stage estimates of the instrumental variables regression. Controls are displayed and commented in Equation 5 and Table 3. I only present the results with the market specifications controls and not with worker fixed effects only as in Table 2. Coefficients are always positive and significant across all specifications, both when considered separately and jointly.

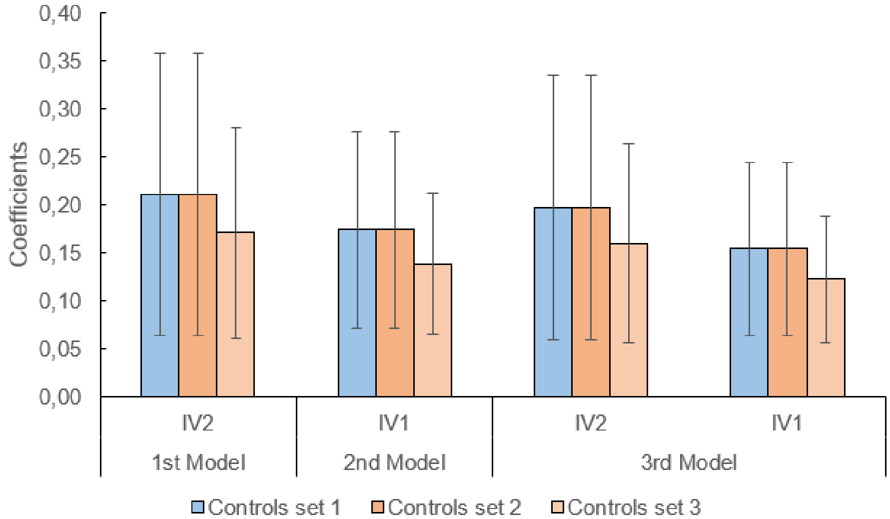


Figure B8: **IV First-stage estimates of Equation 5.**

Notes: "Model" notation indicates a different instrument in use: in (1) 2-years lagged mergers as in Equation 7; in (2) 1-year lagged mergers as in Equation 6, and in 4 both instruments jointly. Estimates should be interpreted as semi-elasticity as the specification is in a linear-log form. The three different sets of controls are displayed extensively in Table A6 in the Section 6 in the Appendix. Errors are always clustered at the market-level. Observations are 3,573,677 yearly spells between 2005 and 2018 associated to 5,008 markets and approximately 1,500,000 workers.

	(1)	(2)	(3)
	ln(HHI)	ln(HHI)	ln(HHI)
Panel (a)	(1)	(2)	(3)
IV_t^2	.2109**	.2109**	.1708**
	(.07448)	(.07448)	(.05573)
Panel (b)	(1)	(2)	(3)
IV_t^1	.1740**	.1740**	.1387***
	(.0520)	(.0520)	(.0372)
Panel (c)	(1)	(2)	(3)
IV_t^2	.1973**	.1973**	.160**
	(.0703)	(.0703)	(.0530)
IV_t^1	.1542**	.1542**	.1229***
	(.0456)	(.04567)	(.0336)
Observations	3,573,677	3,573,677	3,573,677
(mean) sex & age	√	√	√
reg-ind-occ FE	√	√	√
year FE	√	√	√
occupation FE	-	√	-
region FE	-	√	-
industry FE	-	√	√
region-year FE	-	-	√
occupation-year FE	-	-	√

SE clustered at market-level
*** p<0.01, ** p<0.05, * p<0.1

Table A6: **IV fist stage estimates of Equation 5.**

Notes: Panel indicate the use of different instruments: (a) 2-years lagged mergers as in Equation 7; (b) 1-year lagged mergers as in Equation 6 and (c) both jointly. Observations are 3,573,677 employment contracts between 2005 and 2018. Controls are those of Equation 5 and are displayed in Table 3. Errors are clustered at the market-level.

F.4 Robustness check - clusters choice

To test the robustness of my estimates, I now allow the observations to be correlated within the same market over time, clustering the standard errors at the market-level. The reason why I do not cluster at an industry-level is that industry is just one of the levels defining a market. Clustering at an industry-level could be too conservative, in light of the fact that each industry further segments into regions and occupations, and that I exploit a somewhat raw classification of the industry. Consider for instance the logistic industry (2-digit NACE cell number 52), which presumably shows different concentration levels across regions, especially when a merger targets firms in a specific region. The observations that are affected are those that compete in that industry and region, and definitely not those firms operating in the same industry but eventually far away. Thus, the shock I aim to capture influences only some of the markets associated with that industry and hence the most adequate choice to cluster observations is within industries and not markets.

Results for wages hold across the different specifications. In the preferred one, including time-varying control for occupations, firms' sizes and regions and considering the two instruments, Column (4) of Panel (c) in Table 4, the t-statistic is equal to -2.49 and the p-value to 0.013[40]. Considering instead the preferred instrument alone (Column (4) of Panel (b) in Table 4), the t-statistic and the p-value become -2.52 and 0.012. Moving to the specification of Column (3), results for the two IVs specification are again significant (t-statistic=-2.08 and -1.9; p-value=0.038 and 0.057).

G.5 *Eurostat* Data

The full of list of variables extracted from Eurostat at the industry-year-level is the following: *No. of Enterprises, Turnover or gross premiums written, Production value, Gross margin on goods for resale, Value added at factor cost, Gross operating surplus, Total purchases of goods and service, Purchases of goods and services purchased for resale in the same condition as received, Payments for agency workers, Change in stocks of finished products and work in progress manufactured by the unit, Personnel cost, Wages and Salaries, Social security costs, Payments for long-term rental and operational and financial leasing of goods, Gross investment in tangible goods, Gross investment in land, Gross investment in existing buildings and structures, Gross investment in construction and alteration of buildings, Gross investment in machinery and equipment, Sales of tangible investment goods, Net investment in tangible goods, Persons employed, Unpaid persons employed, Employees, Employees in full-time equivalent units, Hours worked by employees, Turnover from the principal activity, Purchases of energy products, Turnover per person employed, Apparent labour productivity (Gross value added per person employed), Wage-adjusted labour productivity, Gross value added per employee, Gross value added per employee FTE, Gross value added per hour worked by employees, Share of personnel costs in production, Average personnel costs (personnel costs per employee), Labour cost per employees, Labour cost per hour worked by employees, Share of employees in persons employed, Growth rate of employment, Employer's social charges as a percentage of personnel costs, Persons employed per enterprise, Gross operating surplus/turnover (gross operating rate), Value added at factor cost in production value, Share of personnel costs in total purchases of goods and services, Share of gross operating surplus in value added, Share of principal activity in turnover (degree of specialisation), Share of value added in manufacturing total, Share of production value in manufacturing total, Share of turnover in manufacturing total, Share of employment in manufacturing total, Ratio of stocks of finished products and work in progress to production*

[40] Results are not attached to the paper but available.

value, Investment per person employed, and *Investment rate (investment/value added at factors cost)*.

G Additional Figures

In this section I attach additional material regarding the Intensive and the Extensive margin analysis, discussed in Section 5.4.1, and the heterogeneity analysis, discussed in Section 5.6.

G.1 Intensive and Extensive Margin

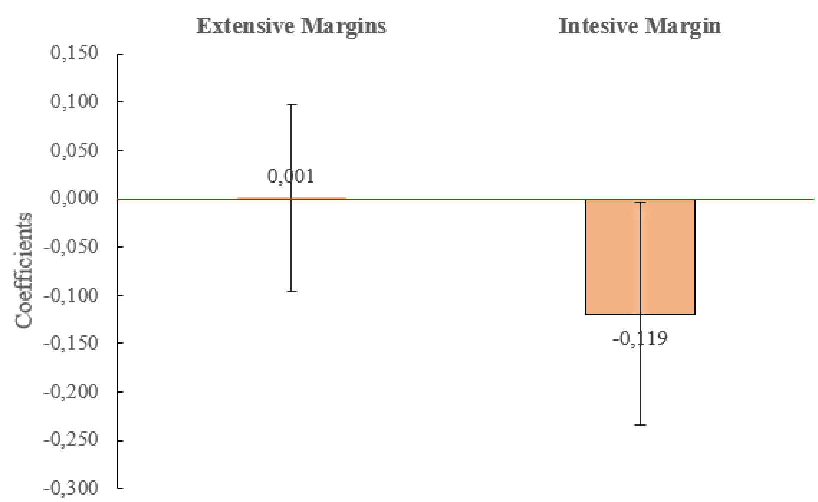


Figure B9: **IV estimates of Equation 11.**

Notes: Extensive margins is the number of worked days for each yearly spells, while Intensive margin is the overall gross nominal remuneration of each spell. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control: market, individual, industry, occupation-year, region-year, and size-year fixed effects. Errors are clustered at the market-year-level. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and market fixed effects.

G.2 Job Security

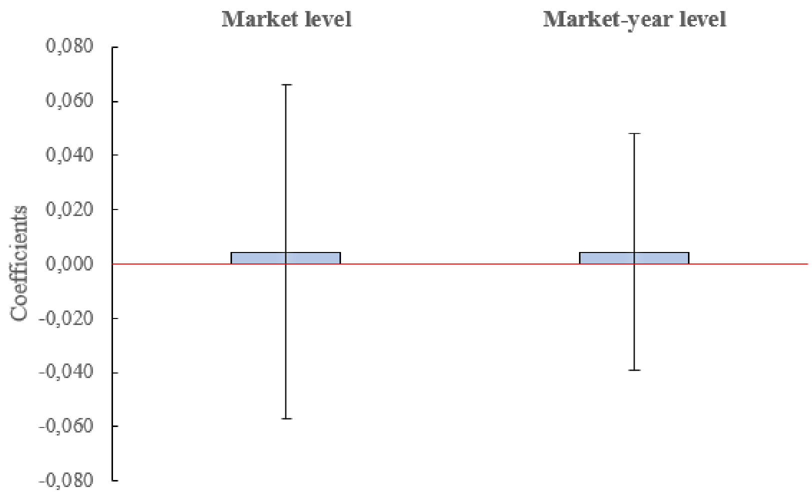


Figure B10: **IV estimates of Equation 12, clustering at a market and market-year-level.**

Notes: The estimates are formally displayed in Equation 9. The two panels indicate a different clusterization level, on the left at the market-level and on the right at the market-year-level. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are 3,573,677 yearly spells between 2005 and 2018. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

G.3 Heterogeneity: Sex

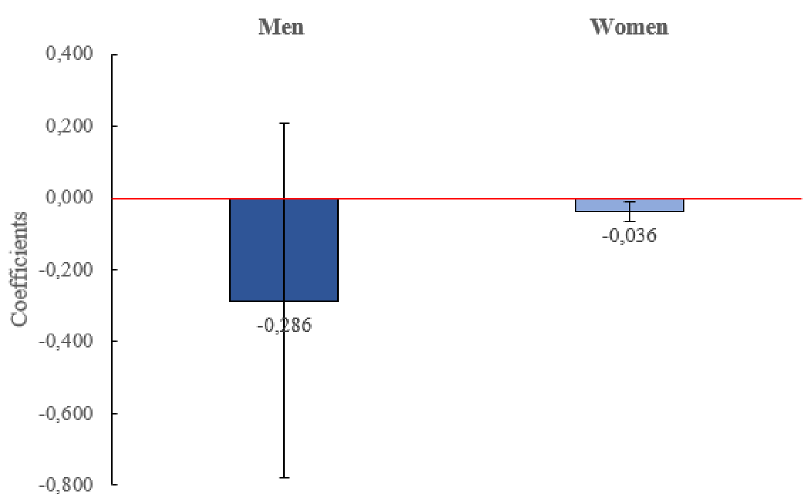


Figure B11: **IV estimates of Equation 5, by sex.**

Notes: The estimates are formally displayed in Equation 9. Estimates are obtained relying on two IVs. Errors are clustered the market-level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

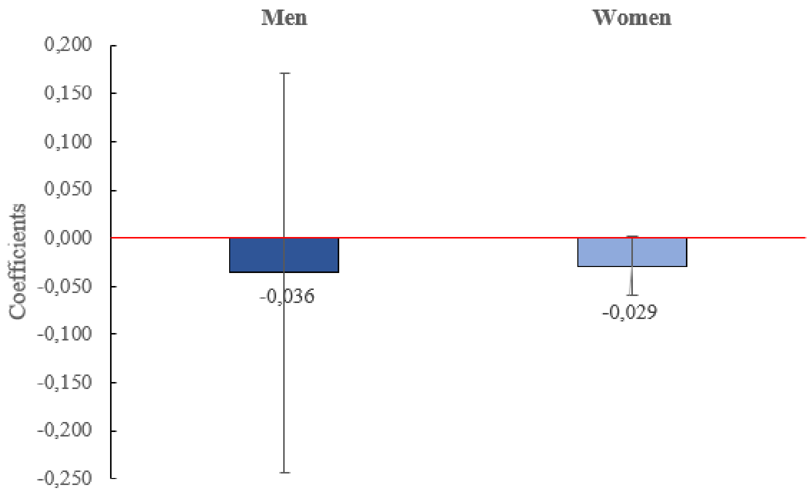


Figure B12: **IV estimates of Equation 12, by Sex.**

Notes: The estimates are formally displayed in Equation 9. Estimates are obtained relying on two IVs. Errors are clustered the market-level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

G.4 Heterogeneity: Concentration levels

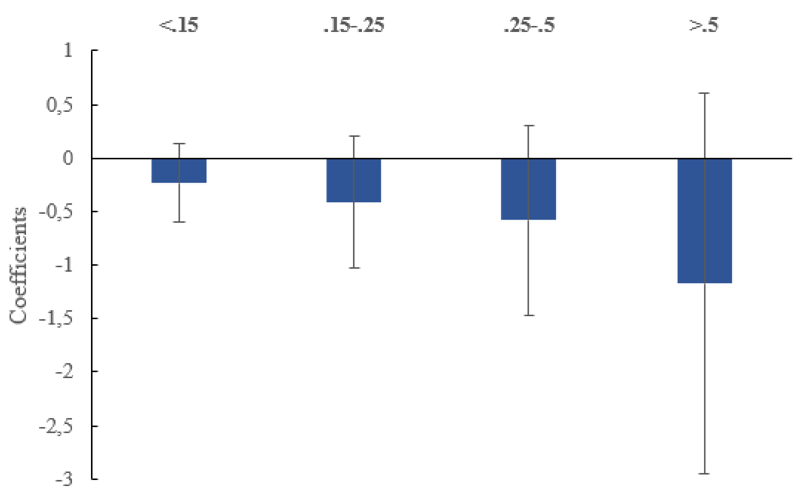


Figure B13: **IV estimates of Equation 5, for men.**

Notes: Concentration is divided in the standard bandwidths defined by the US Antitrust agency: <.15 indicate a weakly concentrated market, .15-.25 a medium concentrated market, .25-.5 a highly concentrated market while above .5 a very highly concentrated market. The estimates are formally displayed in Equation 9. Estimates are obtained relying on two IVs. Errors are clustered the market-level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

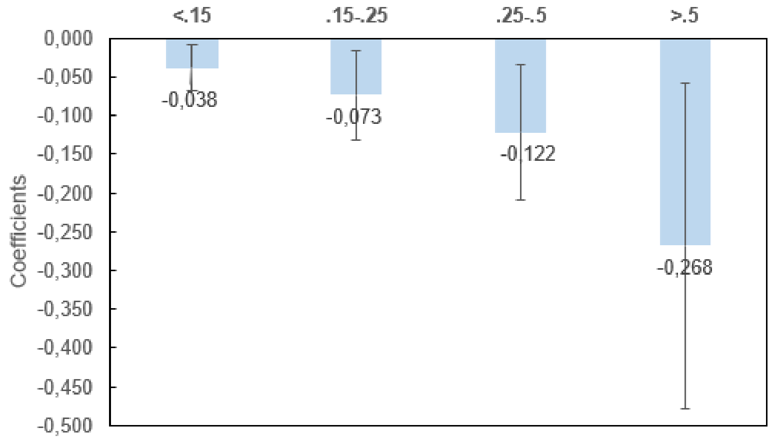


Figure B14: **IV estimates of Equation 5, by concentration levels, for women.**

Notes: Concentration is divided in the standard bandwidths defined by the US Antitrust agency: <.15 indicate a weakly concentrated market, .15-.25 a medium concentrated market, .25-.5 a highly concentrated market while above .5 a very highly concentrated market. The estimates are formally displayed in Equation 9. Estimates are obtained relying on two IVs. Errors are clustered the market-level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

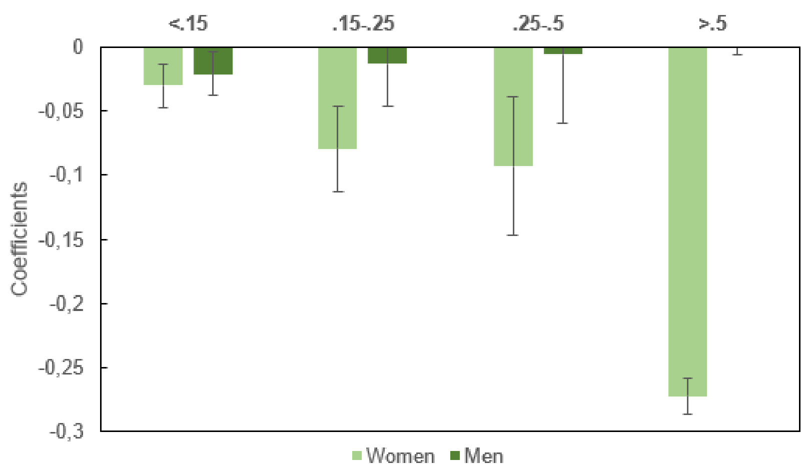


Figure B15: **IV estimates of Equation 12, by sex and concentration levels.**

Notes: Concentration is divided in the standard bandwidths defined by the US Antitrust agency: <.15 indicate a weakly concentrated markets, .15-.25 a medium concentrated market, .25-.5 a highly concentrated market while above .5 a very highly concentrated market. The estimates are formally displayed in Equation 9. Errors are clustered the market-level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.