Working Paper no. **186**

Concentration and mergers: evidence from Italian labor markets

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July, 2024
This version substitutes the previous one dating July, 2023

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Concentration and Mergers: Evidence from Italian Labor Markets*

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July 23, 2024

Abstract

This paper investigates the effects of labor market concentration on employment, job security, and wages. By constructing a flow-based index, I find that concentration is generally low across markets but varies across industries. Then I employ a TSLS strategy based on the different exposures of industries to horizontal mergers. I find that mergers increase concentration, which in turn reduces wages by -0.14 and -0.07 and hires by -0.77 and -0.68 percentage points. I also find that (1) concentration reduces the likelihood of a permanent hire and increases that of a temporary renewal; (2) firms exert their power through different channels on men and women: men are more affected, only through wages, while women are less affected but also through job security; (3) all estimates' magnitude increases in concentration levels.

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

*I am indebted to my PhD supervisor, Claudio Lucifora; to my thesis reviewers, Andrea Garnero and Giovanni Sulis; to the components of my PhD committee, Paolo Ghinetti, Mara Grasseni, and Simona Comi; and to the anonymous OEP reviewers for their valuable comments. I also thank the participants of the 2023 ICEEE conference, the 2022 GRAPE Gender Wage Gap Conference, the French STATA conference in applied econometrics, the IZA workshop on labor market institutions, the 13^{th} IAB workshop on the perspectives on (un-)employment, the 10^{th} Giorgio Rota Conference, the AIEL workshop on labor market institutions, the 14^{th} Trier Workshop, Salvatore Lattanzio, Bernardo Fanfani, Massimiliano Bratti, Fabio Berton, and Giuseppe Grasso.
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1 Introduction

Monopsony indicates a 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). It may also arise in the labor market and affect many outcomes (Manning 2011). Paker et al. 2023 say "*there is nowadays a need to look beyond models of perfect competition to understand wage determination in early modern labor markets*". Similarly, Card 2022 says "*many or even most firms have some wage-setting power*", while Deb et al. 2022 find that monopsonistic dynamics can explain one-quarter of wage stagnation in the United States in recent decades.

The literature has mainly estimated monopsony with the labor supply elasticity of workers to firms. The former find that positive elasticities imply that labor supply increases with wages and vice versa, indicating the presence of monopsony power (*e.g.,* Card 2022; Langella and Manning 2021; Amodio and Roux 2021; Datta 2023; Sokolova and Sorensen 2021). More recently, labor market concentration has been identified as a proxy of monopsony (*e.g.*, Arnold 2021; Azkarate-Askasua and Zerecero 2023; Bassanini et al. 2023; Bassanini et al. 2024; Dodini et al. 2023), especially in the presence of workers' limited bargaining power and relevant labor market frictions (Boeri et al. 2023; Amodio et al. 2022). Theory predicts that when concentration increases, wages and employment should fall (Manning 2021; Azar et al. 2019). Recently, a stream of this literature has started to revise the labor economics literature in light of monopsonistic competition. Wage inequality (Mertens 2021), gender wage gap (Dodini et al. 2023; Fanfani 2022), workers' misallocation (Lamadon et al. 2022), migration dynamics (Manning 2021), and minimum wage (Azar et al. 2023; Popp 2023) have been studied. However, what drives concentration and what all the effects remain largely unknown.

In this paper, I estimate the effect of monopsony, proxied through labor market concentration, on wages, employment, and job security, implementing a novel identification strategy.

Italy is the perfect scenario to address job security in particular, as, in the last decades, different reforms have been implemented to reduce firing costs for larger firms[1]. Araki et al. 2023 indeed find that although European labor markets have stronger institutions than the United States, they are not necessarily more competitive, while Luccioletti 2022 finds that a large share of the city-size wage and employment gap between small and large cities in Spain can be attributed to differences in labor market power exerted by firms across locations. The same might apply to the Italian context.

Furthermore, if for wages and employment there is already evidence in the literature, job security has been instead mostly overlooked in all its dimensions while it may represent a relevant channel through which firms exert their dominant position over workers. This paper is the first to address the effect of concentration on different dimensions of job security for new hires and renewals and speaks to the emerging literature on monopsony addressing these outcomes (Bachmann et al. 2022; Bassanini et al. 2024; Amodio et al. 2022).

Regarding the identification strategy, most of this literature addresses endogeneity through a leave-one-out instrumental variable regression, in which each market concentration index is instrumented with the mean of other indexes in different areas within the same industry or occupation[2]. Only a few papers investigate different channels. Dodini et al. 2023 leverage clusters of skills demanded by firms, Schubert et al. 2024 exploit differential local exposure to national firm-level trends, while Datta 2024 emphasizes the role of the spatial distribution of activity and the distaste of workers for commuting.

Recently, a stream of literature has increasingly turned its attention to antitrust issues in light of monopsonistic competition. A growing number of researchers call for more stringent antitrust enforcement, especially regarding horizontal mergers, which are identified as a driving factor for monopsonistic dynamics (Berger et al. 2023; Posner and I. E. Marinescu 2020;

[1]More details in Section B.

[2]The aim is to capture national-industry or occupational shocks, ruling out the endogeneity induced by local labor market-specific confounders.

Jarosch et al. 2024; Shapiro 2019; Suresh et al. 2018). These scholars argue that the US product markets have become more concentrated in recent decades due to weak merger laws enforced by antitrust agencies. Recently, labor markets have also been scrutinized (Berger et al. 2023; Cali and Presidente 2023). This debate is drawing attention in Europe, but empirical evidence is scarce. I leverage the heterogeneity of labor markets to horizontal mergers to set up a quasi-experiment, additionally contributing as I am among the first to provide causal evidence on this topic.

To my knowledge, only two previous studies used mergers to identify concentration variations in reduced form (Arnold 2021; Guanziroli 2022). However, Guanziroli 2022 focuses on a specific event, and neither explores all the mechanisms and outcomes as I do. Moreover, only two studies have addressed monopsony in Italy (Sulis 2011; Fanfani 2022) while only one labor market concentration (Bassanini et al. 2024), although not specifically in Italy.

Firstly, I compute concentration across Italian labor markets (LMs) using the Herfindahl-Hirschman Index (HHI). I select only entrant workers' spells—a flow measure—from *LoSaI*, a matched employer-employee dataset drawn from *INPS* (Italian Social Security Agency) between 2005 and 2018. I use a flow-based concentration index 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 the available job opportunities for workers (I. Marinescu et al. 2021; Azar et al. 2020).

Markets are defined as interactions between regions, industries, and job titles. The definition slightly differs from the standard one in the literature, which usually defines a market as an interaction between a commuting zone and an occupation or industry. We use this novel definition to (i) obtain a more "granular" index; and (ii) investigate industry heterogeneity in monopsonistic dynamics (Fanfani 2022) as mergers are intrinsically heterogeneous across industries. Most of the 5,008 labor markets identified are not concentrated, as the median value is below the low concentration threshold, and only 3% of workers are exposed to non-low concentration levels according to the US antitrust agency.

I then exploit the *Zephyr* archive to observe horizontal mergers across industries in Italy. I link labor markets and mergers by industry and year and set up a quasi-experimental framework where markets are treated in a year if they experience at least a merger. Mergers do not randomly target markets and are positively correlated with concentration over time. However, industry-level variations are presumably orthogonal to market-specific confounders that affect both outcomes and concentration and bias the estimates toward zero. This allows me to identify unbiased effects of concentration on the outcomes.

TSLS estimates show that in treated markets concentration increases by 15–21 percentage points (p.p. henceforth), which in turn reduces daily wages by approximately 0.09–0.14 and hires by 0.7–0.8 p.p.. The wage effect is entirely driven by the intensive margin (remuneration), while the extensive margin (worked days) is unaffected. I then address job security, measured as the likelihood (1) of being hired with an open-ended contract (OE) and (2) of receiving temporary renewal. I find no effect of concentration on job stability for new hires (Bassanini et al. 2024), but positive on temporary renewal. Renewal wages are also negatively affected, although less than new hires' ones and closer to the magnitude estimated in Bassanini et al. 2023 for incumbents.

Results indicate that employers reduce hires and job stability while leveraging temporary renewals to submit workers to a screening period that secures the chance of dismissals. Simultaneously, they compress wages regardless of job security. Estimates exhibit relevant heterogeneities: wages are more strongly affected for men but precisely estimated for women only, while job security is affected for women only. This suggests that firms exert their power over men and women through different channels. Furthermore, magnitudes, regardless of the outcome and the workers, increase greatly in concentration levels.

To conclude, this paper makes three main contributions: (i) implements a novel identification exploring different channels and heterogeneity; (ii) delves into how firms jointly leverage wages, hires, and renewals to secure their dominant position over workers and (iii) investigates unexplored concerns in Europe on the effects of mergers on the labor market suggesting that more stringent and evidence-based antitrust enforcement might be necessary. The rest of the paper is structured as follows: Section 2 describes the data and labor market concentration; Section 3 presents the empirical strategy; Sections 3.3 and 4 present the identification and the estimates, and Sections D.5, 4.1, and 4.2 contain several robustness exercises and additional analysis. Section 5 concludes.

2 Data and Concentration

2.1 *LoSaI*

LoSaI is a longitudinal sample of workers extracted from the *INPS* universe based on their birth date that covers the period from 1985 to 2018. For each worker, approximately 7% of the Italian private sector, the sample contains all his working spells. For each one, it provides the gross overall remuneration, the days and weeks worked, the job title (employees, managers, middle managers, apprentices, and standard workers), the type of contract, the time schedule, and the region of residence. It also provides a unique firm code that can be matched with a firm dataset to obtain a matched employer-employee dataset. This dataset provides the industry (2-digit), and the size class, which varies over time and is classified into 14 brackets from 1-5 to over 500 employees (Section C.1).

I select only new hires between 2005 and 2018[3]. Although incumbents are also affected by concentration (Bassanini et al. 2023)[4], theoretical predictions and empirical evidence suggest that employers' power compresses entrants' wages more than long-term incumbents, who are more experienced and protected by stronger legislation (Bassanini et al. 2024). New hires are

[3]Spells whose worker did not work within the same firm in the previous year.

[4]They find that, in the French labor market, incumbents are affected by concentration with a magnitude that is from two-thirds to three-quarters of that of new hires.

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. Transformations are kept separate for the job security analysis (Section 4.2). I delete repeated observations for each worker-year keeping the longest spell and obtaining an unbalanced worker-level panel made of 3,573,677 newly activated contracts and 1,400,000 entrant workers (Table C.1.2).

2.2 Herfindahl-Hirschman Index

A labor market is defined as an interaction between an industry s, a job title o, and a region r. The measure of labor market concentration is the standard one in the literature, the Herfindahl-Hirschman Index, whose formula is:

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

, 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 firms' population is presumably not representative. I cannot rely on firms' shares and the HHI as in Equation 1. However, firms' distribution within and across class sizes is similar to that of Italy (Table C.1.1). Therefore, I calculate the concentration by modifying the previous formula:

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

where N_{dm} represents the number of size bins in each market m and k is the ratio of the number of new hires for the representative firm in class d, market m, and year 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 a labor market. The fact that larger firms or plants pay higher wages is widely documented in the literature[5].

The slightly different HHI definition is due to the data features, as *LoSaI* does not cover the universe of firms, but rather a sample extracted on the worker's side. Hence, if I focus on the sample of available firms, it would yield a sampling bias as firms' inclusion depends on their size. Hence, a firm's share HHI might be biased upward, as it is more likely to include larger rather than smaller firms. However, I find that the class size HHI differs little from the firm's share HHI (Figure C.2.2). I discuss in detail my index in Section C.2.

2.3 Descriptive Evidence

I compute concentration for approximately 6,000 labor markets. I delete market-year tuples with one spell only[6]. I obtain an almost perfectly balanced panel of 47,727 market-year tuples and 5,008 markets in Italy between 2005 and 2018. I describe concentration across labor markets in Table C.1.6 and Figure C.1.1a, and between industries and regions in Figures C.1.2 and C.1.3. On average, concentration across Italian labor markets is mild: the median value is much lower than the threshold defined by the US antitrust agency, 0.15, and only a few markets are concentrated. However, the average is 0.14, indicating a mild concentration. Figure C.1.1a 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 HHI distribution on the worker-level panel. Substantially, I weight the HHI by its size, the number of spells, thus removing the potential bias induced by those markets with fewer spells and therefore mechanically higher HHI. Results

[5] *e.g.*, Bertola and Garibaldi 2001.

[6] With only one spell the index, for a mechanical bias of the HHI formula in Equation 1, is equal to 1.

are displayed in Figure C.1.1b and indicate that, when each market is weighted by the number of spells, the levels of concentration sharply decrease and so do the spikes. This confirms that the spikes are not a concern, and that on average concentration levels in Italy are low. Martins and Melo 2024 find that approximately 9% of Portuguese workers are subjected to non-low concentration levels. Moreover, since they rely on a stock-based index, he probably underestimates the true level of concentration across LMs. In the Italian case, according to my estimates, the percentage is 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 occur in markets with an HHI higher than 0.15. They represent approximately 3%; a low share that should not be of concern.

When computing concentration between regions, industries, and job titles separately, concentration increases (Table C.1.6 and Figure C.1.2). Although the distributions tend to shift toward normality, concentration largely differs across industries. My findings are consistent with those of Fanfani 2022, who finds that industry heterogeneity in monopsonistic dynamics explains a relevant portion of the gender wage gap. Concentration could vary over time, peaking during periods of recession, and thus exacerbating the damages of financial shocks on workers. Financial turmoil can amplify labor market volatility (Boeri et al. 2013; Autor et al. 2016) and reduce the access of firms to credit markets, which in turn reduces employment (Berton et al. 2018). However, Figures C.1.3 and C.1.4 suggest that concentration does not change over time even during the peak of the financial crisis.

3 Empirical Strategy

3.1 Wages

I compute daily wages by dividing the total gross remuneration of each employment contract by the number of days worked, thus ruling out the presence of any measurement error. The number of records with a value of 0 for wages is less than 50,000 and, since they likely represent a measurement error, they are dropped. To identify the correlation between concentration and entrants' wages, I estimate the following equation:

$$log(Y_{i,m,t}) = \delta_i + \mu_m + \gamma_s + \Gamma_{r,t} + \Lambda_{d,t} + \Phi_{o,t} + \beta_t + \theta log(HHI_{m,t}) + \Gamma Z_{i,t} + v_{i,m,t} \tag{3}$$

where i indexes workers, r regions, o job titles, j firms, d class sizes, s industries, and t years. $Y_{i,m,t}$ is the gross daily remuneration of worker i in market m and year t. Z contains worker-level covariates as a quadratic polynomial for age and spell length to proxy individual-specific working experience and on-the-job working experience. Standard errors are clustered at the market-year level, the level at which concentration varies.

The model is a log-log and hence θ should be interpreted as the elasticity of daily wages to concentration. Equation 3 is estimated with OLS. I exploit both cross-sectional and time variation in concentration and 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, to control for all possible confounding factors. I also control for job title-year, region-year, and size-year fixed effects to take into account potential time-varying confounding effects that jointly influence concentration and wages.

This empirical strategy requires that individuals move between employers and labor markets over time. On average in my sample, approximately 60% of the workers change at least one job and approximately the same also share an employer[7]. As the industry depends on the firm where the worker is employed, it also means that the industry changes at least once for more than 50% of the population. Approximately half of the workers also switch at least one size class, suggesting that size-by-year fixed effects can explain a lot of wage heterogeneity. Individuals do not frequently change job titles; 20% did so between 2005 and 2018. However,

[7] I define these workers "switchers" and I provide more evidence on whether they differ from "non-switchers" in Tables C.1.4 and C.1.5.

I control for job title and region-by-year fixed effects to capture regional and occupational-specific trends in wages and concentration.

3.2 Employment

I measure employment as the number of labor contracts signed in a market during a year (I. Marinescu et al. 2021), which I denote as $F_{m,t}$, and estimate the following equation:

$$log(F_{m,t}) = \delta_m + \Phi_s + \gamma_{o,t} + \Theta_{r,t} + \beta_t + \theta log(HHI_{m,t}) + \phi X_{m,t} + v_{m,t} \tag{4}$$

where m indexes markets, δ and β represent market and year fixed effects and γ, Φ, and Θ are job title-year, industry, and region-year fixed effects. Standard errors are 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 markets. Since it is a log-log specification, θ should be interpreted as the elasticity of employment to labor market concentration. X contains the average age and share of men for each market m in year t to account for feasible differences in composition over time, while I rely on a full set of job title-year, industry, and region-year fixed effects to control for value added and employment specific to each market and year.

Estimates again suffer from endogeneity: in 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 toward zero estimates. To address all of these threats, I need to identify a shock that affects 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 to the mechanism inducing a positive correlation between concentration and wages.

3.3 Identification Strategy

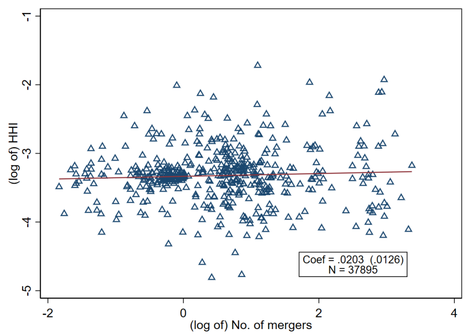
To obtain an exogenous variation in concentration, I rely on an instrumental variable strategy exploiting horizontal mergers. The antitrust literature has focused on mergers, while a recent stream has begun investigating the relationship with labor market. Several studies find that mergers increase product market concentration (Saidi and Streitz 2021; Affeldt et al. 2021; Benkard et al. 2021). However, growing evidence in the United States and Europe suggests that mergers also affect labor market outcomes (Posner and I. E. Marinescu 2020; I. Marinescu and Hovenkamp 2019; Suresh et al. 2018). Berger et al. 2023 find that mergers decrease employment and wages with greater effects in concentrated markets. Reduced form and structural estimates (Shapiro 2019; Arnold 2021; Guanziroli 2022; Jarosch et al. 2024) and simulations (I. Marinescu et al. 2021) show that horizontal mergers increase labor market concentration. Section D.2 describes the literature extensively.

Data I exploit the *Zephyr* database provided by the *Bureau Van Dijk* archive, which contains times series of worldwide rumored, announced, and completed mergers and acquisitions operations of all types (partial or full acquisitions, mergers, etc.) from 1997 to today. I select all completed mergers and acquisitions whose target country is Italy from 2005 to 2018. For a subsample of these events, I also have information on the number of workers involved and vendor and acquirer sizes. The final sample contains 5,932 events, associated with 4,237 different acquiring firms and approximately the same number of vendors[8]. On average, approximately 423 events happen per year. I provide further details in Section D.3.

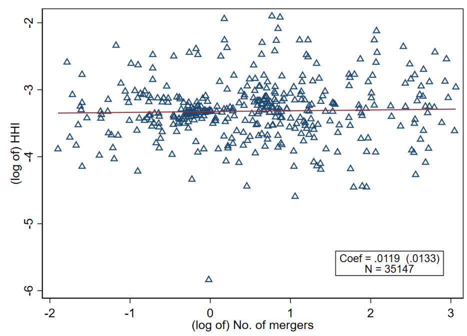
I select only *horizontal mergers*, in other words, those operations between firms belonging to the same industry. Since markets are defined across industries, mergers between firms across different industries do not raise concentration and thus are discarded. The final number of events from 2005 to 2018 decreases to 184. *Zephyr* provides many details for each record, such

[8] The Italian labor market mergers exposure is weak compared to France and Germany, in which, approximately, the same number of operations occurred in 2014-18 (*Oxford Economics*).

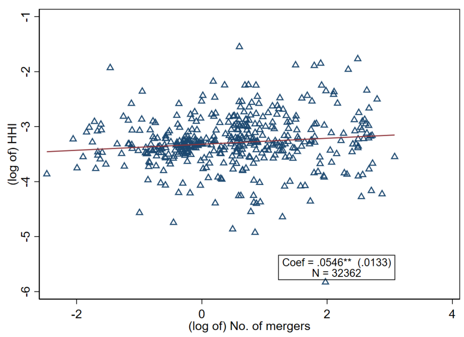
as the targeted industries (up to 6 digits), the number of employees involved[9], or whether only certain plants and therefore locations are involved, and the names of the firms. Ideally, one would match these firms with other archives through their names, which can be used to recover the fiscal code with virtually no errors. However, the same information is absent in *LoSaI*, which instead provides a 2-digit industry and no fiscal code. Therefore, I can only match mergers with workers' records through the industry associated with each firm and the year, exploiting a national-industry-level shock in mergers.



(a) Current.



(b) 1-year lagged.



(c) 2-years lagged.

Figure 3.1: Binscatters between concentration and mergers.

Notes: The red lines represent OLS fits of log(HHI) on IHS(mergers); the coefficient is the slope. Market fixed effects are included. Standard errors are clustered at the market level. The sample consists of 48,219 market-year tuples, 4,874 markets, and 184 mergers in 2005-18.

The underlying idea of the identification strategy is that markets that experience mergers become more concentrated over time. Concentration varies through different channels: An industry-level shock could raise market concentration, as the shock would translate to differ-

[9] Unfortunately, due to missing data, I cannot exploit this variable.

ent extents to all markets associated with that industry. Therefore, I exploit a *national-industry shock* in concentration. There is evidence that industry heterogeneity is a driver of monopsonistic dynamics in Italy (Fanfani 2022). The most targeted industries are *Financial Activities, Information and IT Services Activities, Editorial Activities, Electric and Gas Furniture, Manufacture of Machinery and Equipment*, and *Satellite Telecommunication* (Figure D.3.1). Figure 3.1 suggests that the more mergers occur within industries, the more these become concentrated over time.

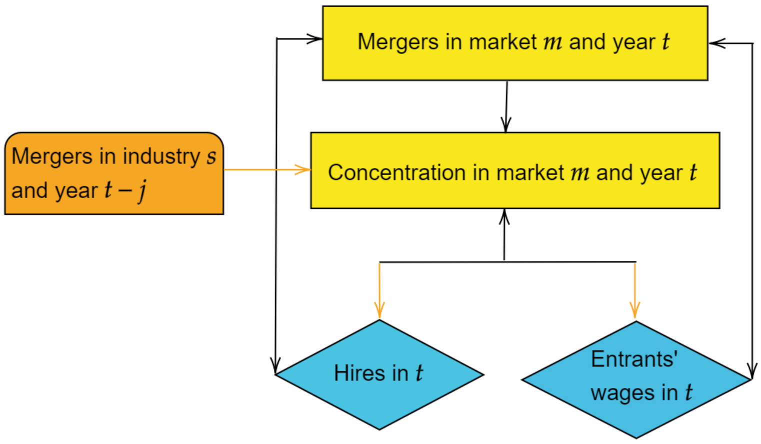


Figure 3.2: Sketch of the Identification Strategy.

Notes: The yellow boxes indicate the endogenous variables, the orange boxes are exogenous, and the blue boxes indicate the outcomes. The black and orange arrows indicate correlations and causal effects.

To the extent that I control for all confounders that jointly influence concentration and mergers, mergers would be a shock in concentration targeting only a subsample of markets and workers[10], creating a quasi-experimental framework in which mergers are the random treatment affecting concentration. Figure 3.2 sketches the intuition behind the identification strategy. 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 role of firm-specific characteristics and market-specific confounders. Instead, an industry-national shock affects concentration but does not have a direct effect on the outcomes as income and employment

[10]Consider, for instance, a merger between two competitors at the national level, whose plants are located in one region of Italy. It does not reasonably directly influence workers employed by competitors whose plants are located in different regions.

depend on market-specific factors.

I rely on lagged mergers to (i) ensure exogeneity to market-specific dynamics simultaneously influencing mergers and outcomes; (ii) incorporate the fact that merged firms need some time to consolidate and display their labor market power. Figure 3.1 suggests that this is the case, since only the two-year lagged mergers are correlated with concentration[11]. I define two binary (Wald) instruments as:

$$\text{IV}^1_{m,t}\text{: } \forall \text{ t in [2005,2018], } \text{I}_{m,t}\{Mergers_{m(s),t-1} > 0\} = 1; \tag{5}$$

$$\text{IV}^2_{m,t}\text{: } \forall \text{ t in [2005,2018], } \text{I}_{m,t}\{Mergers_{m(s),t-2} > 0\} = 1 \tag{6}$$

where I is a dummy variable equal to 1 if industry s of market $m(s)$ experiences at least one merger in year t-1 or t-2, 0 otherwise. In other words, I instrument concentration for each labor market and year with a dummy variable indicating whether a market has experiences at least a merger in the previous one or two years. A Wald estimate takes this form:

$$\theta_{IV^k} = \frac{\text{cov}\left(Y_{i,m,t}, IV^k_{m,t}\right)}{\text{cov}\left(HHI_{i,m}, IV^k_{m,t}\right)} = \frac{E\left[Y_{i,m,t} \mid IV^k_{m,t} = 1\right] - E\left[Y_{i,m,t} \mid IV^k_{m,t} = 0\right]}{E\left[HHI_{i,m} \mid IV^k_{m,t} = 1\right] - E\left[HHI_{i,m} \mid IV^k_{m,t} = 0\right]} \tag{7}$$

which, substituting the expected values with their corresponding averages in the sample, becomes $\widehat{\theta}_{IV^k}$ *i.e.*, the difference in the average Y between workers belonging to markets that experienced in the past one or two years at least one merger[12] divided by the difference in HHI between treated and untreated markets as predicted by the instrument k. Estimates are Average Treatment Effects on the Treated (ATT) as long as the instruments do not directly affect the outcomes (exogeneity) and are correlated with the endogenous covariate (relevance). I discuss these assumptions in Section D.4, and I perform a battery of robustness checks in

[11]The estimated elasticity is .055 percentage point, which, following an SD increase in the measure of mergers, would yield an increase in HHI of approximately 50%.

[12]The underlying assumption is that the instruments do not directly affect the outcomes, although they affect concentration and in turn the outcomes.

Section D.5.

4 Results

Wages The results are shown in Table 1. I display results for the three different specifications: in Panel (a) I use the instrument defined in Equation 5, in (b) the instrument defined in Equation 4, while in (c) I use both. The first panel contains the endogenous estimates. Standard errors are clustered at the market-year level, the level at which concentration varies. Estimates indicate that concentration has a sizeable negative impact on entrants' wages. TSLS estimates are larger in magnitude than OLS ones, which are close to zero and not significant, because of different labor market level confounders, positively correlated with both concentration and wages, inducing a downward bias. The opposite mechanism is the main threat: the higher concentration, the more there are large, more productive, high-wage firms. This also suggests that the instruments capture exogenous HHI variations.

Magnitude and significance differ across specifications and the Equation 5 instrument is the most significant. Estimates range between -0.14 and -0.068 p.p., while preferred ranges 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 are larger than those of the literature; I. Marinescu et al. 2021 reduced form elasticities range between -0.067 and -0.052 points, indicating a reduction in wages following a 10% increase in market HHI of 0.67 and 0.52 p.p., and in general are in line with Card 2022.

Dependent variable: log(Daily Wages)	(1)	(2)	(3)	(4)
OLS				
$\widehat{\theta}$	0.00209**	-0.00152**	0.00115	-0.0014*
	(.00075)	(.00068)	(.0011)	(.00081)
TSLS				
Panel (a): $\widehat{\theta}_{IV^1}$	-0.319**	-0.114**	-0.1258**	-0.134***
	(.1354)	(.0471)	(.04514)	(.03803)
Panel (b): $\widehat{\theta}_{IV^2}$	-0.282**	-0.0525	-0.0684*	-0.209
	(.1189)	(.04419)	(.0404)	(.1754)
Panel (c): $\widehat{\theta}_{IV^{1:2}}$	-0.300**	-0.0920**	-0.1052**	-0.1393***
	(.0890)	(.0326)	(.0315)	(.0375)
Observations	2,928,818	2,928,818	2,928,474	2,928,474
quadratic spell length & age	✓	✓	✓	✓
part time dummies	✓	✓	✓	✓
worker FE	✓	✓	✓	✓
year FE	✓	✓	✓	✓
industry FE	-	✓	✓	✓
region FE	-	✓	✓	-
job title FE	-	✓	✓	-
size FE	-	✓	✓	-
market FE	-	-	✓	✓
job title-year FE	-	-	-	✓
size-year FE	-	-	-	✓
region-year FE	-	-	-	✓

Table 1: OLS and TSLS estimates of Equation 3.

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the market-year level in parentheses. Observations are 3,573,677 working spells between 2005 and 2018. $\widehat{\theta}_{\text{IV}}$ is formally shown in Equation 7. Panels indicate different instruments: (a) 2-year lagged mergers as in Equation 6; (b) 1-year lagged mergers as in Equation 5; and (c) both jointly. Observations are lower than in the full sample and differ across columns because of singletons when including worker and market fixed effects.

Estimates are in line with those obtained in I. Marinescu et al. 2021 by simulating a horizontal merger between two top-employing firms, as they find a reduction in a new-firm wage bill of approximately 7 p.p. following a 10-point HHI increase. 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[13]. Jarosch et al. 2024 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[14], they

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

[14]From the 25^{th} to the 75^{th} percentile in the HHI distribution.

find that wages decrease by 1 p.p. These values are higher than those estimated on average in the literature and more similar to mine. In summary, my estimates range between those obtained with leave-one-out IVs (I. Marinescu et al. 2021; Azkarate-Askasua and Zerecero 2023; Dodini et al. 2023; Bassanini et al. 2023; Bassanini et al. 2024) and those obtained relying on mergers (Arnold 2021; Guanziroli 2022; Jarosch et al. 2024; Berger et al. 2023; I. Marinescu et al. 2021), and are close to those of Luccioletti 2022[15].

Dependent variable: log(Hires)	(1)	(2)	(3)
OLS			
$\widehat{\theta}$	-0.1166***	-0.1167***	-0.0948***
	(.00445)	(.00446)	(.00332)
TSLS			
Panel (a): $\widehat{\theta}_{IV^1}$	-0.681**	-0.681**	-0.692**
	(.2819)	(.2821)	(.2867)
Psnel (b): $\widehat{\theta}_{IV^2}$	-0.771*	-0.771*	-0.747*
	(.4689)	(0.4694)	(.4402)
Panel (c) $\widehat{\theta}_{IV^{1;2}}$	-0.699**	-0.699**	-0.704**
	(.2791)	(.2794)	(.2792)
Observations	47,180	47,180	47,180
market avg. men share & age	✓	✓	✓
market FE	✓	✓	✓
year FE	✓	✓	✓
job title FE	-	✓	-
region FE	-	✓	-
industry FE	-	✓	✓
region-year FE	-	-	✓
job title-year FE	-	-	✓

Table 2: OLS and TSLS estimates of Equation 4.

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the market level in parentheses. Employment is measured as the number of newly activated working spells within each market and year. Full sample is made up of 47,727 market-year tuples. Columns indicate different sets of fixed effects. $\widehat{\theta}_{\text{IV}}$ is formally displayed in Equation 7. Panels indicate different instruments: (a) 2-year lagged mergers as in Equation 6; (b) 1-year lagged mergers as in Equation 5; and (c) both jointly.

Employment Results are shown in Table 2: the first panel contains the TSLS estimates, while the following contain the OLS ones with the three sets of instruments. TSLS estimates are larger in magnitude than OLS ones, as the different confounders, positively correlated with both

[15]He uses an alternative instrument based on the changes in the local size of the public sector in Spain, estimating elasticities between -0.14 and -0.07 p.p.

concentration and hires, induce a downward bias. TSLS estimates are stable across panels, with elasticities ranging between -0.68 and -0.77 p.p.. These elasticities are slightly greater than those estimated by I. Marinescu et al. 2021, Arnold 2021, and Luccioletti 2022, which range between -0.31 and -0.585 p.p., -0.9 and -1.4 p.p., and -1.7 and -1.5 p.p. respectively. Results indicate that a 10-point increase in HHI would reduce hires by approximately 3-6 p.p. The difference in magnitude might be due to the different framework, identification strategy, or definition of new hires[16]. The more conservative definition of new hires in my framework could explain the higher magnitude of my estimates. Results indicate that when markets move from low to high concentration, hires decline by 7-8 p.p..

4.1 Job Security

One of the contributions of this paper is to try to fill a gap in the literature addressing several measures of job security and shed light on all channels through which firms exert their power over workers. Indeed, Bassanini et al. 2024 find that higher concentration reduces the likelihood of being hired with a permanent contract; Bachmann et al. 2022 find that workers performing non-routinary intellectual tasks are exposed to higher degrees of monopsony than workers performing manual tasks, and Amodio et al. 2022 find that firms leverage self-employment to overcome workers in monopsonistic frameworks. Furthermore, Italy is the perfect environment to investigate job security, as several reforms have recently been implemented to remove restrictions on temporary employment[17].

These reforms created a dual labor market in which firms could exert their power over workers through job security. They might, for instance, hire temporarily to secure the possibility of dismissals with little cost after a screening period. To avoid looking at the trajectories of workers across different contracts would result in overlooking potential mechanisms through

[16] They define new hires as workers whose spell starts in each quarter, deleting those whose spells start on each January 1^{st}.

[17] More details in Section B.

which monopsony power displays itself. Therefore, I study (1) the likelihood of being hired with an open-ended contract and (2) being renewed within the same firm in two consecutive years with temporary contracts. Renewals are defined as the spells of workers who have two consecutive spells for two years within the same firm, and the first one is temporary. I obtain approximately 300,000 records. The specification is shown in Equation 11.

Permanent Hires Panel D.6.1b indicates null effects on outcome (1) as in Bassanini et al. 2024. I estimate a semi-elasticity of 0.004 (SE=0.0315; p-value=0.888), with standard errors clustered at the market level. Estimates remain not statistically significant with errors clustered at the market-year level (SE=0.022; p-value=0.842). The results indicate that concentration does not affect the probability of being hired on a permanent contract.

Renewals The results of outcome (2) are shown in Table D.6.1. The estimates are statistically significant and positive, indicating that where concentration is higher, the probability of being renewed temporarily is substantially higher. The semi-elasticities are approximately 0.48 points, which means roughly a 60 p.p. increase. Renewals' wages, although not precisely estimated, are negatively affected by concentration as well, with a magnitude lower than that of new hires and closer to those of incumbents ($\approx$ -0.08 p.p.). I interpret this as a sign that, in monopsonistic frameworks, employers reduce new hires and their job security while relying on renewals to extend the screening period and secure future change in dismissals. Meanwhile, they lower wages of both new hires and renewals.

4.2 Heterogeneity

Sex Dodini et al. 2023 and Manning 2021 find that monopsony explains gender wage gap in the UK and Norway, while Sulis 2011 and Fanfani 2022 find similar evidence in Italy. I explore whether and to what extent merger-induced shocks in concentration hurt wages and the job security of men and women differently. The estimates are shown in Figure D.6.3. Surprisingly, wage estimates are highly significant for women and only marginally men. Men's coefficient becomes marginally significant at 90% confidence level. Although not precisely estimated, men's coefficient drives the magnitude of the baseline estimate upward, as it is approximately ten times that of women (-0.27 vs -0.03 p.p.). There is no effect on the job security of men, whereas it is statistically significant at the 90% confidence level and equals -0.03 points for women. A 10 p.p. increase in HHI reduces women's job security by 0.3 percentage points. Overall, men's job security is not affected by monopsony, while their wages are.

Concentration Levels I split concentration into two brackets: from 0 to 0.15 in HHI, and above it, indicating, respectively, a low versus a not-low concentration level according to the US antitrust agency. Arnold 2021 and Berger et al. 2023 find that mergers have greater detrimental effects on wages and employment in more concentrated markets[18]. The same might apply to job security. Results, additionally split by gender, are shown in Figure D.6.3. From low to high concentrated markets, the elasticity of wages to HHI more than doubles (-0.13 vs -0.3 p.p). For men, although still not precisely estimated, it reaches -0.5 p.p. in highly concentrated markets, while for women, it is precisely estimated and equals -0.1 p.p.

The results on sex and concentration levels shed light on the mechanisms contributing to the gender wage gap in the Italian labor market, as the estimates indicate a striking difference in how monopsony power affects men and women. Furthermore, they also say that the baseline estimates hide a relevant heterogeneity in magnitude and significance and that both concentration levels and shocks are relevant. There is no effect on job security even in highly concentrated markets for men, while the effect is statistically significant and increases in concentration levels for women (-0.03 vs. -0.06 points). In short, concentration (1) damages job security for women only; (2) matters in levels and not only in shocks.

[18] Moving from low to high concentrated markets, Arnold 2021's elasticity ranges between -0.31 and -0.08 percentage points, while Berger et al. 2023's between -0.44 and -0.11.

5 Conclusions

This paper investigates concentration and its effects on entrants' wages, job security, and employment by exploiting horizontal mergers as a shock across Italian labor markets. I find that very few new hires occur in markets where concentration is high. I find that mergers raise concentration, which reduces wages and employment. If an average concentrated market becomes 10 points more concentrated, wages and hires decrease by 0.9-1.4 and by 0.7–0.8 percentage points in the following two years, implying a substantial loss of 9-19 euros per month for a full-time worker with an average wage.

Workers who experience mergers are less likely to be hired with a permanent contract but more likely to undergo consecutive screening periods, indicating that firms combine the tools at their disposal to exert power over workers. In addition, I find that (i) monopsonistic power affects men and women differently, with the former affected only and largely through wages, and the latter, although less so, also through job security; and (ii) concentration levels and shocks must be jointly evaluated to identify the riskiest labor markets.

Future research should further explore and open the black box by looking at job content and tasks to obtain a full picture of monopsonistic power. Responsible authorities must therefore be attentive to the labor market spillovers of mergers, in addition to the well-known product market ones, and evaluate mergers on an industry-specific basis, taking into account concentration levels and all outcomes. I believe that stronger enforcement of antitrust laws, data-driven, and in specific industries, may be necessary in Italy.

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Appendix

A Conceptual Framework

Labor market concentration is a proxy of monopsonistic dynamics (Card 2022). 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 et al. 2019 predict that both employment and wages should fall when labor market concentration rises. 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.

The point is to motivate why an index of labor market concentration can efficiently proxy the degree of power of employers across labor markets, as I define them. Amodio et al. 2022 derive an oligopsony model in which, when concentration increases, wages decrease. They also show that 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, migration dynamics (Amior and Stuhler 2024), and increases in non-compete and no-poaching agreements (Boeri et al. 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 2021 highlights the importance of the source of the concentration variation. 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. Assuming that firms compete à *la Cournot*, one can derive the following equation[19]:

$$w_m = \underbrace{\left(\frac{\eta_m}{HHI + \eta_m}\right)}_{\text{worker fraction } = \gamma_m} \underbrace{\theta_m}_{\text{AMRPL}} \tag{8}$$

[19] Arnold 2021, Section 2.3, page 7.

, in which w_m is the average market wage, η_m is the elasticity of labor supply in market m, HHI is an employment-based concentration index, 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[20]. The empirical challenge and the contribution is to isolate a plausible exogenous variation in HHI to identify the effect of employers' power on wages. In Section 3.3, I elaborate on how I use horizontal mergers to achieve this.

B Institutional Background

This section provides a brief overview of the Italian labor market, highlighting the collective bargaining system, minimum wages, and the deregulation process started in '90s, to contextualize and motivate my analysis. Collective bargaining in Italy is structured on two levels. The first level, known as *Contratti Collettivi Nazionali di Lavoro* (*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 representatives. 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 better terms. Consequently, the labor market in Italy is fragmented, making it difficult to map all the different contracts (Fanfani et al. 2021; Fanfani 2023).

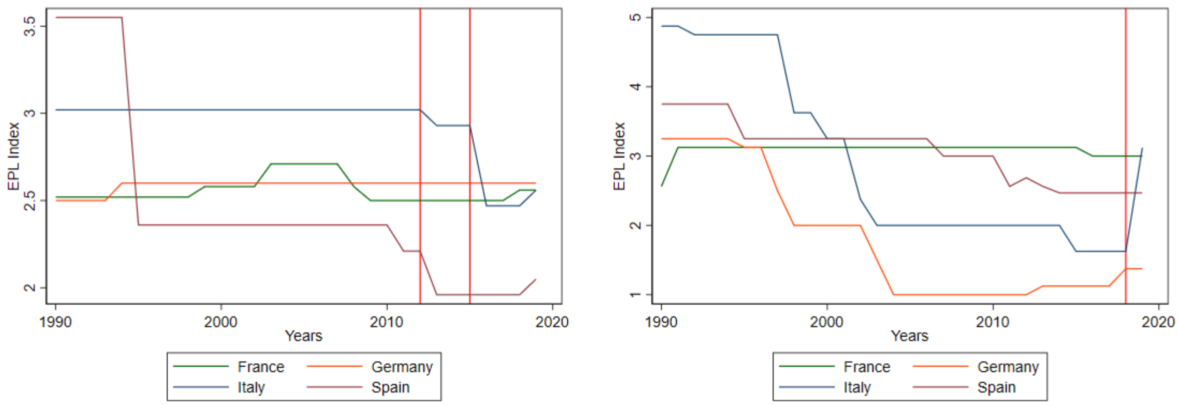
Garnero 2018 maps 860 industry-wide collective agreements that cover practically all private sector employees in Italy. Trade union density, which is defined as the number of members over the total number of employees, is below 30% in the private sector, and employers' orga-

[20] An analogous prediction is that of Luccioletti 2022, in which, as HHI and wages are directly related in the FOC, labor market power affects wages only through variation in labor market concentration.

nizations' 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 to regional median wages, particularly 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 southern Italy.

Furthermore, several labor market reforms have been promulgated in recent decades reducing Employment Protection Legislation (EPL) on open-ended contracts to favor employment. Figure B.0.1 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*[21] essentially prohibited firms from dismissing workers covered by an open-ended contract for economic reasons. The *Jobs Act* introduced this opportunity. The reforms increased the likelihood of economic dismissals and decreased costs, both monetarily and in terms of probability of reinstatement (Ardito et al. 2022). 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 and differential impacts of minimum wages across labor markets.

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



(a) Open-Ended contracts.

(b) Temporary contracts.

Figure B.0.1: Strictness of EPL in Europe in the last decades.

Notes: The vertical lines represent the main reforms promulgated recently: in Panel (a), the *Fornero Law* in 2012 and the *Jobs Act* in 2015, while in (b) the *Dignity Decree* in 2018. *Source:* OECD.

Another potential of monopsonistic power is the presence of legal provisions limiting workers mobility, such as non-compete agreements (Boeri et al. 2023), defined as contracts in which an employee agrees not to compete with her employer after the employment spell ends. They find that about 16% of Italian 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 common among employees with manual and elementary job titles 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 labor markets because there are already fewer competitors. In such a scenario, firms, especially the largest, could exert their market power over workers.

C Additional Statistics

C.1 LoSaI

LoSaI is made of several datasets extracted from the *INPS* archive. The first provides a random set of individuals working spells with the 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 individuals are randomly sampled as those 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 data set, I obtained information from firms on class size and industry (two-digit NACE Rev.2) ranging from 1990 to 2018. Class sizes are, right element included: 0-5; 6-10, 11-15, 16-20, 21-25, 26-30, 31-40, 41-50, 51-100, 101-200, 201-300, 301-400, 401-500, and above 500. Firms can be linked across datasets through a unique code. By merging these datasets, I obtain an employer-employee dataset. The sample is not obtained based on stratified randomization by size class, region, and industry, thus, firms' population is not necessarily representative of the Italian one. Table C.1.1 reassures on this concern.

size bin LoSal	% plants 2018	% plants (2005-2018)	% firm (2005-2018)	size bin ASIA	% 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 C.1.1: Summary statistics for LoSaI and ASIA firms population, by size class.

Variable	Obs.	Mean	St. Dev	Min	P1	Median	P99	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 C.1.2: Summary statistics of new hires.

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

#spells	Freq.	Percent	Cum.
1	1,484,293	41.53	41.53
2	877,333	24.55	66.08
3	522,665	14.63	80.71
4	307,283	8.60	89.31
5	176,928	4.95	94.26
6	99,321	2.78	97.04
7	53,722	1.50	98.54
8	27,861	0.78	99.32
9	13,784	0.39	99.71
10	6,427	0.18	99.89
11	2,698	0.08	99.96
12	1,003	0.03	99.99
13	294	0.01	100.00
14	65	0.00	100.00
Total	3,573,677	100.00	

Table C.1.3: Switchers between 2005 and 2018.

	Non-switchers		Switchers		
	Mean	Std. dev.	Mean	Std. dev	Std. Diff
Sex	.5726	.49471	.615	.4866	-0.08644
Age (years)	36.26	12.62	35.41	10.873	0.07247
Daily wages	62.99	51.216	60.69	40.026	0.04999
Log(Daily wages)	3.947	.64909	3.964	.56765	-0.02760

Table C.1.4: Standardized differences between switchers and non-switchers.

Notes: Switchers are defined as those workers who are associated to two or more spells in the period of interest, while non-switchers are those workers who appear only once in our matched employer-employee database. 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. On the rows, there are covariates of interest, namely sex, age, and daily wages, in log and levels.

	Class size	NACE (2-digit) code
Switchers	0.0078^{***}	0.0139^{***}
	(0.0000)	(0.0000)

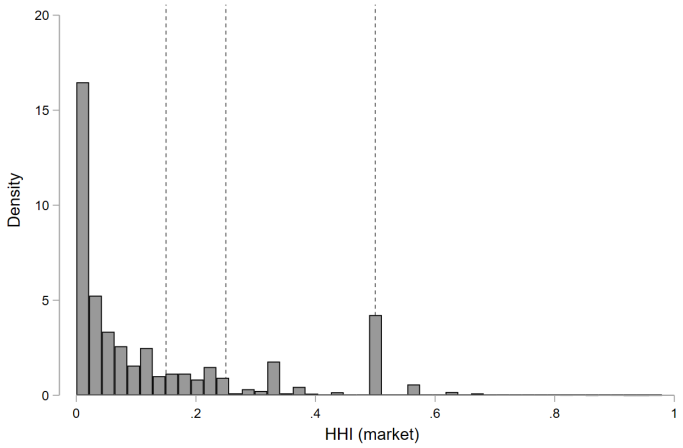
Table C.1.5: Correlations between switchers and firms' characteristics.

Notes: The p-values for the significance of the correlations at 99% are in parentheses. Switchers is a dummy variable, equal to 1 for workers that have at least 2 spells in the period of analysis, 0 otherwise. Size classes are 14 discrete brackets, defined extensively in the Appendix of the paper. The highest NACE 2-digit codes are associated to services and retail industries, while the lowest to manufacturing sectors.

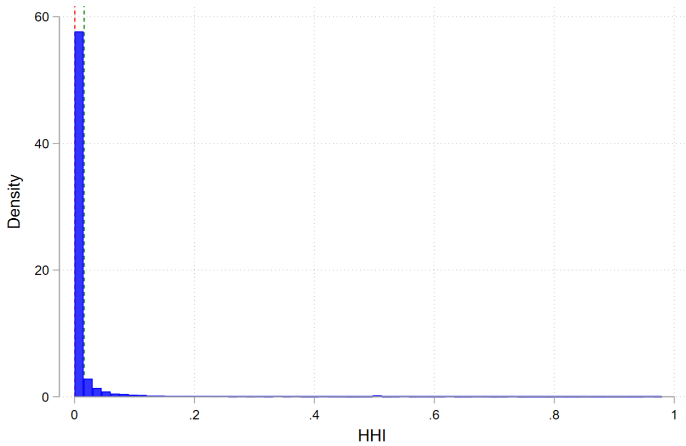
Index	**Observations**	**Mean**	**St. Dev**	**Min**	$1^{st} Perc.$	**Median**	$99^{th} Perc.$	**Max**
HHI_m	47,727	0.136	0.174	0.000	0.000	0.054	0.625	0.979
HHI_s	1,064	0.155	0.087	0.006	0.027	0.141	0.424	0.642
HHI_r	280	0.148	0.040	0.088	0.094	0.138	0.269	0.291
HHI_o	84	0.211	0.094	0.091	0.091	0.206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

Table C.1.6: Summary statistics of HHI across markets (m), industries (s), regions (r) and job titles (o).

Notes: Labor markets are 5,008. The indexes are calculated across markets according to Equation 1 and then taken the averages within respectively job titles, industries, and regions. Markets-year tuples are 47,727, industry-year tuples are 1,064, region-year tuples are 280 and job title-year tuples are 84 in 2005-2018.



(a) Across markets.



(b) *, weighted by market hires.

Figure C.1.1: HHI distribution.

Notes: Panel (a) plots the HHI distribution across markets and years, while (b) plots HHI across markets and years, weighted by markets spells. Obs. are 47,727 market-year tuples associated with 5,008 labor markets in Italy in (a), while are 3,573,677 spells in (b). A market is defined as an interaction between a region, a job title, and an industry. The dotted lines are the thresholds for, respectively, low, medium, high, and high levels of concentration. HHI is calculated in Equation 8.

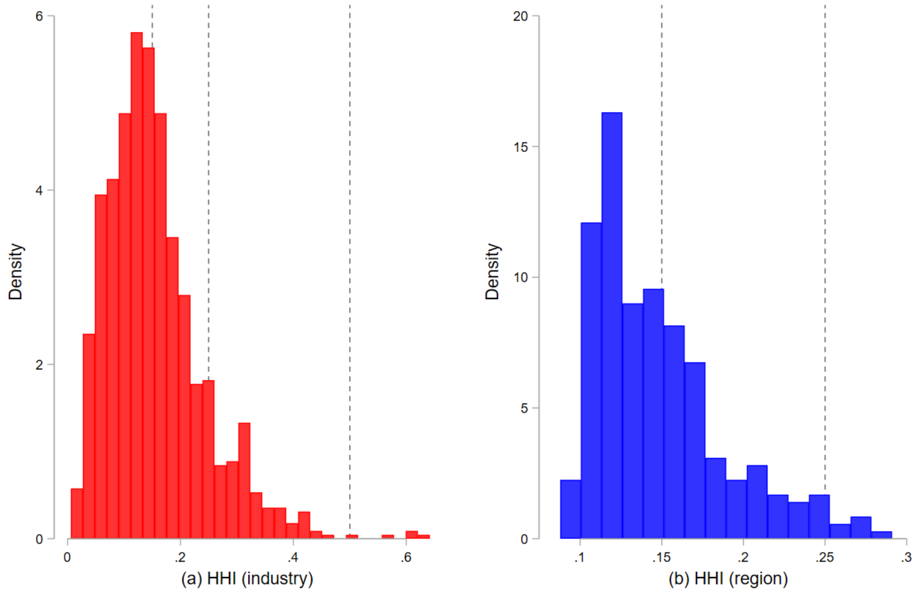


Figure C.1.2: HHI distribution across industries and regions.

Notes: The dotted lines represent the standard thresholds for defining, respectively, low, medium, high, and very high concentration. Industries and regions HHI are calculated as averages of market HHI within regions and industries. Market HHI is calculated in Equation 8. Observations are respectively 1,064 industry-year and 280 region-year tuples between 2005 and 2018.

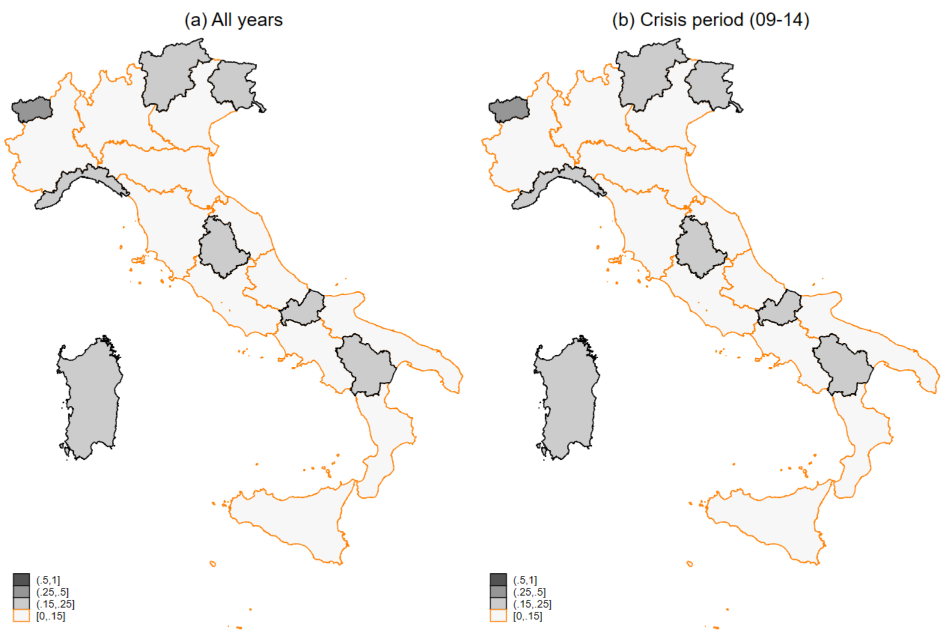


Figure C.1.3: HHI across Italian regions.

Notes: The figure displays concentration across Italian regions. 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 HHI within each region and across all years in Panel (a) and for 2009-14 in Panel (b). Observations are 280 region-year tuples in both panels.

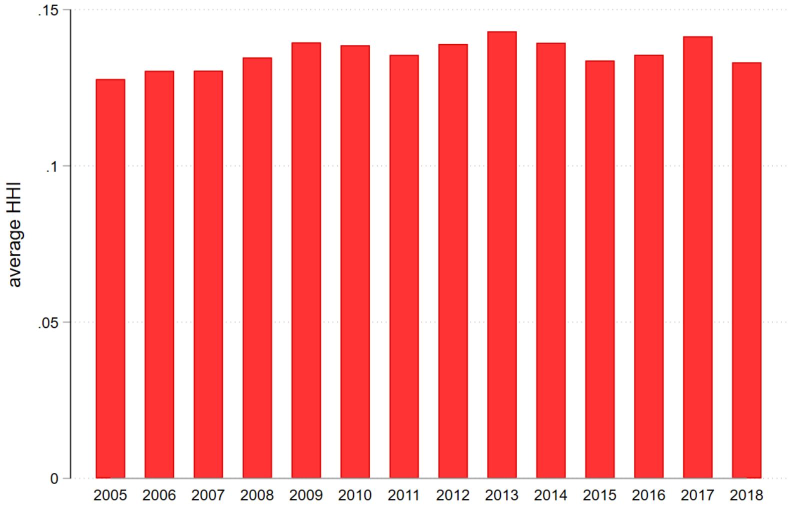


Figure C.1.4: Average HHI across labor markets, by year.

Notes: HHI is calculated in Equation 1. Obs. are 47,727 market-year tuples in 2005-18.

C.2 Index Limitations

Time Granularity I calculate the index within a year, which Azar et al. 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 number of workers in each cell. I believe that the latter bias would have a greater impact than the former. It is worth noting that this fact applies to the US labor market, where mobility is significantly higher than in the Italian labor market, and in which the bias is less pronounced.

Commuters The data that do not allow me to observe the actual place of work, as there is no such information in *LoSaI*. Furthermore, there is no way to link this data set with another, such as *AIDA*, which provides the municipality where the firm is located, as the fiscal code is anonymized. It is also true that in Italy, in particular, people tend to move from the south to the north for work purposes. However, I believe that, although the concern is real, it is less relevant than might be feared. The reason I think this is mainly that I rely on new hires, so I mainly select workers who appear in the dataset a few times, mostly one or two (Table C.1.3). 90% of the workers appear at most four times in the sample. This means that most of the workers appear in the longitudinal sample for a short period; they work right after they enter the sample and then exit. Therefore, the information on the region of residence is likely to be updated. Furthermore, due to fiscal rules, individuals are incentivized to move their residence where they live and work shortly afterward. As the information is wide (*i.e,* the region) it is likely to coincide with the workplace.

Location The literature on labor market concentration typically uses more detailed measures, such as commuting zones, while I have used regions due to the inaccessibility of a more granular measure. Similarly, the industry is defined at a 2-digit NACE level, and job titles are classified into five categories, which may not be precise enough. To address these limitations, I have interacted the regions with industries and job titles, resulting in 5,008 markets after several cleaning procedures. However, our assumption is that firms hire within a region, which is the mechanism through which non-competitive dynamics arise and that I want to capture. All the aforementioned limitations can induce an upward bias in the concentration index, which could weaken the robustness of the following descriptive analysis. However, results, compared to those of Martins and Melo 2024, confirm 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 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.

Sampling Issue 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[22]. As a result, my index of concentration may be inflated. To address these concerns, I compute the standard firms' share HHI in Figure C.2.2, which is virtually identical. However, caution in interpreting the HHI levels is needed.

Bin Size Index The underlying assumption to my index is that hirings are uniformly distributed within size bins. However, consider the case of a market m with two size bins and two firms in each bin. Consider two points in time: from point 1 to point 2 hires within a class size move from one firm to another. A firm's share HHI will increase, whereas mine will not. This example illustrates a difference between the standard HHI based on firms' shares and mine based on class size shares. Indeed, the use of this index is motivated by the data, but also because this is a dynamic that I would like to capture[23]. This is indeed the mechanism I want to capture with my identification: two firms merge, the merged firm becomes larger, workers move towards a larger size class, and hires become more concentrated in the largest size classes. To test this, I perform two exercises.

First, I calculate for each market and year the share of hires in firms above 15 employees[24] over the total number of hires and regress the HHI on it in Figure C.2.1. The idea underlying a labor market concentration index is that the higher the number of firms in a given labor market, or, differently, the higher the number of smaller firms in a given labor market, the lower the index. Vice versa, the higher the share of hires concentrated in larger firms the higher the index. I find a statistically significant and positive correlation between the share of hires of the largest firms and concentration. This suggests that the size bin HHI captures the main concentration dynamics, namely that the more hires are concentrated among the largest firms, the higher the class size index. Second, I compute the standard index based on firms' share

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

[23] I explore that firms of similar sizes tend to pay similar wages and that wage heterogeneity in Italy is driven by the mobility of workers over time across different size classes.

[24] I chose 15 because the main labor market reforms promulgated in the last decades did the same. However, changing the threshold does not change the results.

and plot the correlation between the two indexes in Figure C.2.2. Results indicate that there is a strongly positive correlation, even controlling for a large set of fixed effects, indicating that the two indexes are very similar.

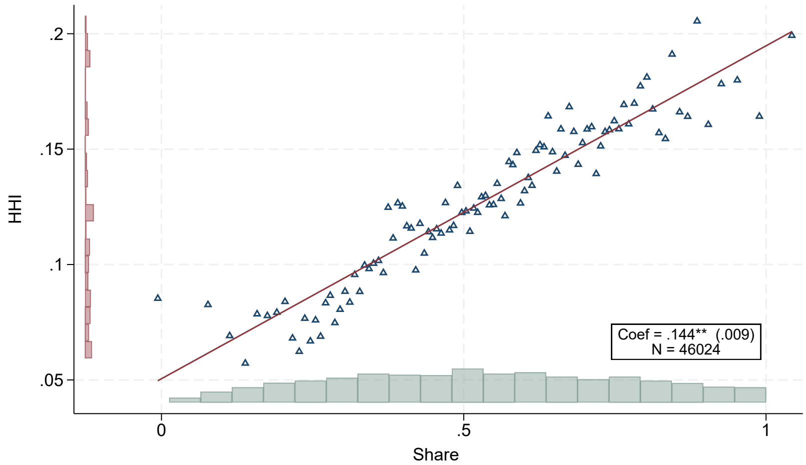


Figure C.2.1: Binscatter between concentration and share of hires in firms above 15-employee.

Notes: The graph plots the histograms of the share of hires of firms above 15 employees (x-axis) and the HHI (y-axis) and the linear fit with the slope of y on x. I plot 100 bins. Size is measured as the mode of firm size between 2005 and 2018. I control for the average share of women and average age by market-year, and cluster the standard errors at the market level. HHI is computed in Equation 1. An observation is a tuple market-year.

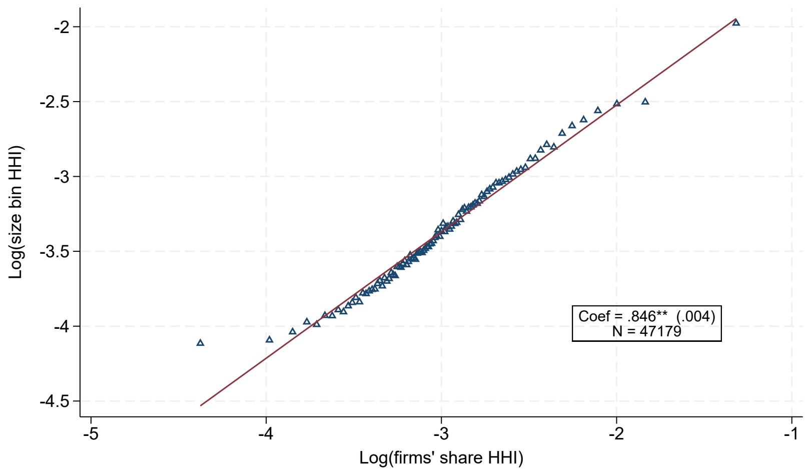


Figure C.2.2: Binscatter between size bin and firms' share HHI.

Notes: Both HHI are in log. I plot 100 bins. I control for market, year, region by year, occupation by year and region by year fixed effects and the average share of men and age by market-year cell. SE clustered at the market-level.

D Empirical Strategy

D.1 Potential Threats to the Identification

I control for year fixed effects to capture macro shocks, homogeneous across markets, at a national level and possibly influencing wages and firms' hiring dynamics[25]. Job title-year, size-year, and region-year fixed effects capture instead specific time-varying dynamics across regions, proxying local and job title specific employment dynamics, while bin size fixed effects capture different productivity trends across firms of different size. Industry-specific time trends, firm productivity, and market tightness are additional threats. Firm-specific characteristics might correlate with the outcomes, such as productivity, human capital, employers' attitude, and other factors that explain wage heterogeneity, would bias the estimates. I control for this by adding firm fixed effects (*e.g.,* I. Marinescu et al. 2021) in Tables D.5.1 and D.5.2.

Ideally, I exploit a more granular geographical level. Commuting zones are the preferred choice as they precisely take into account local employment dynamics, especially in a country characterized by a dense presence of the so-called "distretti industriali"[26]. However, I have no access to further information beyond the region. Anoter concern is raised by the absence of product market concentration: as it is positively correlated to labor market concentration omiting it would bias the estimates downward (I. Marinescu et al. 2021; Dodini et al. 2023; Bassanini et al. 2023). Unfortunately, I don't have access to firm-level information regarding prices and markups. Firm and HHI levels by year fixed effects mitigate this concern.

The latter issue is reverse causality, which is induced by time-varying market level shocks that simultaneously influence wages and concentration. The trigger is primarily based on market tightness, which is correlated to wages and concentration, as it depends simultaneously on hires and vacancies. There is no way to properly take this mechanism into account

[25] Such s workers' out-of-work benefits (set at a national level), macroeconomic fluctuation etc..

[26] 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.

in a reduced-form model, as the proper way is to set up a structural model that simultaneously realizes the covariate and the outcome. I address this threat relying on a TSLS strategy. There may be other confounding effects, such as industry-year shocks influencing simultaneously concentration and wages or trade shocks (*e.g.*, trade shock) targeting specific industries at specific points 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. I control for this in the robustness section.

Moreover, there is an additional relationship between wages and concentration: on one hand, everything else equal, higher wages attract more workers and therefore increase 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 OLS estimates are bounded to zero to the TSL ones. The specification in Equation 3 also suffers from reverse causality because of the mechanical relationship that assigns a higher concentration to markets with fewer spells. This bias is inevitable as long as the outcome is measured as a flow. Instead, the opposite holds for markets with more spells. I again expect the exogenous estimates to be larger in absolute terms because they are not constrained toward zero.

D.2 Extended Literature on Mergers and Concentration

Posner and I. E. Marinescu 2020 discuss extensively the need for a more intense antitrust regulation, focusing on the US, to prevent the rise of monopsonistic dynamics in the labor market. They mention M&A's as a potential trigger for monopsonistic dynamics, especially when combined with relevant labor market frictions and anticompetitive behaviors, , *e.g.*, non-poaching and non-competitive agreements[27]. 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. They exhort authorities to take into evaluation labor markets spillovers when they evaluate mergers, besides those on prices and markups.

Shapiro 2019 argues that antitrust law should be enforced. There is evidence that larger and more efficient firms have been growing at the expense of their smaller and 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: to identify the subset of mergers that 'may substantially lessen competition', one must assess the likely competitive effects of a proposed merger *before* it is completed.

Jarosch et al. 2024 simulate the merger between two of the largest employers in each labor market with Austrian firm-level data and recompute wages at all employers. On average, wages in merging firms decrease by 7%. Mergers also have large spillovers on other workers, whose wages decreased by 3%. Their model also implies non-linear effects of concentration on wages: large effects are estimated in already highly concentrated markets. Moving from the 25^{th} to the 75^{th} percentile of concentration, wages decrease by 1 p.p..

Suresh et al. 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 occurs in about 5% of their events and 40% of those analyzed by Jarosch et al. 2024. 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 concentra-

[27]These mechanisms are discussed extensively in Boeri et al. 2023.

tion, which in turn enhances employers power, reducing the extensive (wages) and intensive (employment) margin.

Dodini et al. 2023 address mergers in the Norwegian labor market and prove that on average concentration is lower than expected and therefore many relevant M&A's have been denied to ensure competion when there was no need to. I. Marinescu et al. 2021 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 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 stronger in highly 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., which suggests that, beyond ruling out endogeneity, mergers represent a different kind of variation of concentration with a more detrimental effect.

Finally, Guanziroli 2022 estimates the effect of concentration on wage 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 effects are heterogeneous across different workers.

D.3 *Zephyr*

The *Bureau Van Djik* is the worldwide leader in providing all types of information regarding businesses and industries around the world. *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 rumors as well as announced and completed deals from the end of the 1990s to today. It covers all types of deals, from standard MAs to joint ventures, delocalizations, or closures. The entire database contains more than a billion records. The 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 the target, acquirer, and vendor employment volume. Table D.3.1 provides the list of the most targeted industries.

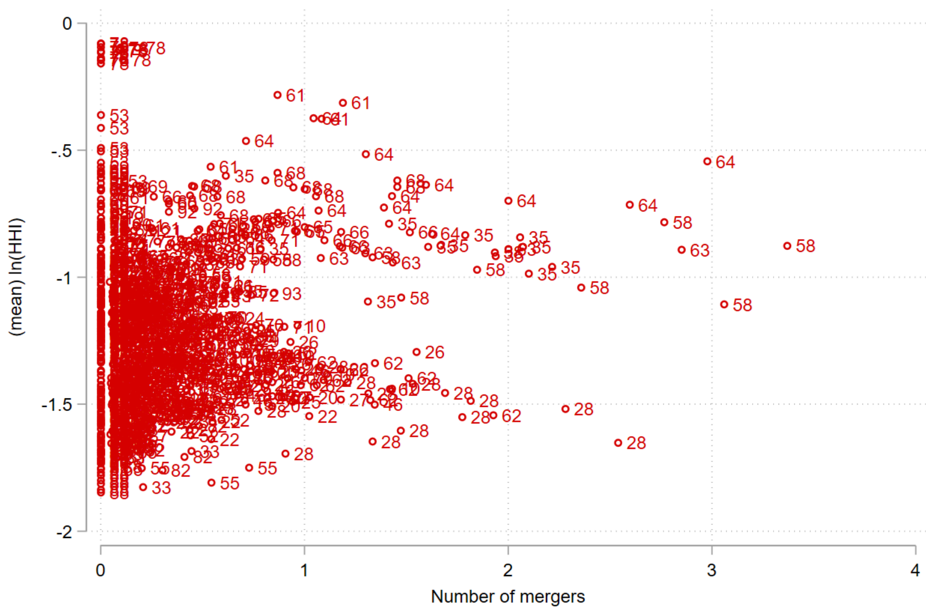


Figure D.3.1: Scatterplot between average (log of) concentration and the number of mergers across industries and years.

Notes: Observations are labeled with the corresponding two-digit NACE Rev.2 code, whose labels are displayed in Table D.3.1. Mergers are 184 between 2005 and 2018. On the y-axis there is the average industry HHI, computed as the average of all market HHI for that industry, while on the x-axis there is the number of mergers for each industry and year. Observations are approximately 1000 industry-year tuples associated with 71 two-digit NACE Rev.2 industries.

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 D.3.1: Most merger-targeted industries in my sample.

D.4 Instruments

Exogeneity To interpret estimates as causal, I must assess the validity of the exogeneity assumption. This means ensuring that the instruments do not directly influence the outcomes. 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, this is unlikely to 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 the 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 job title-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.

Instruments would only violate the exclusion restriction if mergers persist over time within the same markets. To address this issue, I exploit lagged mergers. This allows me to account for the fact that increases in concentration induced by mergers take time to show, and thus I can rule out the simultaneous determination between concentration and the outcomes. As a result, any market-specific endogeneity sources can be ruled out. Guanziroli 2022 andArnold 2021 used mergers to isolate concentration variations. They argue that the events were decided at the national level and that the local-based increase in concentration is thus exogenous.

Similarly, I rely on a national and industry-level measure of merger exposure, except for the small population of workers directly targeted by the merger under examination[28]. The exclusion restriction would be violated if mergers directly affect wages through productivity gains. The aim is to isolate the monopsonistic power effect while controlling for the potential bias posed by productivity gains. Arnold 2021 decomposes the average treatment effect of a merger on wages into three components, namely monopsony power, product market power, and productivity gains[29].

The aim is to isolate the former effect while avoiding the latter. Only mergers that affect concentration and productivity simultaneously are a concern. However, I believe that the mechanism highlighted in Equation 29 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. Furthermore, the estimator in Equation 7 compares the outcomes in the treated and control groups, leaving the difference in wages between the treated and control groups induced by productivity gains as bias. Table D.5.1 suggests that the bias is not relevant. Exogeneity is supported by the correlations of Table D.4.2 between the three measures of mergers exposure and wages. These correlations are very

[28]This mechanism involves only a negligible share of the treated workers, considering how I define markets.

[29]He derives the following equation in Section 2.2 at page 6:

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

(9)

small and negligible, suggesting that there is no direct relationship between the instruments and the main outcome of interest.

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 D.3.1, mergers occur between markets with different concentration levels, thereby dispelling the concern that they only target markets that are already highly concentrated.

Another concern is that mergers often reduce employment, which can lead to a downward bias in the employment specification. However, in the employment specification in Equation 4, new hires are the number of newly activated employment contracts in each market-year tuple and layoffs affect employment levels rather than employment flows. Therefore, this should not affect the identification strategy. However, it is 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 is why I use lagged measures of mergers. Similarly, bigger and more efficient companies can raise their employment. To account for variations in wages and hiring practices across firms of different sizes, I incorporated size bin-year fixed effects[30]. Additionally, mergers have nothing to do with mechanical bias inducing higher concentration in markets with fewer spells. The identification is thus robust to this bias.

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

Panel (a): $IV^2_{m,t}$	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
SD	.5259	.10451	.5073	.069363	0.20957*

Panel (b): $IV^1_{m,t}$	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
SD	.5218	.10425	.4988	.069976	0.25905*

Table D.4.1: Standardized differences in the mean, median and standard deviation of (log of) daily wages between treated and not industry-year tuples.

Notes: The bandwidths to assess whether differences are significant (*) are -.15 (.25) and .15 (.25) (Imbens and Rubin 2015). Instruments are defined in Equations 5 and 6. On the rows, there are the industry-year mean, median and St.Dev. of the outcome, while on the columns 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.

To test exogeneity, I compute the standardized differences for different moments, *i.e.,* mean, median, and standard deviation, of daily wages[31] between treated and untreated industry-year tuples[32]. Results are displayed in Table D.4.1. The estimated differences in mean and median daily wages are always not statistically significant. It means that mean and median daily wages do not differ significantly between treated and controls. Regarding the standard deviation, 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. It seems thus that the instruments do not directly affect outcomes.

[31] I don't 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 to wages, which likely implies that the same holds for all other outcomes as well.

[32] 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 4 and 5, I calculate the standardized differences for the three moments of wages between treated and untreated tuples.

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 D.4.2: Correlations between wages and mergers.

Notes: t, t-1 and t-2 indicate the number of mergers that occurred in the current year and in one and two years prior to the current one for each market-year tuple. Mergers are 184 in the period of analysis. Observations are 3,573,677 market-year tuples associated to 5,008 markets between 2005 and 2018.

Relevance The instruments must be strongly correlated with the endogenous covariate. First-stage estimates, shown in Table D.4.3, show positive and always significant coefficients, both when the instruments are considered separately and jointly. The instruments additionally satisfy the rule-of-thumb check: All F-statistics are much higher than the standard threshold of ten (Stock and Yogo 2005). Although correlated, instruments capture different sources of variation in concentration, as when they are considered jointly, both remain significant and sizeable.

The results indicate that the coefficients 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 $e^{\widehat{\theta}_j}$=$(2.718^{0.123} - 1) * 100$=13 p.p. and $e^{\widehat{\theta}_j}$=$(2.718^{0.211} - 1) * 100$=23 p.p.. The two instruments specification indicates instead that workers belonging to treated markets on average experience an increase in concentration of 30-38 p.p. to those belonging to not-treated markets. The coefficients do not differ significantly from those estimated in a DiD specification in Arnold 2021. With additional controls, industry-commuting zone-year fixed effects, he estimates an increase in concentration for treated markets from 19 to 27 p.p..

I. Marinescu et al. 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 et al. 2024 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 previous works.

I estimate an Average Treatment effect on the Treated (ATT) as an elasticity or a semi-elasticity depending on the outcome as long as the identification holds. The correlation between the outcome and concentration, θ, becomes $\widehat{\theta}_{\text{IV}}$, which, under certain assumptions, takes a 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 exposed to mergers with those that are not. Wald instruments do the same with the difference that they weight the difference in outcome between the two groups by the change in the endogenous covariate predicated by the instrument.

Dependant variable: Log(HHI)	(1)	(2)	(3)
Panel (a)			
$IV^2_{m,t}$	0.2109**	0.2109**	0.1708**
	(.07448)	(.07448)	(.05573)
Panel (b)			
$IV^1_{m,t}$	0.1740**	0.1740**	0.1387***
	(.0520)	(.0520)	(.0372)
Panel (c)			
$IV^2_{m,t}$	0.1973**	0.1973**	0.160**
	(.0703)	(.0703)	(.0530)
$IV^1_{m,t}$	0.1542**	0.1542**	0.1229***
	(.0456)	(.04567)	(.0336)
observations	3,573,677	3,573,677	3,573,677
(mean) sex & age	✓	✓	✓
market FE	✓	✓	✓
year FE	✓	✓	✓
job title FE	-	✓	-
region FE	-	✓	-
industry FE	-	✓	✓
region-year FE	-	-	✓
job title-year FE	-	-	✓

Table D.4.3: First stage estimates.

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates should be interpreted as semi-elasticity as the specification is in a linear-log form. Standard errors clustered at the market level in parentheses. Panel indicates the use of different instruments: (a) 2-years lagged mergers as in Equation 6; (b) 1-year lagged mergers as in Equation 5 and (c) both jointly. Observations are 3,573,677 employment contracts between 2005 and 2018. Controls are those of Equation 3.

D.5 Robustness Exercises

Alternative Identification To address potential concerns regarding the validity of the instruments, I test an alternative identification, namely a staggered event study design in which mergers are the treatment[33]. As the treatment is staggered and non-absorbing, I implement the De Chaisemartin and d'Haultfoeuille 2020 estimator. The results are shown in Figure D.5.1. Estimates are consistent, both in significance, magnitude, and timing, with the TSLS ones. Mergers take time to display their effect as concentration increases from the second year

[33]This strategy however does not estimate an ATT but an Average Treatment Effect (ATE).

which approximately corresponds to IV^2. This roughly translates into an increase of 14 p.p. in HHI at the mean[34], being close to the first-stage estimate of Table D.4.3. The large confidence intervals are inevitable as Hollenbach and Egerod 2024 show that, especially with small sample sizes, few events, and not-large causal effects, these new event study estimators hardly estimate precise results.

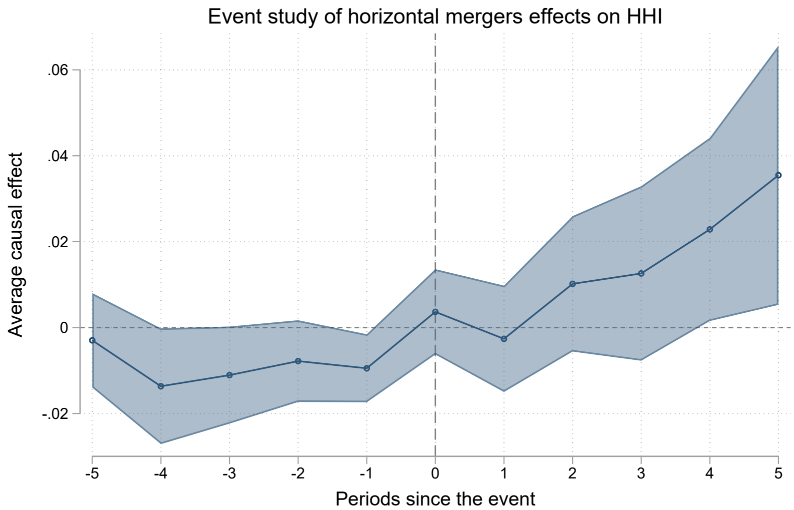


Figure D.5.1: Dynamics effects of horizontal mergers on concentration.

Notes: The graph displays the De Chaisemartin and d'Haultfoeuille 2020 estimates, with HHI as outcome. Treatment is a dummy equal to 1 if a market experiences a merger in t, 0 otherwise. 0 is the year in which the merger happens, while 1 and 2 correspond basically to IV^1 and IV^2 of Eq. 5 and 6. Controls are market and year fixed effects and market-year avg. age and share of men. Standard Errors are estimated with 50 bootstrap replications. Obs. are 47,180 market-year tuples.

Spillovers Mergers may increase productivity, profitability, and investment, leading to higher growth. This mechanism also relates to the absence of product market concentration in the specification, which can be correlated with all of these gains and, in turn, with the outcomes. In this case, the exclusion restriction fails. To address this, 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 industry-by-year variables between 2005 and 2018 from *Eurostat*[35]. To rule out collinearity concerns, I perform a Principal

[34] The estimate is approximately 2 points which, compared to a 0.14 mean, roughly translates into a 14 p.p. increase.
[35] Full list is available here or upon request.

Component Analysis (PCA), saving the first 10 components that explain roughly 77% of the total variance[36]. The advantage of this strategy is that the PCA factors are, by construction, orthogonal to each other, thus ruling out any collinearity concern. With IV^1, the estimated elasticity is still not statistically significant, while with IV^2 and both instruments the estimates are statistically significant and similar in magnitude to the baseline. In the former, the coefficient is -0.132, the SE 0.0375, and the t-statistic -3.53. In the latter, however, the coefficient is -0.138, the SE 0.0372, and the t-statistic -3.71. This suggests that industry-specific gains from mergers are not a concern for identification.

Firm-specific Confounders Mergers could involve firm-specific gains that are not fully captured by sectoral covariates. This would violate the exclusion restriction, as the instruments would be correlated with the outcomes directly through a firm-specific channel. Therefore, I estimate the standard TSLS specifications with firm fixed effects. The results are shown in Table D.5.1. The estimates are substantially unchanged, both in magnitude and in statistical significance, thus excluding the fear that there could be a possible threat induced by the correlation between firm-specific characteristics and mergers[37].

Log(Daily Wages)	(1) IV^1	(2) IV^2	(3) $IV^{1;2}$
$\widehat{\theta_{IV}}$	0.049	-0.134**	-0.113**
	(.112)	(.0574)	(.049)
Observations	2,195,970	2,195,970	2,195,970
Full set of controls	✓	✓	✓
Firm FE	✓	✓	✓

Table D.5.1: Daily wages estimates with the full set of controls and firm fixed effects.

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the market-year level in parentheses. The empirical specification and controls are displayed in Equation 3. The estimates are based on the main sample of all news hires in the period 2005-2018. Obs. are lower than the full sample because of the singletons dropped by adding fixed effects. The instruments are defined in Equations 5, 6 and 7.

[36]The first two explain approximately 35%.

[37]This is true under the assumption that these firm-specific characteristics are time-invariant, which is reasonable considering the short period on average in which firms appear in our panel.

HHI Levels Mergers may target specific markets whose concentration levels differ. This may bias the identification as the estimates would potentially reflect differences in concentration levels rather than the exogenous shocks induced by the instruments. Therefore I add to the baseline specification market-specific HHI levels by year fixed effects, which is also a proxy for market-by-year fixed effects. The results are shown in Table D.5.2. The estimates are still highly statistically significant and the magnitude is similar to the baseline. This suggests that the identification is robust to (i) mergers targeting systematically overly concentrated markets; and (ii) time-varying market-specific confounders[38].

Log(Daily Wages)	(1) IV^1	(2) IV^2	(3) $IV^{1;2}$
$\widehat{\theta_{IV}}$	-0.196	-0.147***	-0.152***
	(0.147)	(0.0439)	(0.0426)
Observations	2,928,474	2,928,474	2,928,474
Full set of controls	✓	✓	✓
Market HHI-year FE	✓	✓	✓

Table D.5.2: Daily wages estimate with the full set of controls and market-specific HHI levels by year fixed effects.

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered at the market-year level in parentheses. The empirical specification and controls are displayed in Equation 3. Market HHI is computed as the average of the HHI for each market between 2005 and 2018, and then interacted with the yearly dummies. The instruments are defined in Equations 5, 6 and 7.

Layoffs Mergers can be followed by rebuilding processes, involving layoffs, which in turn affect wages and employment. Therefore, I control for layoffs as a covariate in the TSLS regressions to address this concern. *LoSaI* provides, for each ended spell, the motivation. For each market and year, I count the number of spells terminated because of the firm's decision. Hereafter, I report the results for daily wages obtained with the full set of controls plus the number of layoffs per market and year across the three specifications. With IV^1, the estimated elasticity is still not statistically significant. With IV^2 and both, estimates are still statistically significant and the magnitude does not change significantly. In the former, the coefficient is

[38]Mainly, product market concentration or market tightness.

-0.1386, the SE is 0.040, and the t-statistic is equal to -3.44. In the latter, however, the coefficient is -0.144, with an SE of 0.0402, and a t-statistic equal to -3.59. Both coefficients are still significant at a 99% confidence level, suggesting that bias induced by the side effects of mergers is not of concern for the TSLS strategy. As expected, the coefficient associated with layoffs is always negative and statistically significant.

Clustering So far, I clustered the standard errors at the market-year level as concentration varies across markets and years. However, the variation exploited arises at the industry level and flows through markets afterwards. Moreover, the instruments I built rely on lagged mergers, thus exploiting the persistence of the mergers' effect within markets over time. The implicit assumption is that, within each market and year, observations can be correlated as they are exposed to the same shock. I relax the clustering constraint, allowing the observations to be correlated within markets over time[39]. In the preferred one (Panel (c)-column 4 in Table 1), the t-statistic becomes -2.49 and the p-value 0.013. The t-statistic and the p-value of Panel (b)-column 4 become -2.52 and 0.012. With column 3 controls, estimates are again significant (t-statistic=-2.08 and -1.9; p-value=0.038 and 0.057). In summary, the significance of all estimates slightly decreases but the null hypothesis is always rejected.

Placebo The mechanism underlying the identification is that mergers concentrate hires among fewer firms within the same labor market thus increasing concentration. Accordingly, non-realized mergers should not have any effect on concentration. Therefore, I select from the *Zephyr* archive all announced but not realized horizontal mergers in Italy between 2005 and 2018[40]. I define the same instruments as above and estimate the same set of TSLS regressions. Results are displayed in Table D.5.3 and none of the estimates are statistically significant and they are small in magnitude. This indicates that there is no effect of non-realized mergers as

[39] Results are not attached to the paper but available.
[40] The events consist of slightly more than 500 still pending or withdrawn mergers.

they do not raise concentration.

Log(Daily Wages)	(1) IV^1	(2) IV^2	(3) $IV^{1;2}$
$\widehat{\theta_{IV}}$	-0.119	0.0102	-0.0179
	(0.147)	(0.0281)	(0.0235)
Observations	2,928,474	2,928,474	2,928,474
Full set of controls	✓	✓	✓

Table D.5.3: Placebo estimates with announced but not realized mergers.

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the market-year level in parentheses. The empirical specification and controls are displayed in Equation 11. The instruments and the estimate are defined in Equations 5, 6 and 7, which non-realized mergers instead of realized.

D.6 Additional Analysis

Margins I estimate the following equation on two separate outcomes, namely the number of worked days (extensive) and overall remuneration (intensive):

$$log(O_{i,m,t}) = \delta_i + \mu_m + \gamma_s + \Gamma_{r,t} + \Lambda_{d,t} + \Phi_{o,t} + \beta_t + \theta log(HHI_{m,t}) + \Gamma Z_{i,t} + u_{i,m,t} \tag{10}$$

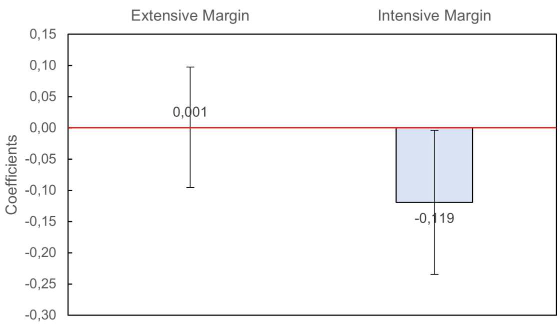
, where i indexes workers, r regions, o job titles, j firms, d class sizes, s industries, and t years. The model is specified in a log-log form and $\widehat{\theta}$ should be interpreted as the elasticity of the outcome O to concentration. Results are displayed in Figure D.6.1a. The estimated elasticity 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 channel. In contrast, the intensive margin is not affected at all by concentration. It seems thus that employers in more concentrated markets simply can, and thus do, reduce the wages of entrants' workers. They do it presumably due to the high search and matching costs and frictions that characterize Italian LMs, which prevent workers from easily switching jobs and markets, and due to the few skills acquired by workers over time.

Job Security and Heterogeneity I estimate the following equation:

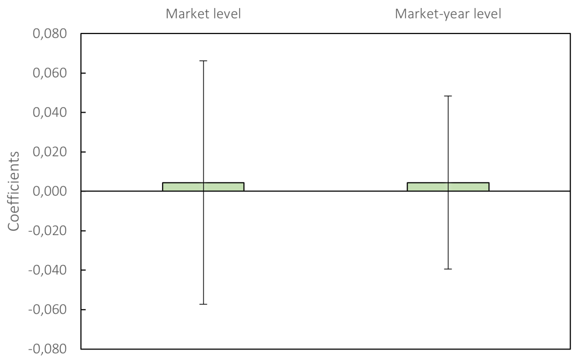
$$P_{i,m,t} = \delta_i + \mu_m + \gamma_s + \Gamma_{r,t} + +\Lambda_{d,t} + \Phi_{o,t} + \beta_t + \theta log(HHI_{m,t}) + \Gamma Z_{i,t} + u_{i,m,t} \quad (11)$$

where Z contains a cubic polynomial in age and spells length to proxy individuals' specific on-the-job working experience. The equation is specified as a linear log - thus θ should be interpreted as a semi-elasticity - and is estimated as a Linear Probability Model, and

$$P_{i,m,t} = \begin{cases} 1 \text{ if worker } i \text{ in market } m \text{ is hired with an OEC in year } t\text{; 0 otherwise;} \\ 1 \text{ if worker } i \text{ in market } m \text{ is renewed with a FTC in year } t \text{ and firm } j \mid FTC_{t-1,j}\text{; 0 otherwise.} \end{cases}$$



(a) Worked days and overall remuneration.



(b) Probability of OE hiring.

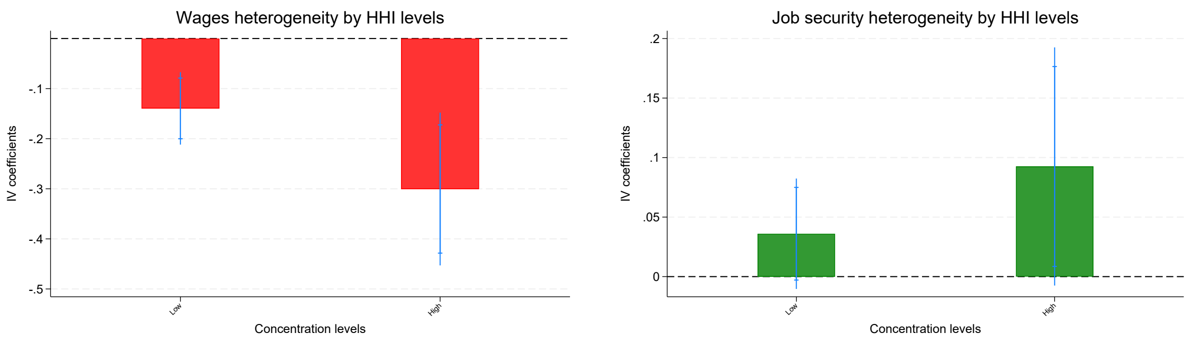
Figure D.6.1: TSLS estimates on additional outcomes.

Notes: Panel (a) displays estimates from Equation 10. The extensive margin is the number of worked days per spell, while the intensive margin is the spell's nominal remuneration. Standard Errors are clustered at the market-year level. Panel (b) displays estimates of Equation 11. The outcome is a dummy equal to 1 if the entrant is hired with an OE contract, 0 otherwise. Standard errors clustered at the market and market-year level. 95% confidence interval displayed. Observations are 3,573,677 working spells between 2005 and 2018. In (a) and (b), I use the two IVs and the full set of controls.

	(a) P($\text{FT}_{i,j,t}\|FT_{i,j,t-1}$)			(b) Log(Daily $\text{Wages}_{i,j,t}\|FT_{i,j,t-1}$)		
	(1) IV^1	(2) IV^2	(3) $IV^{1;2}$	(1) IV^1	(2) IV^2	(3) $IV^{1;2}$
$\widehat{\theta_{IV}}$	0.719	0.417**	0.498***	0.0637	-0.130**	-0.0794*
	(0.503)	(0.176)	(0.176)	(0.0835)	(0.0632)	(0.0447)
Observations	164,598	164,598	164,598	161,926	161,926	161,926
Full set of controls	✓	✓	✓	✓	✓	✓

Table D.6.1: Estimates on FT-to-FT renewals.

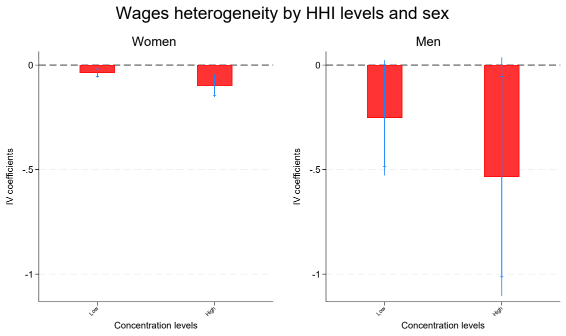
Notes: *** p<0.01, ** p<0.05, * p<0.1. The empirical specifications and the controls for outcomes (a) and (b) are displayed in Equations 11 and 3. Standard errors clustered at the market-year level in parentheses. I restrict the sample to workers who have at least two temporary contracts in the same firm. The outcome is a dummy variable equal to 1 if the following contract is temporary, 0 otherwise. Obs. are lower than the full sample because of the singletons dropped by adding fixed effects. Wages are the log of daily wages of temporarily renewed workers. Obs. are lower in (a) than in (b) because the log transformation drops zeros in wages. The instruments are defined in Equations 5, 6 and 7.



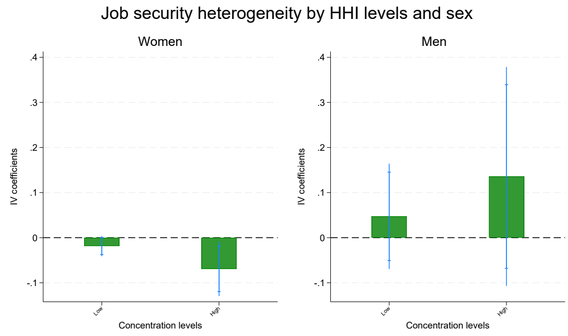
(a) Daily wages. (b) Probability of OE hiring.

Figure D.6.2: Heterogeneity estimates by concentration levels.

Notes: The empirical specifications and controls are displayed in Equations 11 and 3. Panel (a) displays daily wages (red), while Panel (b) job security (green). On the y-axis, there is the TSLS coefficient. while on the x-axis, the group. Job security is the likelihood of being hired with an OEC. The bars represent the 95 and 90% confidence intervals. Estimates are obtained through the preferred TSLS specification with both IV^1 and IV^2. HHI levels are split according to the US antitrust norm: low concentration for HHI values below 0.15, high concentration for HHI values equal or above 0.15.



(a) Daily wages.



(b) Probability of OE hiring.

Figure D.6.3: Heterogeneity estimates by sex and concentration levels.

Notes: The empirical specifications and controls are displayed in Equations 11 and 3. Panel (a) dispalys daily wages (red), while Panel (b) job security (green). On the y-axis, there is the TSLS coefficient. while on the x-axis the group. Job security is the likelihood of being hired with an OEC. The bars represent the 95 and 90% confidence intervals. Estimates are obtained through the preferred TSLS specification with both IV^1 and IV^2. HHI levels are split according to the US antitrust norm: low concentration for values below 0.15, high concentration for values equal or above 0.15.