Essays on Labor Market Concentration and Atypical Contracts

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Introduction

Preamble This thesis is made up of two chapters and it has been written under the supervision of Professor Claudio Lucifora, Full Professor at the Catholic University of Milan and IZA. The first chapter, entitled *"Monopsony in Labor Markets: Empirical Evidence from Italian Firms"*, is written by me alone, while the second, entitled *"Do Alternative Work Arrangements Substitute Standard Employment? Evidence from Worker-Level Data"*, is coauthored with Bernardo Fanfani, Assistant Professor at the University of Turin. The reviewers are Andrea Garnero (OECD and IZA) and Giovanni Sulis (University of Cagliari and IZA).

Employers' power has grown largely in the last decades worldwide. However, there is a lack of evidence on all the effects it might have on the labor market and, in particular, on workers' outcomes. My dissertation addresses this question, studying in the first chapter labor market concentration while in the second a indirectly related topic which is the spread of Alternative Working Arrangements (AWAs so on). To accomplish these aims, I exploit different and novel administrative dataset. On these data, I implement several quasi-experimental techniques, namely instrumental variable regression, sample selection estimator, and difference in difference, to rule out all different sources of endogeneity arising in the two contexts and identify causal effects between the variables of interest.

Italy is a case study to address the power of employers and its effect on workers' outcomes. Collective bargaining in Italy is structured on two levels: the first one (Contratti Collettivi Nazionali di Lavoro or *CCNL*) sets minimum wages schedules and working conditions at the industry and local level, while the second happens at a local or even firm-level bargains additional and firms or local specific details. However, the framework from the first level is confusing, as it is hard to map all different contracts and even harder to recognize the players and verify to which extent they are representative of workers and employers. There is evidence that many workers' unions are not representative, as they cover few workers and consequently sign the so-called *Pirate* agreements. Minimum Wages are bargained and set within this context. The minimums set across industries are formally high, compared to average and median wages, but empirical evidence on administrative microdata shows that real wages paid to workers are significantly lower than those formally contracted.

In addition, Italy has experienced different trends in labor market reforms. Until the '90s, the Italian labor market was characterized by a high stringency for both open-ended (OE) and temporary (FT) contracts, while employment growth was decreasing, and firms demanded higher flexibility in order to be able to raise productivity and become more resilient. Following the examples from other OECD countries, several reforms were promulgated to reduce legislation stringency on the use of temporary contracts. These reforms were law No.108 in 1990, and the *Biagi* Reform in 2003. Italy ends up in a dual-labor market situation, so where the Employment Protection Legislation (EPL so on) stringency between open-ended and temporary contracts largely differs: it was high for the first and low for the second. Due to this, several reforms were additionally promulgated to reduce the stringency of OE contracts, to foster employment and recovery after the financial crisis: the *Fornero* reform in 2012 and then the *Jobs Act* in 2015. Figure 1 displays an index that describes the evolution of the strictness of labor market legislation in several European countries.

Shortly after indeed, the trend began to reverse: the centre-left coalition led by PM Paolo Gentiloni abolished the labor vouchers, the Italian-specific form of Alternative Working Arrangements, that were largely criticized because of their supposed detrimental effect on workers' earnings and careers. In 2018, the Five Stars Movement and the Northern League, which have strongly opposed previous governments and their reforms, won the election. Shortly after, they lowered the retirement age in the *Quota 100* reform, increased the EPL significantly for temporary contracts with the *Dignity Decree*, as shown in Figure 1, and approved the *Citizenship Income*, basically an income for all unemployed provided with little constraints and no fixed time window. Previous reforms, regardless of their effects on employment, have certainly not enhanced workers' power but rather firms', as they have reduced the severance of the legislation and increased firms' flexibility to hire and fire. In short, these reforms could have been a trigger for monopsonistic dynamics and thus represent an additional motivation to study the Italian Labor Market.

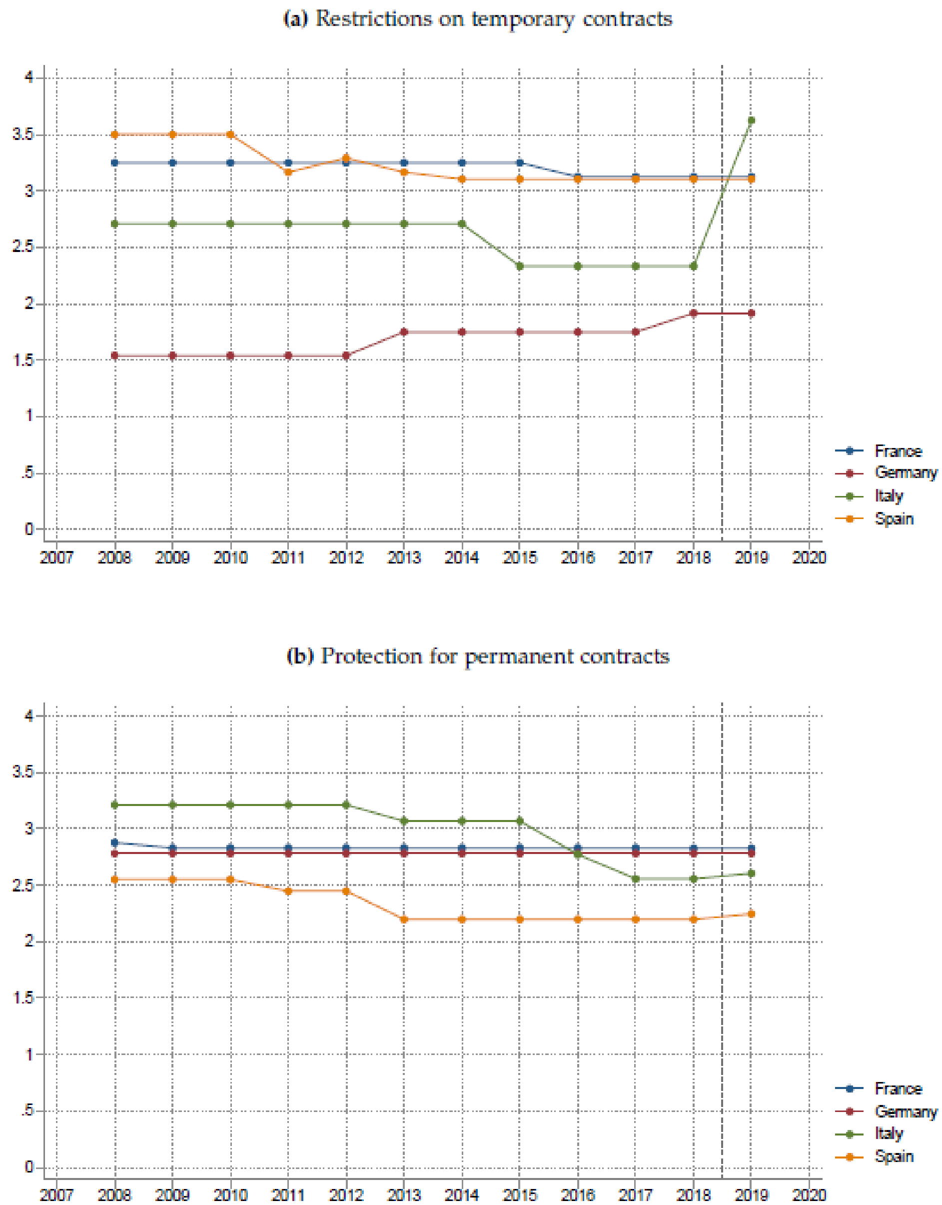

Figure 1: **Strictness of EPL on FT and OE contracts in Italy, Spain, France, and Germany between 2007 and 2020**.

Source: Palladino and Sartori (2022) on OECD data.

In addition to all these country-specific determinants, there are several global trends: the spread of globalization, the global value chain becoming larger and larger and always more connected, businesses becoming more and more specialized and demanding, workers' skills continuously updating, becoming more technical and complex, while country-specific tax system and social earnings mechanisms cannot keep up and provide their citizens with the adequate protections. In this context, studying employers' power and the spread of non-competitive dynamics is necessary. In particular, it is necessary to study all the different channels that employers' power can take to display its effect in the Labor Market and investigate all the outcomes that, to some extent, can be affected. This is the main goal I aim to accomplish in my thesis.

My thesis develops as follows: in the first chapter, I address employers' power directly by studying the presence of monopsony across Italian local labor markets (LLMs so on). I rely on a matched employer-employee database, drawn by the *INPS* archive, representative of the universe of Italian private sector workers to calculate a measure of labor market concentration based on new hires. In the literature, concentration is used as a proxy for monopsony, *i.e.*, a situation in which few firms dominate a specific market stream and workers have weak or null bargaining power. Theoretical models predict that, in such a framework where employers' power is large and undisputed, firms can lower the use of productive inputs on the extensive and intensive margin, namely labor supply and wages.

I investigate this by identifying the effects of concentration on entrants' wages, on job security, measured as the probability of being hired with an open-ended contract, and hires. On average, LLMs are not concentrated. However, the distribution is highly heterogeneous especially across industries. I then combine my entrants' matched-employer-employee dataset with the *Zephyr* database, which contains the universe of mergers and acquisitions worldwide, to obtain a measure of industries' exposure to horizontal mergers in Italy. I hence develop an IV strategy based on the heterogeneous exposure of industries to mergers to find a source of variation in concentration. I aim to address unanswered concerns about their labor market spillovers, as feared in the antitrust literature and suggested by empirical evidence.

I find that concentration, when driven by mergers, negatively affects all outcomes in the short and medium period. Elasticities range between -0.14 and -0.07 percentage points (p.p. so on) for entrants' wages and between -0.77 and -0.68 p.p. for hires. Instead, job security is not affected overall. However, baseline estimates hide relevant heterogeneity: job security is affected for women only especially where concentration is higher, the significance of all outcomes is driven by women, while the magnitude by men although coefficients are mostly not statistically significant, and concentration harms more wages and job security in southern Italy. Furthermore, estimates on all outcomes increase in magnitude as concentration levels increase. Mergers that occur in markets with different concentration levels thus have highly heterogeneous effects on the outcomes depending additionally on the workers they target.

In the second chapter, we address a more specific and largely unexplored topic, namely the spread of an extreme form of temporary contracts, known as the labor vouchers, available in Italy between 2008 and 2017. Not only but particularly in Italy, the use of temporary contracts has skyrocketed in the last decades. However, recently, even more precarious and atypical forms of employment, the so-called AWAs, have emerged in the European and US labor markets. These types of contract, although different across different labor markets, have many common characteristics. In short, they have been introduced to help firms become more resilient and flexible, covering working activities without fixed time schedules and involving nonspecific tasks, with the aim of fostering employment in the low-wage segments of the labor market. However, they have been harshly criticized because of the low level of social protection entailed, and because they have been used to cover undeclared employment, as proven empirically, instead of bringing it to light.

We exploit two novel administrative sources. The first, extracted from *INPS*, is a unique dataset containing all voucher spells of a sample of voucher users between 2008 and 2015. The second instead is matched employer-employee dataset drawn from the Estratti Conto archive provided by *INPS* describing all the other income sources for the same population of voucher users in that period. We combine the two data sets to obtain a panel that describes the monthly income trajectories of our population between January 2008 and December 2015.

We investigate whether they replace or complement standard labor income sources, welfare transfers, and overall income. I estimate cross-income elasticities relying on a fixed effects specification and on a novel estimator controlling for the selection of individuals in the labor market depending on observable and unobservable determinants. This exercise has never been done before in the literature. Estimates indicate that vouchers increase overall labor income and substitute standard contracts, but, when selection is addressed, the positive effect on total labor income reduces while the substitution effects become larger. The effects of vouchers are heterogeneous, depending on sex, age, and nationality.

We then set up an event study exploiting a threshold which set a limit on workers' yearly voucher earnings between 2012 and 2014. The event consists of a worker reaching the 6,667 gross voucher income in a year: *INPS* prohibits any worker from earning additional voucher income for the remainder of the year. We aim to identify what happens to these workers once they have reached the threshold, to see whether they can replace the income they previously gained through vouchers with other income sources and whether their total income increases or not. We find that when the threshold is reached, voucher income drops and so does overall income, with an increasing effect over time. No effects are found on other incomes sources. It seems thus that vouchers were used as a tool to access the labor market and, when they are not anymore available, users are pushed out of the labor market, at least the formal one. Overall, the effect of AWAs in the Italian Labor Market depends on whether sample selection bias is addressed, but overall they did not foster earnings.

In my thesis, I provide several contributions. From an academic point of view, I address the effect of employers' power in two different frameworks measuring it with highly precise administrative microdata and identifying its effect on several worker-level and overall outcomes, namely wages, job security, and hires. Methodologically, I contribute as I set up several different quasi-experimental techniques which separately exploit different sources of variation in covariates of interest. In the first chapter, I set up a novel IV strategy exploiting horizontal mergers as a trigger for increases in concentration. To my knowledge, I am the first to do that.

In the second chapter, instead, we take into account the endogenous selection of workers into the sample exploiting the Wooldridge and Semykina (2010) Correlated Random Effect estimator. We then estimate a Difference-in-Difference event study exploiting an unexplored discontinuity in the legislation. We are among the few to address AWAs, and the first ever to address them by answering these research questions and with these methodologies.

I also contribute from a policy point of view. In the former chapter, I address the role of monopsonistic dynamics in the Italian labor market and how they are triggered by horizontal mergers, thus helping policymakers and competition authorities to effectively address mergers and counter their detrimental effects. In particular, I find that concentration increases induced by mergers are not homogeneous, but instead highly heterogeneous depending on concentration levels.

I identify five industries which, since they have experienced mergers and that are highly concentrated, should be carefully evaluated by competition authorities. These are Financial Activities, Information and IT Services Activities, Editorial Activities, Electric and Gas Furniture, and Satellite Telecommunication. Moreover, concentration does not affect workers in the same way across Italian regions as women in southern regions are more damaged, especially in terms of job stability.

In the latter, instead, we investigate the income dynamics of those workers who belong to the weakest segments of the labor market, those characterized by greater career fragmentation and by no fixed time schedule and low value-added tasks. In the last decades, AWAs spread in Italy as well with a negligible positive effect on workers' earnings but increasing fragmentation and lowering social protection with respect to standard employment forms. However, for a specific sub-sample of high-intensive users, they have been a tool to access the formal labor market and gather income. We believe that the legislation regulating voucher use was poorly shaped: rather than liberalized and then abolished, vouchers should have been reformed to precisely target specific workers in specific industries.

I want to conclude by discussing the limitations of these works and the additional steps that future research should take, according to my findings, in these fields. Regarding the first chapter, better data would be needed to calculate highly-precise measures of labor market concentration. In particular, more detailed information on occupations and geographical dimensions would improve the calculation of the concentration index and, in turn, the identification strategy. Ideally, one should be able to directly link each merger with the firms in every market through the fiscal code. This would allow to build a more precise instrument which exploits a firm-level source of variation in concentration that would allow to include a set of fixed effects up to a firm-year and industry-year level. Unfortunately, I do not have access to this kind of data.

Furthermore, many questions regarding the effect of concentration on workers' welfare remain unanswered. I aim to answer to one of these in the next future investigating, in a separate project, the effect of concentration on job content, thus shedding light on an additional worker welfare dimension that could be affected by employers' powers.

Regarding instead the latter, limitations are mainly two: the missing of a unique identification strategy which addresses jointly the sample selection and the endogeneity issue, and the fact that I am only able to address the supply side of AWAs and not the supply and demand side jointly. Further research should devote more effort to address it jointly from a supply (*i.e.*, workers) and a demand (*i.e.*, firms) side point of view. For instance, what would happen when firms employ to many workers with vouchers when they reach the threshold? Do they change their workforce mix, and how? Do they rely on different contracts or do they simply hire other workers with vouchers? Which types of firms use AWAs and what are their performances? These are just a few of the questions that remain answered in this field.

Finally, I am working to merge these two streams of research. I aim to address whether, and through which channels, labor market concentration affects AWAs use identifying in turn the effect of monopsony on mostly unexplored worker level outcomes, namely job security and job content.

Monopsony in Labor Markets: Empirical Evidence from Italian Firms*

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Abstract

This paper uses a comprehensive matched employer-employee dataset covering the entire Italian private sector to analyze the effects of labor market concentration on wages, job security, and employment. By constructing a flows-based labor market concentration index, I find substantial heterogeneity in concentration levels across different industries. To estimate the causal effect of concentration, I employ an IV strategy based on the differential exposure of industries to horizontal mergers. First-stage results confirm that mergers raise concentration, with elasticity estimates ranging between -0.14 and -0.07 percentage points for wages and between -0.77 and -0.68 percentage points for hires. Furthermore, I find that job security is negatively affected for women only. The estimates are highly heterogeneous by sex and concentration levels. Overall, the paper provides evidence that horizontal mergers increase concentration, which in turn harms workers and employment.

Keywords: monopsony; labor market concentration; mergers; wages; hires; job security.
JEL codes: J31; J42; J71; L13; L41.

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1 Introduction

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

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

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

Monopsony has traditionally been measured by the degree of concentration in product markets, but monopsonistic dynamics can also arise in the labor market. In such a scenario, firms have the power to employ fewer workers and pay lower wages than would be observed in a competitive market. Labor market concentration has been identified as a potential factor contributing to the emergence of monopsonistic dynamics, especially when workers have limited bargaining power and there are significant labor market frictions (*e.g.,* Boeri, Garnero, and Luisetto 2023, Amodio, Medina, and Morlacco 2022). Although European labor markets have stronger institutions than those in the US, they are not necessarily more competitive (Araki et al. 2022).

There is a dearth of research on monopsony and labor market concentration in Italy. Previous studies on monopsony in Italy are limited (Sulis 2011; Fanfani 2022), and only one study has examined labor market concentration, but not specifically in Italy (Bassanini, Bovini, et al. 2022). This paper aims to address this gap in the literature by estimating the effects of labor market concentration on hiring, daily wages, and job security, measured as the probability of being hired with an open-ended contract (OEC).

The literature on labor market concentration can be divided into mainly two streams. The first utilizes firm-level stock data to estimate a concentration measure across labor markets and assess its effect on firms' wage bill and employment (Azkarate-Askasua and Zerecero 2023; Azar et al. 2020; Martins 2018; Qiu and Sojourner 2022). The results suggest that labor market concentration is associated with lower wages, particularly for women, and employment[1]. The second instead utilizes matched employer-employee dataset to estimate flow-based measures of concentration based on newly activated working spells across different labor markets, in order to identify its effects on different outcomes (I. Marinescu, Ouss, and Pape 2021; Bassanini, Batut, and Caroli 2023, Dodini et al. 2023a; Gregor Schubert and Taska 2020; Bassanini, Bovini, et al. 2022; Martins 2018).

Compared to the first strategy, this approach has a few advantages: it allows for more granular measures of concentration across time, and it distinguishes the effect of concentration on entrants' and incumbents' wages, which has been found to differ (Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022, Martins 2018). Moreover, it provides alternative identification strategies compared to the standard approach based on a leave-one-out instrumental variable regression (*e.g.,* I. Marinescu, Ouss, and Pape 2021; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022). I contribute to this literature implementing a different identification strategy based on horizontal mergers (Guanziroli 2022; Arnold 2021), which investigates a mostly unexplored channel through which concentration may rise.

There is a significant body of literature focused on estimating micro-labor supply elasticities for workers and firms (*e.g.,* Bachmann, Demir, and Frings 2022; Langella and Manning 2021; Datta 2021; Sulis 2011; Sokolova and Sorensen 2021; Fanfani 2022; Amodio and Roux 2021; Amodio, Medina, and Morlacco 2022) to assess the competitiveness of labor markets. Positive labor supply elasticities imply that workers' labor supply increases with wages and firm size, which is interpreted as a sign that competitive dynamics are unlikely to develop (Manning 2003; Manning 2011). This paper contributes to this literature by estimating micro-elasticities of hires and daily wages with respect to market concentration.

I calculate a measure of labor market concentration, using the Herfindahl-Hirschman Index (HHI so on), across Italian Local Labor Markets (LLMs so on). Only the working spells of entrants' workers from *LoSaI*, a matched employer-employee database drawn from the *INPS* archive, between 2005 and 2018 are

[1] Additionally, labor market concentration is positively correlated with product-market concentration and negatively with the unionization rate.

selected. Markets are defined as the interaction of regions, industries, and occupations. The definition slightly differs from the standard one in the literature, which usually defines a market as an interaction of a commuting zone and an occupation, for two reasons. First, I want to obtain a more granular definition of an LLM than the standard one. Second, I want to investigate whether concentration differs across industries, as Fanfani 2022 suggests that industry heterogeneity drives monopsonistic dynamics. I find that this is also the case.

There is a need to investigate alternative channels that enhance concentration. For instance, Dodini et al. 2023b shows that concentration is driven by the concentration of skills demanded by firms. An additional driver could be the heterogeneity of local labor markets to merger exposure. I hence combine my entrants' matched-employer-employee dataset with the *Zephyr* database, which contains the universe of mergers and acquisitions worldwide, to obtain a measure of industries' exposure to horizontal mergers in Italy. Figure 2 indicates that concentration is positively correlated with mergers exposure across industries over time. Therefore, I set up a quasi-experimental framework to predict a variation in concentration where markets are treated or not depending on whether they experience horizontal mergers.

I use a flow-based measure of concentration rather than a standard stock-based one because it provides a more precise and dynamic picture of how concentration evolves over time if new hires accurately measure available job opportunities for workers. The majority of the 5,008 Local Labor Markets (LLMs) identified in my data are not concentrated, as the median value is below the low-concentration threshold. However, a few LLMs are driving the average value upward. In contrast, Martins 2018 found that approximately 9% of Portuguese workers are subject to a level of concentration that raises concerns (*i.e.*, HHI higher than 0.15). In my context, the corresponding share is approximately 3%.

Next, I estimate the correlation between labor market concentration, wages, and employment using a worker and market fixed effects specification, similar to *e.g.*, I. Marinescu, Ouss, and Pape 2021, Azkarate-Askasua and Zerecero 2023, Bassanini, Batut, and Caroli 2023, and Bassanini, Bovini, et al. 2022. To address endogeneity concerns, I rely on horizontal mergers across years to build a set of instruments for labor market concentration. An industry-level variation is presumably orthogonal to all the threats at a market level that simultaneously affect wages and concentration, allowing me to identify the true effects of concentration on the outcomes of interest. Results suggest that the instruments predict an upward shock in concentration at a market level ranging between 15 and 21 percentage points, which in turn reduces wages by approximately 0.09 to 0.14 percentage points and hires by 0.7 to 0.8 percentage points. The estimates exhibit relevant heterogeneities that I explore further in Section 5.6.

Concentration has the potential to reduce wages through two channels. Firstly, on the extensive margin, employers may reduce the remuneration with labor supply fixed. Secondly, on the intensive margin, employers can force the labor supply to increase while keeping the overall remuneration fixed. Qiu and Sojourner 2022 find that workers' human capital decreases in firms with greater market power, Bachmann, Demir, and Frings 2022 that non-routinary jobs are exposed to higher degree of monopsony than routinary jobs because of workers' on-the-job specific human capital and preferences for non-pecuniary jobs characteristics, while Amodio, Medina, and Morlacco 2022 that higher concentration pushes workers towards self employment[2]. However, there are only a few studies that address the mechanisms through which wages reduce. In my study, I aim to shed light on the two main channels through which concentration reduces wages, disentangling the effect on the extensive margin, represented by the number of worked days, and the intensive margin, represented by the overall remuneration[3].

Another dimension of workers' welfare that concentration may impact is job security, that I measure as the likelihood of being hired with an open-ended contract (OEC so on). Previous research by Bassanini, Bovini, et al. 2022 found no effect of concentration on job security in Italy. In this paper, I examine whether this result holds using a different identification strategy. Overall, I find a similar result to previous research, but when estimating separate elasticities by sex and concentration levels, I discover that women are indeed affected by concentration, with the magnitude of the effect increasing along the concentration distribution. In summary, this paper makes two main contributions: (i) it implements a novel empirical strategy to identify the effect of concentration on several labor market outcomes, exploring different transmission channels and heterogeneity, and (ii) it addresses mostly unexplored concerns in Europe and Italy regarding the labor market spillovers of mergers.

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

In recent years, economists from various fields have increasingly turned their attention to antitrust issues. A growing body of academic literature among US antitrust scholars calls for more stringent antitrust enforcement, especially with regard to horizontal mergers (I. Marinescu and Hovenkamp 2019;

[2]Self employment is seen as an exit strategy for lowering wages.

[3]I provide more details on the margins in Section 5.4.1. Unfortunately, the INPS data does not allow me to explore whether the reduction in wages is due to employers' exercise of power or to a worsening of human capital, as I do not observe workers' skills, the tasks they perform on-the-job, or their educational attainment. It is worth noting that investigating the effect of concentration on human capital is an exercise for a separate paper exploiting different data.

E. A. Posner and I. E. Marinescu 2020; Jarosch, Nimczik, and Sorkin 2019; Shapiro 2019; Suresh, E. Posner, and Wey 2018). These scholars argue that product markets in the US have become highly concentrated over the past few decades, due in part to weak legislation on mergers enforced by antitrust agencies. More recently, labor markets have also become a subject of scrutiny. While this debate is also gaining attention in Europe, empirical evidence on the subject is still scarce. From a policy perspective, my work contributes to this literature by identifying the effects of mergers on labor market concentration and its consequences on various labor market outcomes, both at the individual worker and market levels. As far as I know, my study is one of the first to address these concerns in Italy, adding to the ongoing debate on whether a more rigorous antitrust enforcement is necessary.

The paper is structured as follows: Section 2 provides an overview of the main features of the Italian labor market, while Section 3 deepens the relationship between monopsony and concentration. Section 4 describes the dataset and the evidence on labor market concentration, as well as its limitations. In Section 5, I discuss the baseline estimates, while Sections 5.3 and 5.4 present the identification strategy and the exogenous estimates, respectively. In Section 5.6, I explore the underlying mechanisms and various heterogeneities. I conclude with Section 6.

2 Institutional Framework

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

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

According to Garnero 2018, there are approximately 860 industry-wide collective agreements covering practically all private-sector employees in Italy. Trade union density, which is defined as the number of members over the total number of employees, is below 30% in the private sector, and employers organi-

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

zations' density is slightly lower than 50%. The author also finds that the minimum wages, established through *CCNL* and industry-specific, are relatively high compared to industry-specific medians and that, when computed with respect to regional median wages, they are higher in southern regions compared to northern ones. This suggests that firms would likely opt-out of collective agreements to reduce labor costs, especially in regions where real minimum wages are significantly high, such as in southern Italy.

Furthermore, several labor market reforms have been promulgated in the last few decades, reducing Employment Protection Legislation (EPL) on open-ended contracts to promote employment and firms' resilience. Figure B1 illustrates this pattern of de-regularization, which began with law No.108 approved in 1990, continued with the *Biagi* Reform in 2003[5], was followed by the *Fornero* Reform in 2012[6], and ended with the *Jobs Act* in 2015, which abolished the *Article 18*. This article of the *Statuto dei Lavoratori*[7] essentially prohibited firms from dismissing workers covered by an open-ended contract for economic reasons. The *Jobs Act* introduced the change for firms to dismiss workers for economic reasons in the Italian labor market.

To summarize, the reforms implemented resulted in an increase in the likelihood of economic dismissals and a decrease in costs, both in monetary terms and in terms of the probability of reinstatement. These changes were applicable 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 LLMs.

Furthermore, one of the potential source of monopsonistic power, is the presence of legal provisions limiting workers' mobility, such as non-compete agreements[8]. This source is relevant (Sarfati 2020; OECD 2020), particularly in the US labor market. However, as recently highlighted in Boeri, Garnero, and Luisetto 2023, in Italy about 16% of private sector employees are currently bound by a non-compete agreement: this corresponds to approximately 2 million employees. They are more common among highly educated and higher earning employees, but they are also relatively frequent among employees in manual and elementary occupations and low educated and lower earning ones.

The authors also find that the probability of being bound by a non-compete clause is negatively correlated with labor market concentration. They interpret this a sign that these agreements matter less in more concentrated local labour markets because there are already less competitors. Overall, in such a scenario, firms, especially the largest ones, could exert their market power over workers.

[5]To investigate this reform and its effect on the labor market, read Daruich, Di Addario, and Saggio 2023.

[6]To investigate this reform and its effect on the labor market, read Bottasso et al. 2023.

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

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

3 Monopsony and Concentration

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

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

Given this, there is no reason to unequivocally believe that estimating a wage elasticity with respect to labor concentration is appropriate to capture the effect of increasing labor power on workers[9]. However, Arnold 2021 highlights the importance of the source of variation of concentration. According to the model, mergers can be the source of such variation, and the results demonstrate this beyond a reasonable doubt. This is because mergers do not affect monopsony power through channels other than concentration.

Therefore, in this context, it can be inferred that there are no monopsony effects when there are zero changes in local labor market concentration[10]. Therefore, a merger that increases concentration raises firms' power, which in turn results in a wage loss. Arnold 2021, assuming that firms compete à la Cournot, derives the following equation[11]:

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

[9]Results are not attached, but both the endogenous estimates, as well as the exogenous obtained that I performed with a standard leave-one-out technique, yield no significant results, indicating that overall there is no evidence that concentration unequivocally depresses wages.

[10]This preposition implies that labor market competition is local-based. This is a relatively common assumption that, however, could be violated.

[11]Equation 1 is taken from 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 $= \sum_j (s_j)^2$ is the HHI based on employment shares, θ_m is the average value of the marginal product of labor, and γ_m is the fraction of the average marginal revenue product that goes to wages. This equation clearly implies that all else equal, the higher concentration, the lower the wage.

However, it is crucial to properly control for the market power effect to establish a clear causal relationship between mergers and the outcomes (wages and hires). In Section 5.3.2, I will elaborate on how I plan to achieve this. Therefore, while my theoretical framework remains the same, my empirical framework includes several modifications. Firstly, I focus on a labor market with distinct institutions. Secondly, I utilize alternative data sources and redefine labor markets. Finally, I adopt a new merger-based identification strategy, which additionally addresses under-explored policy concerns.

4 Data, Concentration Index and Evidence

4.1 Main Source

To calculate an index of concentration and to measure wages, I exploit a matched employer-employee dataset provided by INPS, *LoSaI*. *LoSaI* provides several datasets that contain information on all working spells, including wages, of a sample of workers and the firms in which they work from 1985 to 2018, of which we know the size class (classified in 14 brackets from 1-5 to over 500 employees) and the industry (2-digit ATECO) that can be associated to registry information of the same workers[12]. I select only new hires between 2005 and 2018, as I want to calculate a flows-based concentration index.

Moreover, both theoretical predictions and empirical evidence indicate that employers' power compresses entrants' wages rather than long-term incumbents, who are more experienced and protected by higher EPL (Bassanini, Batut, and Caroli 2023). New hires are defined as the spells activated for each individual in a given year in which the firm does not match the one for which the same individual worked in the previous year (Bassanini, Bovini, et al. 2022). I also exclude transformations, keeping only newly activated contracts. For each worker, I delete repeated observations within the same year keeping the longest spell (Macis and Schivardi 2016). I compute the main dependent variable, daily wages, dividing the total gross remuneration of each employment contract by the number of days actually worked as provided by *LoSaI*, thus ruling out the presence of any measurement errors.

The number of records with a value of 0 for wages is less than 50,000 and, as they likely represent a measurement error, they are dropped. The final sample is made up of approximately 3,600,00 newly

[12] I provide further details in Appendix 6.

activated employment contracts and 1,400,000 entrants[13].

4.2 Herfindahl-Hirschman Index

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

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

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

$$\text{HHI}_{m,t} = \sum_{N_{dm}} s_{d,t}^2 \tag{3}$$

, where N_{dm} represents the number of class sizes in each market m and s is the ratio of the number of new hires for the representative firm in class d in m in t to the total number of hires in m and t. The representative firm's hires for each size class are computed by dividing the number of hires for each year within that size class by the number of firms hiring in the same year within that size class.

The idea underlying the construction of this index is that firms within the same class size pay similar wages and that market concentration depends on the heterogeneity of hires across firms' sizes within it. The fact that larger firms or plants pay higher wages, and vice-versa, is widely documented in the US, in Europe (Ramaswamy and Rowthorn 1991; Brown and Medoff 1989; Idson and Oi 1999, Abowd, Kramarz, and Margolis 1999), and in Italy as well (Bertola and P. Garibaldi 2001; Mion and Naticchioni 2009).

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

4.3 Descriptive Evidence

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

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

Concentration I compute the HHI for approximately 6,000 LLMs. However, several markets have only one spell, which inevitably induces an upward bias in the HHI[14]. To address it, I deleted all those market-year tuples with one spell only. I finally obtained an almost perfectly-balanced panel of 47,727 market-year tuples regarding 5,008 markets in Italy between 2005 and 2018. I describe market concentration in Table 1 and Figure 1, while across industries and regions in Figures B2 and B3 in the Appendix. On average, concentration across Italian labor markets is mild: the median value is far lower than the standard threshold, indicating a medium level, and only a few markets are concentrated. However, the average value is approximately 0.14, indicating instead a medium level of concentration. Figure 1 indicates that the distribution is right-skewed: most of the markets are not concentrated, while a few are.

[14]With one spell only the index, for a mechanical bias induced by the HHI formula in Equation 2, is equal to 1.

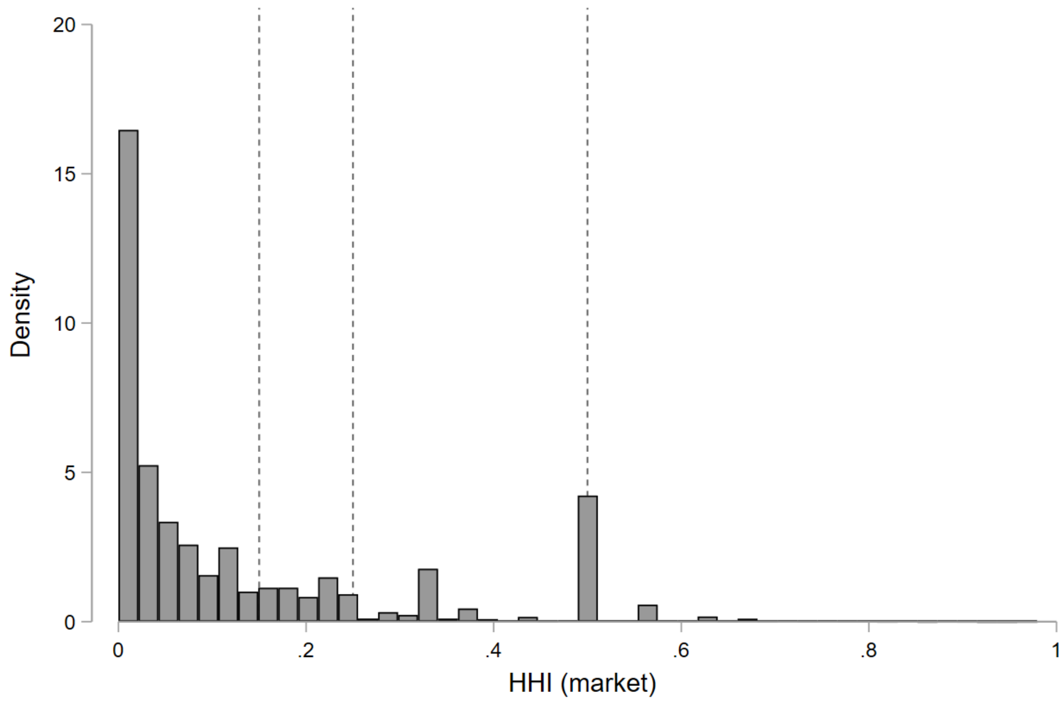

Figure 1: **HHI distribution across markets and years**.

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

Martins 2018 studies labor market concentration in Portugal finding that approximately 9% of Portuguese workers are subjected to a level of concentration that, according to the US antitrust agency, can be classified as medium. Moreover, as he relies on a stock-based index, he likely underestimates the true level of concentration across LLMs. In the Italian case, according to my estimates, the percentage is definitely lower, as the median value in the market distribution is 0.05 points.

I find that approximately 95,000 spells over more than 3,500,000 entrants' spells happen in markets with an HHI higher than 0.15. They represent approximately 3%, so definitely less than 9% in Portugal. This share is lower than expected according to various concerns and thus should not worry competition authorities and policy makers. Summing up, concentration in Italy is weak but it is heterogeneously distributed: most of the markets have low value while few are highly concentrated, driving the average value upward.

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

Source: Author's calculation on *LoSaI.*

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

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

When computing the measure across regions, industries and occupations separately, concentration increases, as displayed in Table 1 and Figure B2. Although the distributions tend to shift toward normality it emerges that concentration largely differs across industries. I believe that my findings are coherent with those of Fanfani 2022, who finds that industry heterogeneity in monopsonistic dynamics in the labor market explains a relevant portion of the gender wage gap. This suggests that monopsonistic dynamics are local and industry-driven. I take advantage of this fact to set up my identification strategy.

Role of financial turmoils Another concern is that concentration could vary over time, peaking during periods of recession, exacerbating in turn the damage that financial shocks can inflict on workers. In fact, there is evidence of this phenomenon, as financial turmoil can amplify labor market volatility (Boeri, Garibaldi, and Moen 2013; Autor, Dorn, and Hanson 2016) and shrink firms' access to credit markets, which in turn reduce hirings (Berton et al. 2018). However, my results point in a different direction. As proved by Figures B3 and B4 in Appendix 6, concentration does not change over time and even during the peak of the financial crisis[15] it does not differ from the whole period. Therefore, it does not appear that labor market concentration is an additional channel through which financial turmoil affects the labor market that, in turn, damages employment and workers.

My index of concentration, mainly due to data issues, suffers of several limitations that I discuss extensively in Section 6 in the Appendix. However, even though with limitations, I believe that it still paints a reliable picture of the levels of concentration and how they have changed across Italian LLMs over time. I now move on to introduce the empirical specifications.

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

5 Empirical Strategy

5.1 Wages Specification

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

$$log(Y_{i,m,dj,t}) = \delta_i + \mu_m + \gamma_s + \Gamma_{r,t} + \Lambda_{d,t} + \Phi_{o,t} + \beta_t + \theta log(HHI_{m,t}) + \Gamma Z_{i,t} + v_{i,m,dj,t} \tag{4}$$

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

The model is specified in a log-log form and hence θ should be interpreted as the elasticity of entrants' daily wages with respect to labor market concentration. Equation 4 is estimated with Ordinary Least Squares (OLS so on) with multiple fixed effects[16]. I exploit hence both cross-sectional and within-time variation in concentration to address its effect on workers' wages, controlling for a full set of time-varying covariates at a worker and market level as well as for market and worker fixed effects. I instead control for occupation-year, region-year, and size-year fixed effects to take into account potential time-varying confounding effects jointly influencing concentration and wages.

[16]The STATA command is that of Correia 2016.

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

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

Table 2: **Equation 4 estimates**.

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

Results, displayed in Table 2, indicate that the relationship between concentration and wages exists, but is weak. The sign switches when adding market fixed effects, suggesting indeed that time-invariant unobserved heterogeneity at a market level explains a considerable amount of variation of both wages and concentration. In the latest specification, the elasticity becomes negative, even though slightly significant. Furthermore, there might be a heterogeneous effect of concentration for contracts protected by different levels of EPL. Differences over time might be also due to variations in the composition of the workforce in terms of sex, nationality and age[17].

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

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

determinants of endogeneity in Section 6 in the Appendix[18].

5.2 Employment Specification

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

$$log(F_{m,t}) = \delta_m + \Phi_s + \gamma_{o,t} + \Theta_{r,t} + \beta_t + \theta log(HH_{m,t}) + \phi X_{m,t} + v_{m,t} \tag{5}$$

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

Table 3 shows that there is a negative and significant correlation between market concentration and employment flows: when (and where) concentration increases, hires diminish. The coefficients are similar in magnitude across all different specifications and are precisely estimated as the standard errors are very close and small. Estimates again suffer of endogeneity, arising from different mechanisms with respect to those of the wages specification. Due to the HHI formula, markets with higher spells tend mechanically to have lower levels of concentration, whereas the opposite holds for markets with fewer spells.

This mechanism induces a negative relationship between the two variables which biases towards zero the estimates. To address all these threats, I need to identify a shock affecting concentration but not the outcomes. This variation should rule out the joint effect of any labor demand and offer shocks at a market level influencing concentration and the outcomes contemporaneously. Furthermore, it should also be orthogonal with respect to the mechanism inducing a positive correlation between concentration and

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

wages across markets.

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

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

Table 3: **Equation 5 estimates**.

Notes: Employment is measured as the number of newly activated working spells within each market and year. Specification and controls are displayed in Equation 4. Clusters are 5,008. The sample is made of 47,727 market-year tuples, associated to 5,008 markets.

5.3 Concentration and Mergers

To obtain this exogenous variation, I rely on an instrumental variable strategy exploiting horizontal mergers. A wide body of literature has focused on mergers, but mostly in different fields of economics with respect to labor. Mergers have been widely discussed in the antitrust literature, while, regarding monopsony, there are several works finding that they could raise product market concentration[19]. However, growing evidence and concerns among competition authorities and policy makers in the US and Europe suggest that mergers also have consequences in the labor market (E. A. Posner and I. E. Marinescu 2020; I. Marinescu and Hovenkamp 2019; Manning 2003; Manning 2021; Suresh, E. Posner, and Wey 2018).

Mertens and Mottironi 2023 found a negative correlation between the markups of firms and their size within the same industry. To explain this evidence, the authors suggest that firms maintain low markups and increase their market shares in order to establish a dominant labor market position, which allows then

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

to shrink wages. This implies that even in the absence of standard product market spillovers, a horizontal merger could have adverse effects on workers. Empirical evidence supports this claim, as there are studies (Shapiro 2019; Dodini et al. 2023a; Arnold 2021; Guanziroli 2022) that show that horizontal mergers increase labor market concentration in both reduced-form estimates and in more structural analyses (I. Marinescu, Ouss, and Pape 2021; Jarosch, Nimczik, and Sorkin 2019). In Section 6 of the Appendix, I provide an extensive discussion of the literature. In short, I exploit this mechanism in my identification strategy.

5.3.1 Data on Mergers

I exploit the *Zephyr* database provided by the *Bureau Van Dijk* archive, which contains times series of worldwide rumoured, announced, and completed mergers and acquisitions operations of all types (partial or full acquisitions, mergers, etc.) from 1997 to nowadays. I select all completed mergers and acquisitions operations whose target country is Italy from 2005 to 2018. For a sub-sample of these events, I also have information on the number of workers involved, as well as the vendor and acquiror size. The final sample contains 5,932 events, associated to 4,237 different acquiror firms and approximately the same number of vendors[20]. On average, approximately 423 events happen per year. I provide further evidence in Appendix 6. I select only *horizontal mergers*, so those operations that occur between firms that operate in the same industry. As I define a market as a region-industry-occupation tuple, mergers between firms in different industries do not raise market concentration and thus are not relevant to set up the identification strategy. The final number of events in the entire analysis period decreases to approximately 200.

5.3.2 Identification Strategy

The underlying idea of the identification strategy is that those markets that experience mergers become more concentrated, both with respect to other markets and with respect to themselves over time. Concentration can vary depending on shocks coming through different channels[21]. An industry level shock (*e.g.*, two large firms merge in a industry at a national level) could raise concentration at a market level, as the shock would translate to different extents to all those LLMs associated to that industry. Furthermore, only workers belonging to the two merged firms would be directly hit, while all others would not.

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

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

The channel I exploit is a *national-industry level shock* in concentration. There is evidence that industry heterogeneity is a driver of monopsonistic dynamics in Italy (Fanfani 2022). There is also evidence that industries' heterogeneous exposure to horizontal mergers explains different increases in concentration and in turn the heterogeneous effects on wages (Jarosch, Nimczik, and Sorkin 2019; I. Marinescu, Ouss, and Pape 2021). This happens also in my data, as Figure 2 indicates that mergers are non-randomly distributed across industries and time but rather hit specific ones whose levels of concentration largely differ. Figure 2 indicates that the most hit industries are *Financial Activities, Information and IT Services Activities, Editorial Activities, Electric and Gas Furniture, and Satellite Telecommunication.*

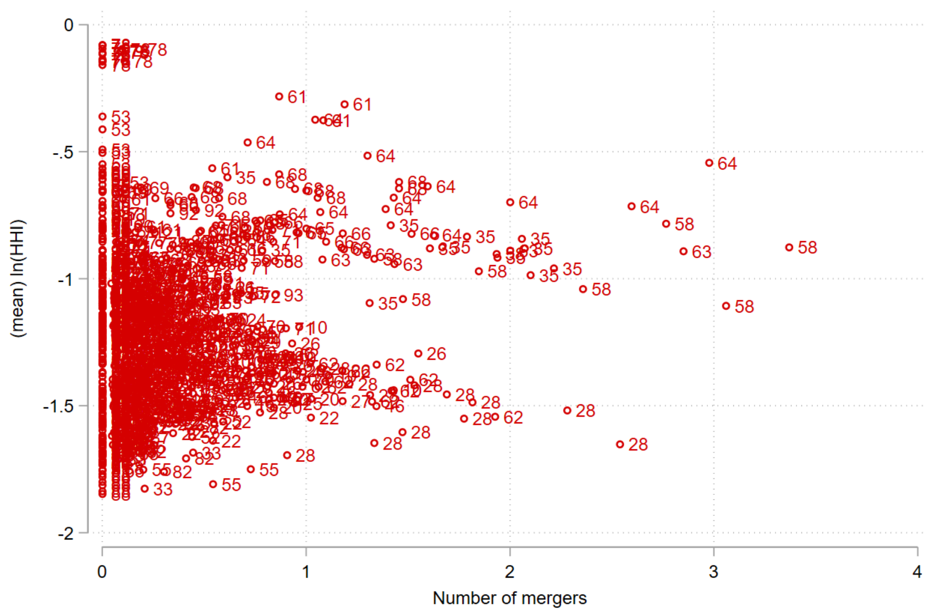

Figure 2: **Scatterplot between average (log of) concentration and the number of mergers, by industry-year tuples**.

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

I hence exploit the fact that the more an industry experiences mergers across time, the more it will become concentrated with respect to others industries that do not experience mergers but also to itself over time. To the extent to which I am able to control for any source of variation of concentration, mergers would hence represent a shock in concentration targeting only a sub-sample of markets, and consequentially workers belong to these markets, allowing me to set up a quasi-experimental framework.

The intuition behind the identification strategy is sketched in Figure 3.

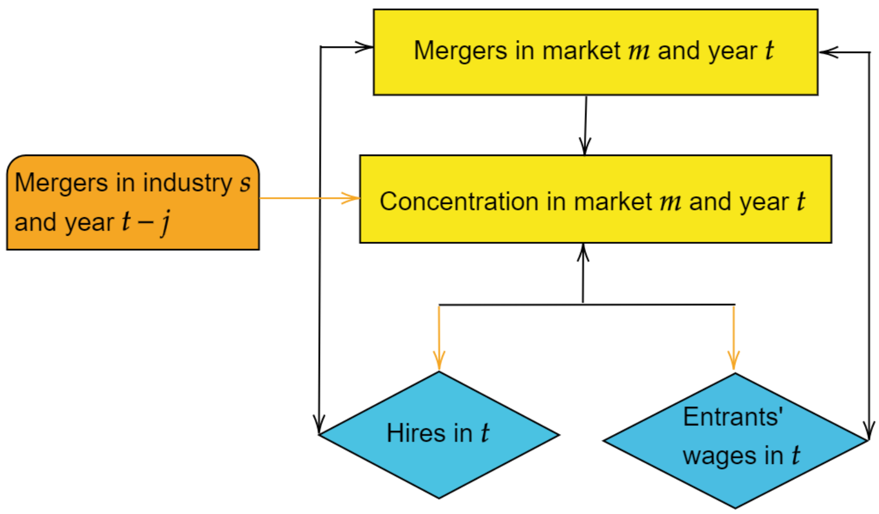

Figure 3: **Sketch of the Identification Strategy**.

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

Relying on mergers in the same market, I would not rule out the direct effect of mergers on wages and hires and the reverse causality triggered by the positive correlation between productivity and mergers within markets. Instead, an industry-national level shock affects concentration but has no direct effect on the outcomes of interest as income and employment dynamics depend on the region and on the occupation of each market. To illustrate this consider, for instance, a merger between two competitors at a national level, whose plants are located in one region of Italy. It reasonably does not directly influence the outcomes[22] of the workers employed by other competitors whose plants are mostly located in different regions. The same applies for hires[23].

Furthermore, I rely on lagged measures of mergers to (i) ensure exogeneity with respect to LLMs dynamics simultaneously influencing the realization of the mergers and the outcome; (ii) take into account that merged firms need some time to consolidate and display their increased labor market power. Figure B5 in Appendix 6 indicates that there is a positive relationship between market concentration and the lagged number of mergers in the same industry[24].

[22]Wages and job security.

[23]I discuss in detail all the feasible violations of the exclusion restriction in Section 5.3.3.

[24]The relationship is robust as it persists considering also seasonally-adjusted market HHI in Panel (b).

Instruments I define two binary instruments as:

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

$$\text{IV}_t^2\text{: } \forall \text{ t in [2005,2018], } \text{I}_t\{Mergers_{s,t-2} > 0\} = 1 \Rightarrow \text{HHI}_{m,t} \tag{7}$$

, where t-1 and t-2 indicate 1 and 2 lagged years and I is a dummy variable taking value 1 if an industry s experiences at least one merger in t-1 and t-2, 0 otherwise. In other words, I instrument ($\Rightarrow$ in my notation) concentration in each labor market and year (*i.e.*, $HHI_{m,t}$) with a dummy variable indicating whether the industry s identifying those markets have experienced at least a merger in the previous one or two years. I define a Wald Instrument[25] as:

$$\beta_{IV} = \frac{\operatorname{cov}\left(Y_{it}, IV_t^k\right)}{\operatorname{cov}\left(HHI_{i,m}, IV_t^k\right)} = \frac{E\left[Y_{it} \mid IV_t^k = 1\right] - E\left[Y_{it} \mid IV_t^k = 0\right]}{E\left[HHI_{i,m} \mid IV_t^k = 1\right] - E\left[HHI_{i,m} \mid IV_t^k = 0\right]}. \tag{9}$$

Substituting all the expected values with their corresponding averages in the sample, β_{IV} becomes $\widehat{\beta}_{\text{IV}}$ which should be hence interpreted as the difference in the average outcome Y (*i.e.*, log of daily wages; probability of being hired with an OEC; log of hires) across workers belonging to markets whose industry has experienced in the past one or two years at least one merger[26] divided by the difference in average HHI between treated and non-treated markets as predicted by IV_t^k with $k = \{1, 2\}$. What I estimate is hence an Average Treatment effect on the Treated (ATT), expressed as an elasticity or a semi-elasticity, as long as the identification strategy holds.

My empirical strategy is similar to those of Guanziroli 2022 and Arnold 2021 when they compare in a DD framework the outcomes in markets/workers that are "treated" (*i.e.*, that are exposed to mergers) with those that are not. Essentially, that is also what the Wald instrument of Equation 9 does with the only difference that it weights the difference in the outcome between the two groups by the change in the endogenous covariate predicated by the instrument. Estimates can be interpreted as causal effects as long as the standard conditions of the instrumental variable regression are met, namely the absence of a direct effect of the instruments on the outcomes of interest (*i.e.*, exogeneity), and the correlation with the endogenous covariate (*i.e.*, relevance). I discuss these conditions in the next section.

[25] Formally, a generic Wald instrument (Wald and Wolfowitz 1940) is defined as follows:

$$\widehat{\beta}_{\text{Wald}} = \frac{(\bar{y}_1 - \bar{y}_0)}{(\bar{x}_1 - \bar{x}_0)} \tag{8}$$

, where the subscript $_1$ indicates the treated group, while $_0$ the control group. The $\widehat{\beta}$ in Equation 9 estimates the difference in average outcome across the two groups divided by the difference in average concentration across the two groups.

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

5.3.3 Instruments properties

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

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

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

Similarly, in my framework, I rely on a national and industry-level measure of mergers exposure, except for the small population of workers directly targeted by the merger under examination[27]. The exclusion restriction would be violated if mergers directly affect wages through productivity gains. My objective is to isolate the monopsony power effect while controlling for the potential bias posed by productivity gains hence. Arnold 2021 decomposes the average treatment effect of a merger on wages into three components - monopsony power, product market power, and productivity gains - as follows:

$$E\left[\tilde{w}_j(1) - \tilde{w}_j(0)\right] = \underbrace{E\left[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0)\right]}_{\text{monopsony effect}} + \underbrace{E\left[\tilde{\mu}_j(1) - \tilde{\mu}_j(0)\right]}_{\text{market power effect}} + \underbrace{E\left[\tilde{\psi}_j(1) - \tilde{\psi}_j(0)\right]}_{\text{productivity effect}} {}^{28}. \tag{10}$$

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

[28]The equation comes from Arnold 2021, Section 2.2., page 6.

My aim is to isolate the former effect, while the latter is a threat. Clearly, only mergers that affect concentration and productivity simultaneously are cause for concern. However, I believe that the mechanism highlighted in Equation 10 is not a threat in my framework. Productivity gains pertain solely to the merged firms, whereas my instruments assign the treatment to the industry year in which the merger occurs. In this case, the effect on productivity and, in turn, on wages is concentrated in the firms directly involved in the merger. Additionally, the estimator in Equation 9 compares the outcomes in the treated and control groups, with the bias being the difference in wages between the treated and control groups induced by the productivity gains. Even if the bias were relevant, and I believe this is not the case, the inclusion of year and industry fixed effects is likely to remove it ensuring exogeneity.

Exogeneity is supported by the correlation results presented in Table A4, which shows the correlations between the three measures of individuals' merger exposure and their wages across the full sample. Furthermore, Figure B6 in Appendix 6 plots the quadratic relationship between entrants' daily wages and the number of mergers at an industry level with a two-year lag. Both correlations are very small and negligible, and no relationships of any kind appear between the two, suggesting that there is no direct relationship between the IVs and the main outcome of interest.

In addition, as demonstrated in Figure B6 in the Appendix, my measures of mergers exhibit clear persistence over time. The correlations among the three measures of merger exposure are all significant. This indicates that I can use lagged measures of mergers to predict upward shifts in concentration at time t, while also ruling out concerns regarding reverse causality and simultaneous determination of the outcomes and the covariate of interest. A valid concern is that mergers may target markets that are already highly concentrated, leading to an overestimation of the effect of concentration on the outcomes of interest. However, as shown in Figure B5 in the Appendix, mergers occur across markets with varying levels of concentration, thereby dispelling the concern that they only target markets that are already highly concentrated.

Another concern is that mergers often result in a decrease in employment, which may lead to a downward bias in estimating the effect of concentration on employment flows. However, in the employment specification described in Equation 5, I model new hires as the number of newly activated employment contracts in each market-year tuple, without taking layoffs into account, as layoffs affect employment levels rather than employment flows. Therefore, these dynamics should not affect the identification strategy. On the other hand, it's possible that following a layoff, merged firms hire more employees to rebuild their workforce, which may induce an upward bias in the estimates of new hires. That's why I use lagged measures of mergers.

Likewise, it's possible that bigger and more efficient companies may raise their employment levels. To account for variations in wages and hiring practices across firms of different sizes, I incorporated class size-year fixed effects into all regression analyses[29]. Additionally, mergers have nothing to do with the mechanical bias inducing higher concentration in markets with fewer spells. The identification is thus robust with respect to this bias.

To conclude, I perform a robustness exercise to test whether exogeneity holds. I compute the standardized differences for different moments, *i.e.,* mean, median, and standard deviation, of daily wages[30] between treated and untreated industry-year tuples[31]. Results are displayed in Table A5 in the Appendix. The estimated differences for mean and median daily wages always lay within the standard bandwidths, indicating that they are not statistically significant. It means that mean and median daily wages do not differ significantly between treated and not industries. Regarding the standard deviation instead, the difference is almost significant in Panel (a), and slightly significant in Panel (b). This indicates that the instruments affect the distribution of wages within industries but do not affect their levels, thus strengthening the exogeneity assumption. I interpret this evidence as proof of the fact that the IVs do not directly affect the outcomes.

Relevance Relevance implies that the instruments must be strongly correlated with the endogenous covariate. First-stage estimates, displayed in Figure B7 in the Appendix, show positive and always significant coefficients, both when the instruments are considered separately and when jointly. The IVs additionally satisfy the rule-of-thumb check: All F statistics are far greater than the standard threshold of ten (Stock and Yogo 2005). Instruments, although correlated, capture different sources of variation in concentration, as the third model shows that when they are considered jointly, they both remain significant and sizeable.

Results indicate that the coefficients associated with the IVs, indexed by the letter j, range between 0.123 and 0.211 points. As the outcome is a log, coefficients are semi-elasticities, which means that the IVs predict an increase in concentration that ranges between $e^{\beta_j}=(2.718^{0.123}-1)*100=13$ p.p. and $e^{\beta_j}=(2.718^{0.211}-1)*100=23$ p.p.. Those of the two instruments specifications together indicate instead that workers belonging to treated markets on average experience an increase in concentration of 30-38 p.p. with respect to those belonging to non-treated markets. The coefficients do not differ significantly

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

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

[31] 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 5 and 6, I compute the standardized differences for the three moments of the main outcome between treated and untreated tuples.

from those estimated in a DD specification in Arnold 2021. With additional controls, as he manages to include industry-commuting zones-year fixed effects, he finds that the treatment effect ranges between 0.175 and 0.239 points, which translates into an increase in concentration for treated markets from 19 to 27 p.p. with respect to controls.

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

5.4 IVs Estimates

Wages I display results for the three different specifications: in Panel (a) I rely on the instrument defined in Equation 6, in (b) I rely on the instrument defined in Equation 5, while in (c) I use both. Errors are clustered at a market-year level, addressing the correlation between workers and spells hit by the same shock in a given market and year. The results are shown in Table 4 and indicate that concentration has a sizeable negative impact on entrants' wages. The estimates are larger in magnitude as the different confounders, which are positively correlated with respect to both concentration and wages, induce a downward bias in the endogenous estimates[32].

Estimates magnitude and significance differ little across specifications, but the instrument of Equation 6 seems to be the most relevant. Estimates range between -0.14 and -0.068 p.p., while the preferred ones, those clearly significant, between -0.14 and -0.09. It follows that a 10 p.p. increase in market concentration reduces new hires' wages by approximately 0.9-1.4 p.p.. Estimates differ from those of the literature: I. Marinescu, Ouss, and Pape 2021 reduced-form elasticities range between -0.067 and -0.052 points, which indicate a reduction in wages following a 10% increase in market HHI of 0.67 and 0.52 p.p..

[32]This suggests that the instruments predict an exogenous variation in concentration.

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

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

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

Notes: Observations are 3,573,677 yearly spells between 2005 and 2018. The estimates are formally displayed in Equation 8. Panel indicate different instruments: (a) 2-years lagged mergers as in Equation 6; (b) 1-year lagged mergers as in Equation 5 and (c) both jointly. *Notes*: observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and market fixed effects.

My estimates are in line with those obtained simulating an *horizontal merger* between two top-employing firms raising concentration by 10 points in I. Marinescu, Ouss, and Pape 2021 as they find a reduction in new-firm wage bill of approximately 7 p.p.. Arnold 2021 also finds elasticities that range between -0.3 and -0.2 p.p.. Jarosch, Nimczik, and Sorkin 2019 find elasticities for wages ranging between -0.18 and -0.09 p.p. in reduced-form estimates, while, simulating a horizontal merger shifting a market from average to high concentration (*i.e.*, from the 25^{th} to the 75^{th} percentile in HHI distribution), they find that wages decrease by 1 p.p..

These values are higher than those on average estimated in the literature and more similar to mine. In summary, my estimates stand in the middle between those obtained with the leave-one-out IV (I. Marinescu, Ouss, and Pape 2021; Azkarate-Askasua and Zerecero 2023; Dodini et al. 2023a; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022) and those obtained instead relying on mergers in reduced-form estimates (Arnold 2021; Guanziroli 2022), structural models (Jarosch, Nimczik, and Sorkin 2019), and simulations (I. Marinescu, Ouss, and Pape 2021).

Employment The exogenous estimates are larger in magnitude than the endogenous ones as the different confounders, which are positively correlated with both concentration and hires, induce a downward bias. Results are shown in Table 5 and indicate that the estimates are stable across panels, with elasticities ranging between -0.68 and -0.77 p.p.. Estimates are slightly greater than that estimated by I. Marinescu, Ouss, and Pape 2021, that range between -0.31 and -0.585 p.p..

This indicates that a standard 10 points increase in HHI would reduce hires by approximately 3-6%. The distance in magnitude might be due to the different framework[33], but most likely it is due to the different identification strategy or the definition of new hires[34]. The more conservative definition of new hires in my framework could explain the higher magnitude of my estimates. Results indicate that when a market shifts from low to high concentration hires reduce by 7-8 p.p..

[33]French labor market is similar but not identical to the Italian one for many reasons, in particular regarding firms' size distribution. For a detailed discussion on this topic, as well as on the heterogeneous effect on labor market outcomes in the two countries, read Berson, Philippis, and Viviano 2020.

[34]They define new hires as those whose employment spell starts in each quarter, deleting those observations whose job spells start on January 1^{st} for each year. I have additionally deleted all transformations keeping only new activations and all observations for an individual that was working in the same firm the previous year.

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

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

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

Notes: Employment is measured as the number of newly activated working spells within each market and year. Full sample is made of 47,727 market-year tuples. Markets are 5,008. The estimates are formally displayed in Equation 8. Panel indicate different instruments: (a) 2-years lagged mergers as in Equation 6; (b) 1-year lagged mergers as in Equation 5 and (c) both jointly.

5.4.1 Extensive and Intensive Margin

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

Throughout the paper, I referred to these two channels as *extensive* and *intensive* margins. I aim to open the black box of concentration effect on wages in order to shed light on which channels monopsonistic power goes through and how they differ in magnitude, an exercise only slightly performed in the literature[35]. In order to answer these questions, I estimate the following equation on two separate outcomes, namely the number of worked days and overall remuneration:

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

, where i indexes workers, r regions, o occupations, j firms, d class sizes, s industries, and t years. O indicates an outcome. The model is still specified in a log-log form. θ should thus be interpreted as the elasticity of the outcome with respect to labor market concentration. I present the results only for the two IVs specifications, that of Panel (a) of Table 4, with the full set of controls displayed in Equation 9[36]. Errors are still clustered at the market-year level.

Results are displayed in Figure B8 in the Appendix. The estimated elasticity for days is not significantly different from zero (=0.001, SE=0.049), while for overall remuneration it is slightly significant and equal to -0.12 (SE=0.059). They suggest that employers exert their monopsonistic power only through one channel, the extensive margin one. The intensive margin, instead, is not affected at all by concentration. It seems thus that employers in more concentrated markets simply can, and thus do, reduce the wages of entrants' workers. They are able to do it presumably due to the high search and matching costs and frictions that characterize Italian LLMs, which prevent workers from easily switching jobs and markets, and due to the few skills acquired by workers over time[37]. I now move on to perform a robustness exercise.

Robustness check So far, I clustered the standard errors within markets and years. The motivation

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

[36]Results across different specifications and with a different set of controls are not attached to the paper to avoid redundancy. However, they have been performed and are consistent with those presented.

[37]I have already discussed the causes in Section 1. However, for a more extensive discussion, I redirect to *e.g.,* Sarfati 2020; OECD 2020 and Manning 2021.

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

5.5 Job Security

Wages are not the only worker level outcome affected by concentration. Amodio, Medina, and Morlacco 2022 find in Perù that the higher concentration, the higher the rate of self employment. Lamadon, Mogstad, and Setzler 2022 find that monopsonistic labor markets increase workers' misallocation to firms, while Dodini et al. 2023b find that workers exposed to higher levels of concentration have substantially worse subsequent labor market outcomes. There is evidence of an analogous shift in the Italian context regarding two labor market reforms reducing EPL[38] (Ardito et al. 2022, Bottasso et al. 2023). To the extent to which an EPL reduction can be potentially considered an increase in employers' power, a similar mechanism might apply to concentration as well.

There is instead a dimension of worker's welfare that could be affected by employers' power that is not captured at all by the wage, namely job security. In a highly dual labor market such as the Italian one, where the costs in terms of uncertainty, firing possibilities, and monetary compensation for workers are definitely lower for fixed-term with respect to open-ended contract[39], firms can overcome workers along different dimensions. They might, for instance, decide to hire but on a temporary basis to secure their possibilities in the near future to dismiss these workers with little or no costs at all[40].

Avoiding looking at the type of contracts that are activated among the new hires would thus result in overlooking potentially a relevant mechanism. This exercise can be seen as a piece of a broader picture portraying the effects of employers' power. This is implied by the definition of labor market monopsony,

[38] The two reforms are respectively the 2015 *Jobs Act* and the 2012 *Fornero* Reform.

[39] For more details read Section 2.

[40] So far I have only considered the entrants' side of this topic: I do not consider for instance the possibility of incumbents' worker to switch from an open-ended to a fixed-term contract, which can be possible.

that is a situation in which employers, in order to maximize profits, decide to reduce the use or the remuneration, or both, of labor. In this simple framework, hires represent the extensive margin, wages the intensive.

Practically, I test whether concentration affects workers' likelihood to be hired with open-ended contracts. It could be the case that, in markets characterized by higher concentration, firms employ more workers with FT contracts[41] with respect to OEC. Bassanini, Bovini, et al. 2022 find no effect in Italy and Spain on this outcome, while they find a small and negative one in France and Germany. Qiu and Sojourner 2022 find a similar effect in the US as well[42]. I estimate the following equation:

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

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

and i denotes the worker, r the region, o the occupation, j the firm, d the class size, s the industry, and t the year. P is a dummy variable taking value equal to 1 if worker i in region r, with occupation o, in firm j of class size d and industry s in year t is hired with an OEC, 0 otherwise.

Matrix Z contains worker level covariates, as a cubic polynomial in age and spells length to proxy individuals' specific on-the-job working experience. $u_{i,m,dj,t}$ is an error term, clustered at a market-year level. The equation is specified in a linear-log form, and thus θ should be interpreted as a semi-elasticity of the probability of being hired permanently and concentration. Model is estimated with OLS, thus as a Linear Probability Model[43].

Estimates are displayed in Figure B9 in Appendix 6, obtained relying on the two instruments' specification with the full set of controls displayed in Equation 10[44]. Overall, I find a null effect on the probability of being hired on a permanent basis, coherently with Bassanini, Bovini, et al. 2022. I find a semi-elasticity of 0.0044 (SE=0.0315; t-statistic=0.14; p-value=0.888), with standard errors clustered at a market level. Estimates remain not significant even clustering at a market-year level (SE=0.0224; t-statistic=0.2; p-value=0.842). Overall, my results indicate that concentration does not affect the probability of being hired on a permanent basis.

In the Italian labor market employers' power does not seem to damage workers along this dimension.

[41] Among the fixed-term contracts I also include apprenticeships.

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

[43] I do not rely on a MLE method, such as the logit or the probit, as it is hard to include a large set of fixed effects.

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

It thus seems that employers, when they have power and the chance to exert it, simply do not hire some workers rather than hire them on a temporary basis. This is likely due to the fact that in Italy most workers are anyway hired at the beginning with fixed-term contracts[45], as firms want to have the chance to evaluate workers and keep them only when they prove their value. Therefore, there is little room to increase the probability of hirings on a temporary basis[46]. I now move on to investigate the heterogeneity.

5.6 Heterogeneity

Sex Dodini et al. 2023a and Manning 2021 find that monopsony dynamics explain gender wage gap dynamics in the UK and Norwegian labor markets. Sulis 2011 and Fanfani 2022 find similar evidence in Italy[47]. I can test the same predictions in my setting, hence exploring whether, and to what extent, merger-induced shocks in concentration hurt differently the wages of men and women. I additionally deepen whether they affect job security of men and women differently. To my knowledge, I am the first to explore this potential dimension of merger spillovers. The estimates are shown in Figures B11 and B10 in Appendix 6.

Wages. Estimates are significant for women only, not for men. This is very interesting as the joint estimates, displayed in Table 4, are significant regardless of the cluster choice. The significance of the estimates is driven by women. Men are not affected by concentration, as the coefficient remains not significant even clustering the standard errors within markets and years. Although not statistically significant, men's coefficient drives the magnitude of the baseline one upward. The elasticity of women is approximately -0.036 p.p. (p-value=0.012, standard errors clustered at a market level) which is in line with that estimated in the literature[48]. This finding explains the greater magnitude of the baseline results of Section 5.4 that I have previously attributed to the different identification strategies.

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

[45] Among the fixed-term I include all types of contracts that are not open-ended.

[46] This does not mean, however, that concentration in the labor market does not affect job security. It might indeed be the case that higher employers' power reduces the likelihood of conversion. I aim to test this hypothesis in the near future by focusing on incumbent workers.

[47] The former estimate a set of labor supply elasticities across different groups of individuals, while the latter builds a theoretical model and then tests its predictions.

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

Concentration levels I split concentration into 4 brackets: from 0 to .15 in HHI, from .15 to .25, from .25 to .5, and above .5, which indicate respectively a weak, medium, high and very high concentrated market according to the US antitrust recommendations. I aim to test whether concentration shocks, induced by mergers, have a higher detrimental effect on workers' wages in markets that start from different levels of concentration. The same mechanism could be in place for the probability of being hired on a permanent basis as well.

Wages. Results are displayed in Figures B12 and B13 in Appendix 6. Mergers-induced concentration shocks do have different effects depending on the pre-treatment level of concentration. However, the effect is well identified for women only, while even in high-concentrated markets men do not seem to be affected by concentration, although estimates show an increasing trend. This is surprising and sheds light on the mechanisms driving gender wage gap dynamics in the Italian labor market.

Overall, shifting from a weakly to medium and then to a highly concentrated market, the elasticity becomes twice (-0.073 vs -0.038) and four times (-0.122 vs -0.038) as negative. Estimates remain always significant. These findings are relevant along different dimensions: first, they indicate a striking difference in how monopsonistic dynamics affect individuals by gender, with several policy implications, and, second, the baseline estimates displayed in Table 4 are to some extent biased as they hide a relevant heterogeneity driving estimates magnitude and significance.

Job security. Estimates are displayed in Figure B14 in Appendix 6. There is no effect at all for men, although the coefficients display an increasing trend, while it is relevant and monotonically increasing for women. The estimated semi-elasticity for women is always significant and equal to -0.03 p.p. in weakly concentrated markets, -0.078 in medium concentrated markets, -0.093 in high-concentrated markets, and -0.272 in very high-concentrated ones. Magnitude hence triplicates shifting from the bottom to the top of concentration distribution. These results highlight one simple but very powerful fact: concentration damages job security for women only, with an intensity depending on the starting levels of concentration of the market. What matters thus are not only the variations but also the levels of concentration.

6 Conclusions

In this paper, I investigate whether monopsonistic dynamics are present in Italian labor markets by analyzing labor market concentration and its effects on new hires' wages, job security, and employment. On average, I find that the level of concentration in Italian labor markets is weaker than expected, with a median of 0.05 points and a mean of 0.14. This suggests that the majority of labor markets are weakly concentrated, while only a small proportion are highly concentrated. Specifically, approximately only 3%

of new hires occur in markets with high enough levels of concentration to raise concerns for competition authorities. I also find that the level of concentration in Italian labor markets does not vary significantly over time, indicating that financial crises do not have an additional negative impact on workers' welfare through this channel.

I find that concentration increases when it is calculated across industries, suggesting that industry heterogeneity, as also found by Fanfani 2022, gives rise to monopsonistic dynamics. To address endogeneity, I exploit this mechanism to implement a novel IV strategy based on horizontal mergers, in spirit of Arnold 2021 and Guanziroli 2022. I use lagged measures of mergers happening within the same industries to instrument for changes in concentration. This approach exploits a source of variation in markets' exposure to concentration that is exogenous to the determinants of wages and employment outcomes, additionally answering to policy concerns.

The instrumental variables, both individually and jointly, explain a significant amount of variation in market concentration over time. This has a negative effect on wages and employment, with the effect only going through the intensive margin. The estimated elasticities range between -0.14 and -0.09 percentage points for daily wages and between -0.77 and -0.68 percentage points for hires. In a standard simulation where a market with an average level of concentration becomes 10 points more concentrated, wages would decrease by 0.9-1.4 percentage points, and hires would decrease by 7-8 over the following two years. This implies a loss of 9-19 euros per month, or 108–205 euros per year for a full-time worker with an average wage. I find no effect on job security.

However, the estimates obtained hide relevant heterogeneity in the data. Specifically, the elasticity of wages is precisely estimated only for women and equals -0.036 p.p., while for men, although bigger in magnitude, it is not statistically significant. A similar pattern is observed for job security, with estimates becoming increasingly larger in magnitude as the level of concentration increases from low to medium to high. In particular, the elasticities for both wages and job security triplicate and quadruple as the level of concentration increases from low to high. This suggests that the levels of concentration, and not just the shocks, are crucial in identifying the most problematic labor markets.

Future research could explore additional channels through which monopsony might affect workers' welfare. One such channel could be the impact of concentration on workers' human capital, which could be examined by investigating how concentration affects job content and the tasks performed on the job (Bachmann, Demir, and Frings 2022). Additionally, it would be worth investigating further the dimension of job security, for instance addressing the relationship between concentration and the spread of precarious employment forms such as Atypical Work Arrangements (Datta, Giupponi, and Machin 2019).

This study's policy implications suggest that competition authorities should be attentive to labor market spillovers resulting from mergers, in addition to the well-known product market ones. Policy makers should assess mergers on an industry-specific basis, taking into account the targeted industries and their concentration levels. Based on these criteria, the author identifies five industries - *Financial Activities, Information and IT Services Activities, Editorial Activities, Electric and Gas Furniture*, and *Satellite Telecommunication* - as the riskiest. I thus believe, consistent with recent developments in the US, that a stronger enforcement of antitrust laws, particularly in these specific industries, might be necessary in Italy as well.

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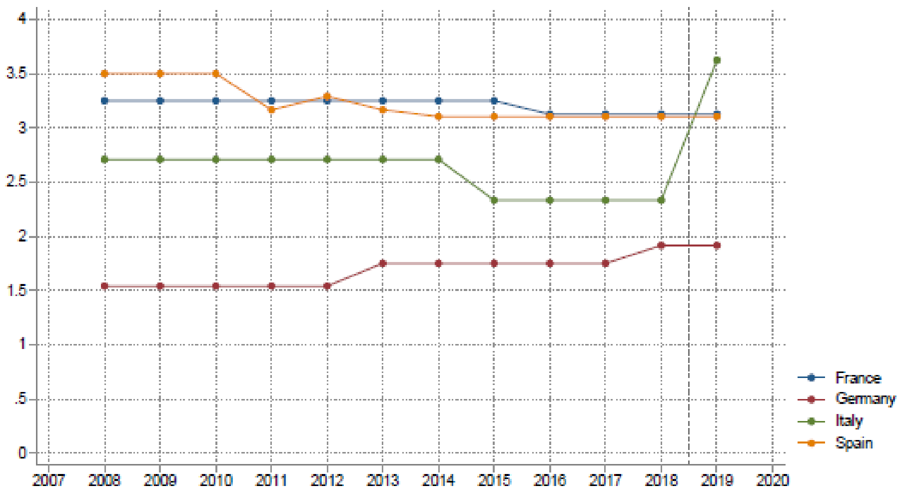
Contents

Appendix

A Institutional framework - Additional material

(a) Restrictions on temporary contracts



(b) Protection for permanent contracts

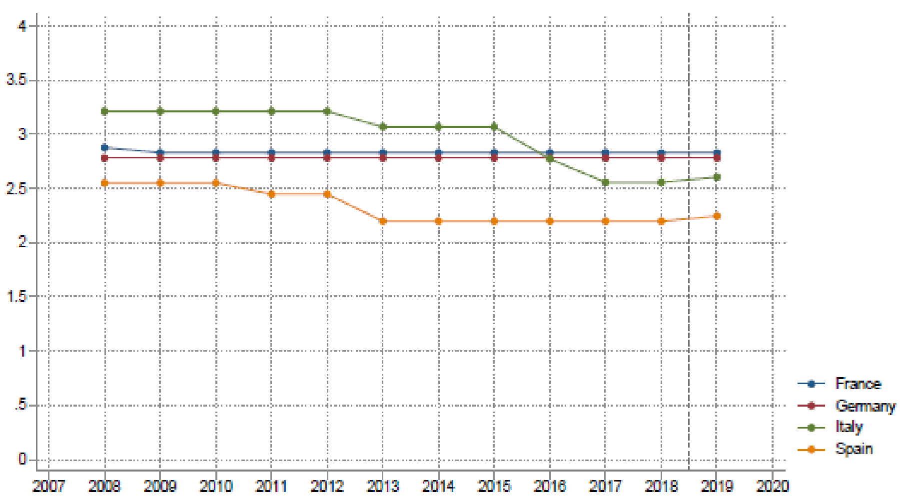


Figure B1: **Strictness of EPL on FT and OE contracts in Italy, Spain, France, and Germany between 2007 and 2020.**

Source: **palladino** on OECD data.

C LoSaI

To get an employer-employee dataset, I can use *LoSaI*. It contains several datasets, extracted from the *INPS* administrative archive. The first provides a random set of individuals working spells with many information such as gross remuneration, date (d/m/y) of start/end of the spell, type of contract, linked firm to the spell and other standard information from 1990 to 2018. The spells recorded are all those associated to a random sample of individuals born in days 1 and 9 of any month and year from 1990 to 2018, representative of the Italian working population. The second dataset provides instead registry information regarding the same workers - including the region of residence - which can be linked to the first through a unique code. In the last dataset, I obtain firms' information regarding class size and industry (ATECO 2007, 2 digits) ranging from 1990 to 2018.

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

C.1 Additional Descriptive Statistics

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

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

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

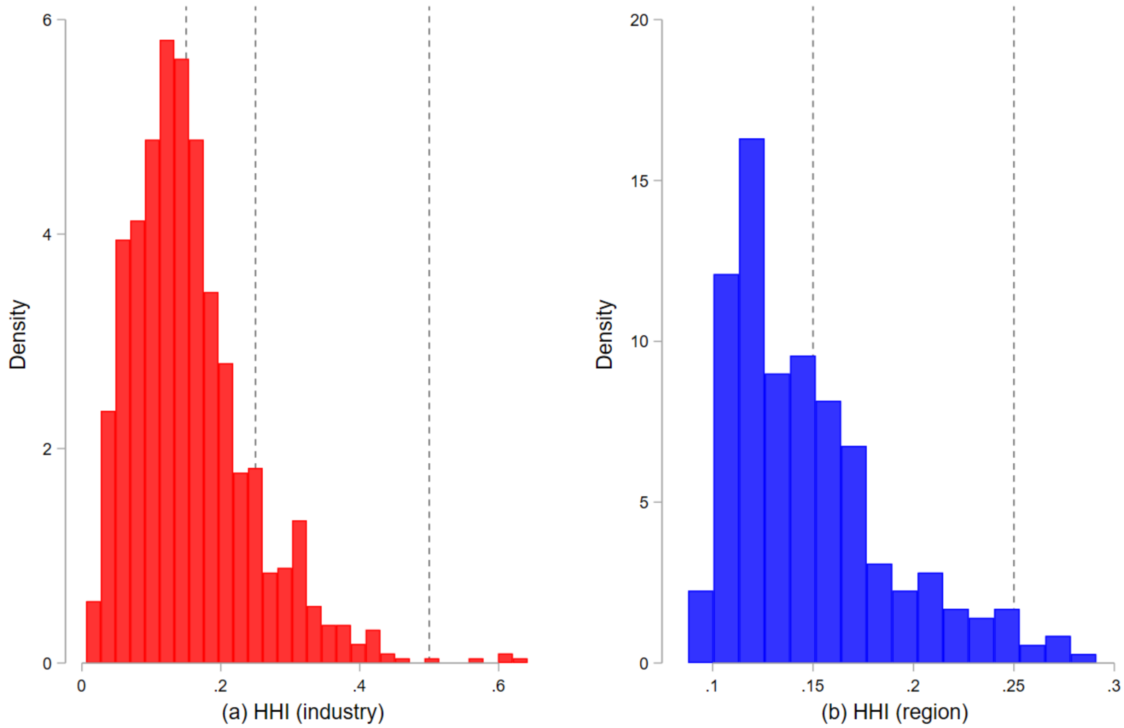


Figure B2: **Distributions of concentration across industries and regions in Italy**.

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

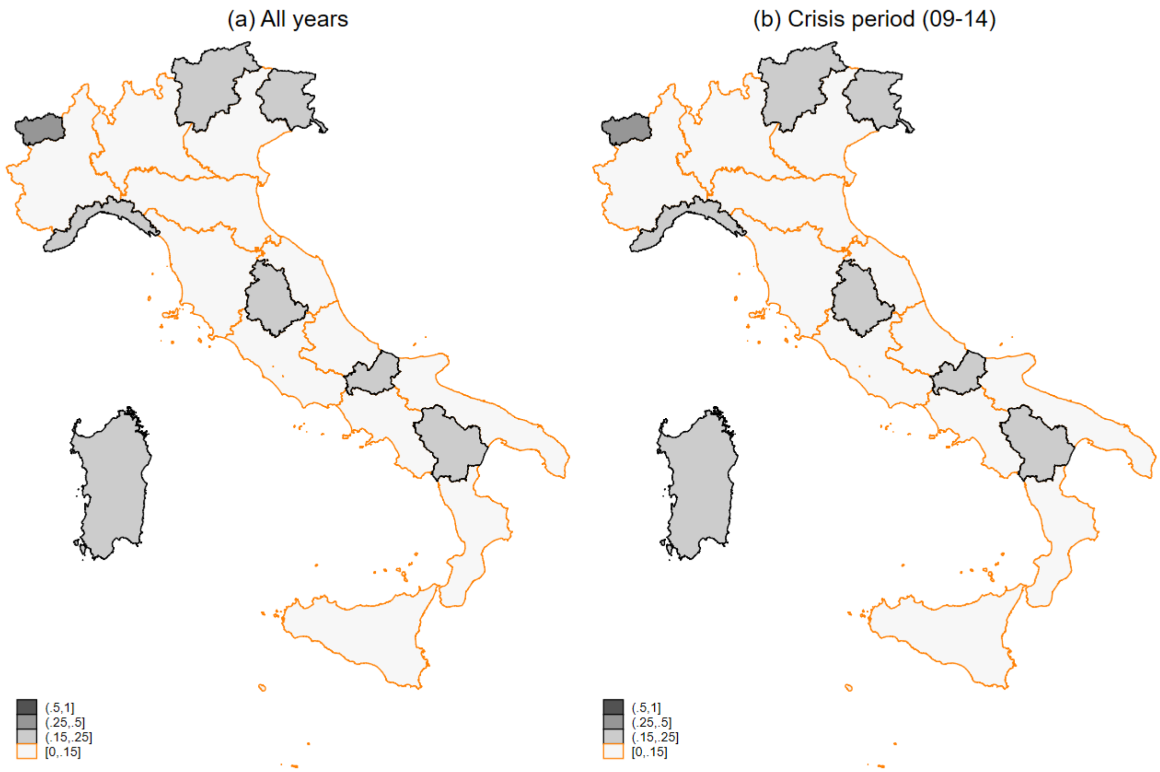


Figure B3: **Map of Italian regions by levels of HHI.**

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

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

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

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

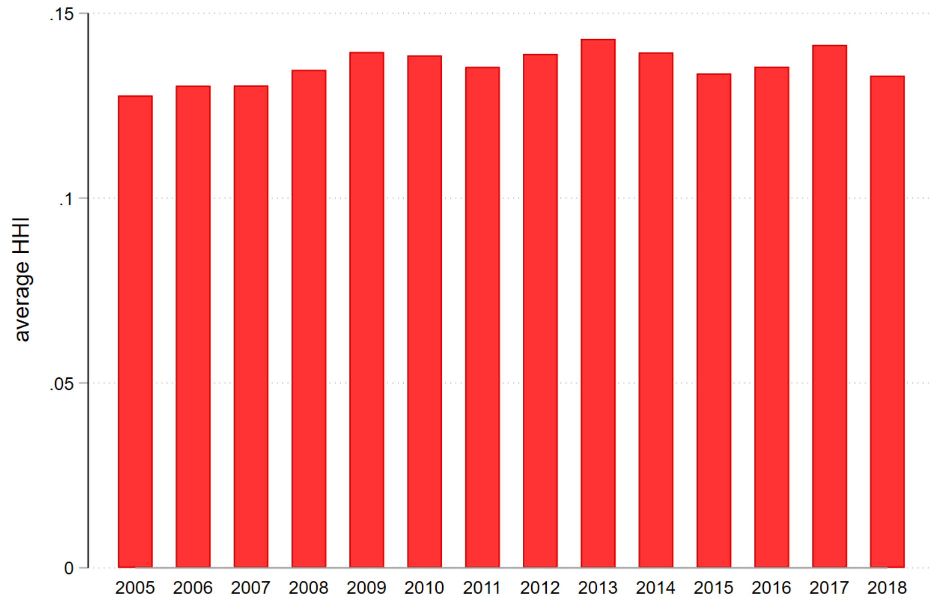


Figure B4: **Average HHI across labor markets, by year, from 2005 to 2018.**

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

C.2 Index limitations

Before proceeding with the empirical strategy and the estimates, it is important to discuss a few limitations of the concentration measure. Although I do not consider these limitations to be too restrictive, there are three noteworthy points. Firstly, I calculate the index within a year, which Azar 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 numerosity of workers in each cell. I believe that the latter bias would have a greater impact than the former. It is worth noting that Azar et al. 2020's observation applies to the US labor market, where mobility is significantly higher than in the Italian labor market, and thus the bias is less pronounced.

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

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

Finally, it is important to note that the data used in this study only represent a sample of workers, not the entire population. This may introduce an upward bias in the estimates, as not all new hires are captured. To address this, I dropped cells where only one hire occurred, although this adjustment may not completely eliminate the bias[49]. As a result, my index of concentration is likely inflated. Therefore, any interpretation of the levels of concentration should be taken with caution. However, the limitations discussed here do not affect the identification strategy, the comparison with labor markets in Portugal, the role of financial shocks, or most of the descriptive analysis. In conclusion, while there are limitations in the estimated levels of concentration, the identification strategy and the take away drawn from the descriptive analysis are valid.

D Empirical strategy: Weaknesses and Limitations

I estimate the models including a full set of fixed effects and controls at a worker and market level, both time-varying and not. Year fixed effects capture macro shocks, homogeneous across regions, industries and occupations, happening at a national level and possibly influencing wages and firms' hires dynamics, such as workers' out-of-work benefits which are set at a national level, macroeconomic fluctuations and trend effects. Occupation-year, size-year, and region-year fixed effects capture instead specific time-varying dynamics across regions - capturing local specific employment dynamics -, firms' size - capturing yearly specific productivity trends for firms of the same size class - and occupations. However, industry-specific time trends, firms' productivity, and market tightness shocks raise concerns about the robustness of Equation 3.

I am already controlling for market, occupation-year and region-year fixed effects but not for industry-

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

year. This means that whether during the period of analysis a yearly-industry-specific shock affecting wages occurs, estimates would be biased. Including firms' fixed effects would solve the former, but as described in the introduction *LoSaI* is not representative at a firm level. *LoSaI* is instead representative across firms' size classes, and hence I control for size-year FEs. However, the presence of firm-specific characteristics correlated to the outcomes of interest - such as productivity, human capital, employers' attitude and others factors explaining wages heterogeneity - would bias the estimates (Abowd, Kramarz, and Margolis 1999). The proper and easiest way to rule them out is to control for firm fixed effects (*e.g.,* Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022; I. Marinescu, Ouss, and Pape 2021). However, my dataset does not allow to see the employment history of all firms and hence I cannot control for firm fixed effects.

However, to the extent that these characteristics are in common among firms belonging to the same markets, then the inclusions of market fixed effects rules out the bias, otherwise is would be a bias in my specifications. I control for both market and region-year fixed effects as proxies. Ideally, I should build more detailed measure of labor market concentration at a geographical level (*e.g.*, I. Marinescu, Ouss, and Pape 2021; Bassanini, Batut, and Caroli 2023; Bassanini, Bovini, et al. 2022). Commuting Zones are the preferred choice as they precisely take into account local employment dynamics, especially in country like Italy which is characterized by a dense presence of the so-called "distretti industriali"[50]. However, I have no access to further information beyond the region and hence I cannot improve the specification.

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

There might be other confounding effects. Industry-year shocks influencing simultaneously concentration and wages or trade shocks (*e.g.*, china trade shock) targeting specific industries in specific point in time influencing human capital, productivity or revenues. This would bias the estimates as I do not

[50]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.

control for industry-year fixed effects. A mass layoff occurring in a given market certainly would increase concentration, but at the same time also has a direct and significant effect on wages and hires. Ideally, I should control for market-year fixed effects, ruling out the presence of all kinds of confounding effects at this level. However, in the literature market-year fixed effects are never included as collinearity likely arises with respect to the remaining set of FEs, resulting in inflated standard errors.

Moreover, there's an additional relationship between wages and concentration: on one hand, everything else equal, higher wages attract more workers and therefore increase markets' concentration. On the other hand, if there is labor market power on the employer side, I expect two workers with the same characteristics to be paid differently depending on the specific local labor market concentration. These two mechanisms cancel out and their interaction does play a relevant role in terms of the magnitude of the bias, as the endogenous estimates contained in the empirical literature are bounded to zero with respect to those exogenous[51]. The specification in Equation 4 additionally suffers of reverse causality because of the mechanical relationship that assigns higher concentration to markets with fewer spells. This bias is inevitable as long as the outcome is measured as a flow. The opposite instead holds for markets with more spells. I again expect the exogenous estimates to be larger in absolute terms because not constrained towards zero.

E Mergers and Concentration: Additional materials

E.1 Extendend Literature

E. A. Posner and I. E. Marinescu 2020 discuss extensively the need for a more intense antitrust regulation, focusing on the US, in order to prevent the birth and the growth of monopsonistic dynamics the in labor market. They explicitly mention mergers and acquisitions as a potential trigger for monopsonistic dynamics, especially when combined with relevant labor markets frictions and anti-competitive behaviors, *e.g.,* non-poaching and non-competitive agreements[52]. I. Marinescu and Hovenkamp 2019 discuss the role played by M&A's in the Labor Market, highlighting the dangers that growing concentration caused by mergers can cause for workers' wages and employment. In fact, they exhort authorities to take into evaluation labor markets spillovers when they evaluate mergers besides those on prices and markups.

Shapiro 2019 also argues that antitrust law should be enforced. There is indeed convincing evidence that larger, more efficient firms have been growing at the expense of their smaller, less efficient rivals, causing industry concentration in the US economy to increase. He adds that the fundamental challenge

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

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

for merger control is that it is a predictive exercise: seeking to identify the subset of mergers that "may substantially lessen competition," one must assesses the likely competitive effects of a proposed merger *before* it is consummated.

Jarosch, Nimczik, and Sorkin 2019 simulate the merger between two largest employers in each labor market with Austrian firm level and stock data and re-compute wages at all employers. On average, wages at merging firms decline by seven percent. Mergers have large spillovers also on other workers, whose wages decreased by 3%. Their model also implies non-linear effects of concentration on wages: large effects are estimated in already highly concentrated markets. From the 25^{th} to the 75^{th} percentile of the concentration distribution, such a merger would depress wages by about 1 p.p..

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

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

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

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

E.2 Zephyr Archive

The Bureau Van Djik is the worldwide leader providing all sorts of information regarding business and industries, across the world. It also has information on an unrivalled number of deals, stored in the *Zephyr* database. *Zephyr* covers over ten years of history for deals around the world and an even longer history for deals with a European counterpart. It also has information on rumours, as well as announced and completed deals, from the end of the '90 to Nowadays. It covers all types of deals, from standard M&A's to joint ventures, de-localization or closures. The full database contains more than a billion records. Headline, type, status, value and details of the target, acquirer and vendor including country and activities plus regulatory bodies are contained in the database, as well as information regarding target, acquiror and vendor employment volume.

In Table A3 we provide the list of the most targeted industries and the corresponding 2digit code according to the ATECO 2007 classification.

Label	**Code (2digit)**
Financial Activities	64
Information and IT Services Activities	63
Editorial Activities	58
Electric and Gas Furniture	35
Satellite Telecommunication	61

Table A3: **ATECO 2007 2digit code and corresponding label of the most targeted industries.**

F Instrumental Variable Regression - Additional Material

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

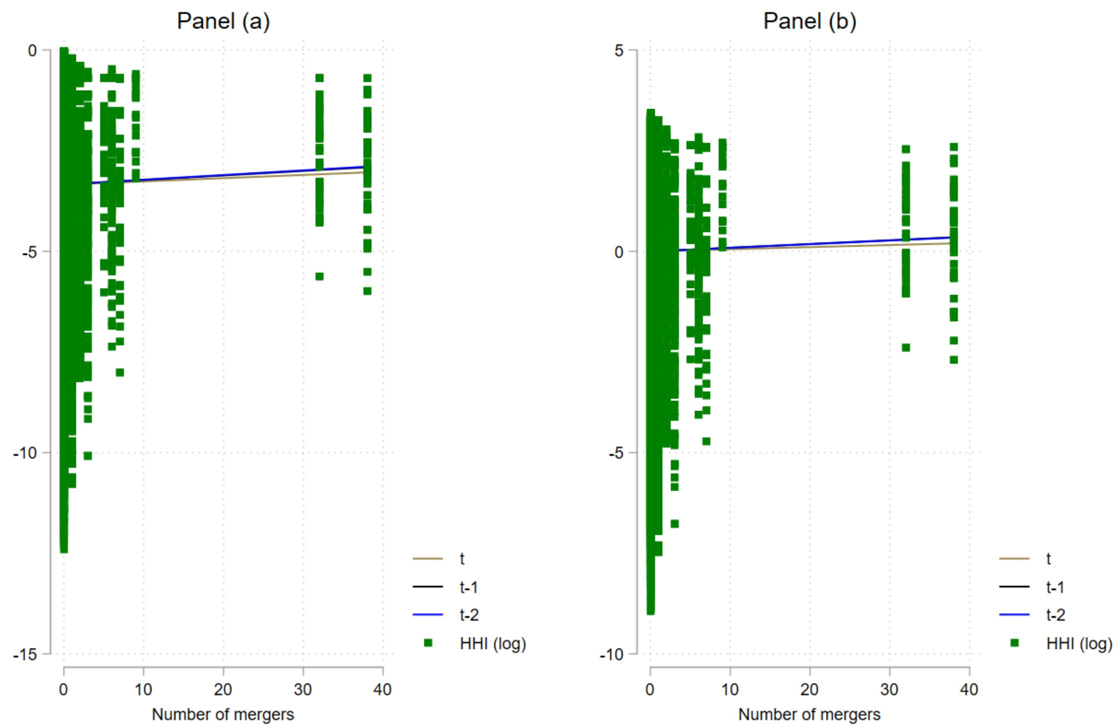


Figure B5: **Scatterplot of HHI and seasonally-adjusted HHI (both in log) with respect to the mergers in the same industry and year**.

Notes: Panel (a) contains market HHI's as calculated in Equation 2 while Panel (b) contains the seasonally adjusted market HHI's - obtained subtracting the yearly means to the HHI's - to rule out time trends. Lines represent the predicted values obtained through a regression of log of concentration w.r.t current, one-year and two-years lagged mergers. Mergers event are approximately 200 events in the period of analysis. t-1 and t-2 indicate, respectively, the number of merger events that occurred in the previous and in the previous two years for each market-year tuple considered. Observations are 47,727 market-year tuples associated to 5,008 markets between 2005 and 2018.

F.1 Threats to Identification - Additional Material

In this subsection, I attach the results of different exercises that I perform to prove that the Exclusion Restriction assumption holds in my empirical strategy. The table and the figure are cited in Section 5.3.3 in the main text.

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

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

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

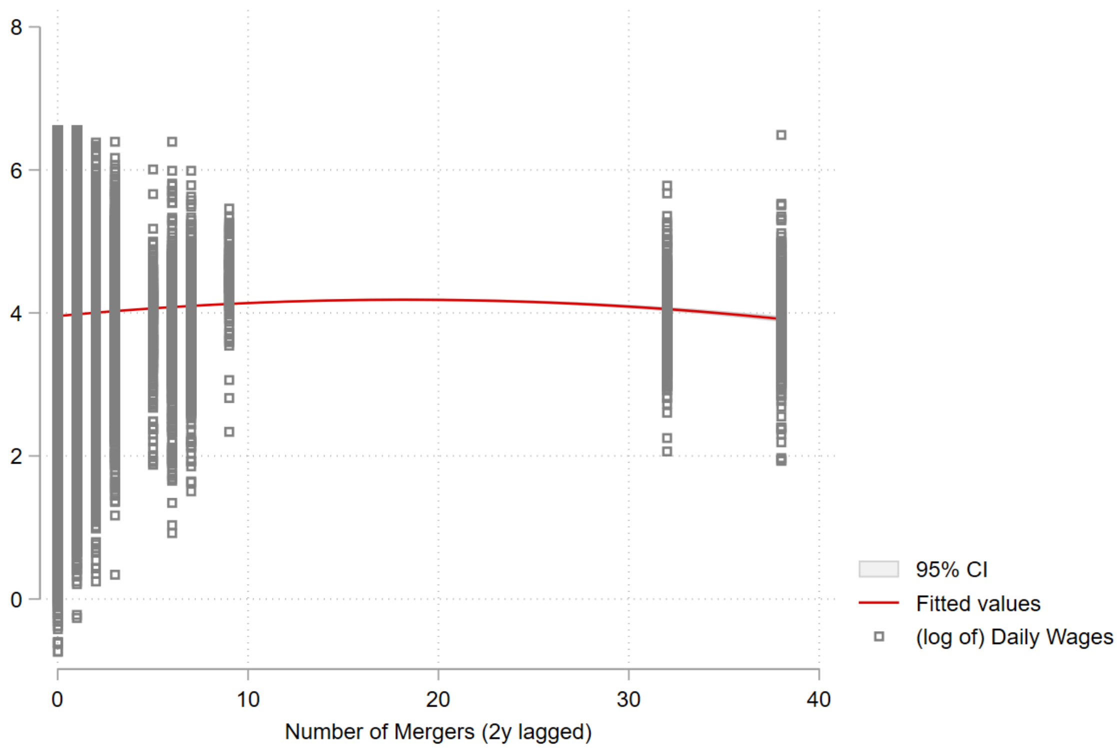


Figure B6: **Scatterplot of the (quadratic) relationship between Daily Wages and 2-years lagged mergers**.

Notes: Daily Wages are on the y-axis, while the number of mergers on the x-axis. The red line represents the fitted regression line, with the 95% confidence bandwidths, obtained through a quadratic regression of log of daily wages on two-years lagged mergers. t-2 indicates the number of merger events that occurred in the previous two years for each market-year tuple. Events are approximately 200 in the period of analysis. Observations are 3,573,677 market-year tuples associated to 5,008 markets between 2005 and 2018.

Panel (a): IV_t^2	Not Treated		Treated		
	Mean	St. Dev.	Mean	St. Dev.	Std Diff
Mean	4.1	.29887	4.11	.25463	−0.03682
Median	4.131	.27277	4.15	.20988	−0.07726
St.Dev.	.5259	.10451	.5073	.069363	0.20957*

Panel (b): IV_t^1	Not Treated		Treated		
	Mean	St. Dev.	Mean	St. Dev	Std Diff
Mean	4.1	.29495	4.103	.24318	−0.01161
Median	4.129	.26837	4.131	.20984	−0.00566
St.Dev.	.5218	.10425	.4988	.069976	0.25905*

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

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

F.2 First-stage estimates

In this Section, I display the results of the First-stage estimates of the instrumental variables regression. Controls are displayed and commented in Equation 4 and Table 3. I only present the results with the market specifications controls and not with worker fixed effects only as in Table 2. Coefficients are always positive and significant across all specifications, both when considered separately and jointly.

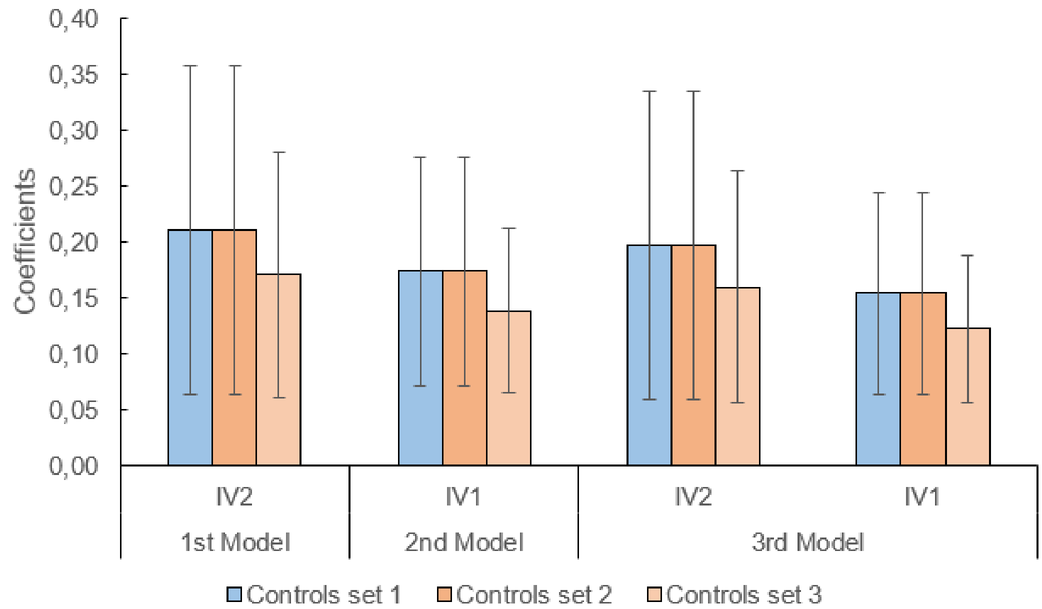


Figure B7: **IV First-stage estimates of Equation 4.**

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

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

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

Table A6: **IV fist stage estimates of Equation 4.**

Notes: Panel indicate 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 4 and are displayed in Table 3. Errors are clustered at a market level.

F.3 Robustness check - clusters choice

To test the robustness of my estimates, I now allow the observations to be correlated within the same market over time, clustering the standard errors at a market level. The reason why I do not cluster at an industry level is that industry is just one of the levels defining a market. Clustering at an industry level could be too conservative, in light of the fact that each industry further segments into regions and occupations, and that I exploit a somewhat raw classification of the industry[53].

Consider for instance the logistic industry (2-digit ATECO cell number 52), which presumably shows different concentration levels across regions, especially when a merger targets firms in a specific region. The observations that are affected are those that compete in that industry and region, and definitely not those firms operating in the same industry but eventually far away. Thus, the shock I aim to capture influences only some of the markets associated with that industry and hence the most adequate choice to cluster observations is within industries and not markets.

Results for wages hold across the different specifications. In the preferred one, including time-varying control for occupations, firms' sizes and regions and considering the two instruments, Column (4) of Panel (c) in Table 4, the t-statistic is equal to -2.49 and the p-value to 0.013[54]. Considering instead the preferred instrument alone (Column (4) of Panel (b) in Table 4), the t-statistic and the p-value become -2.52 and 0.012. Moving to the specification of Column (3), results for the two IVs specification are again significant (t-statistic=-2.08 and -1.9; p-value=0.038 and 0.057).

G Additional Figures

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

[53] *LoSaI* contains indeed only a 2-digit ATECO grid, which contains approximately 76 cells.

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

G.1 Intensive and Extensive Margin

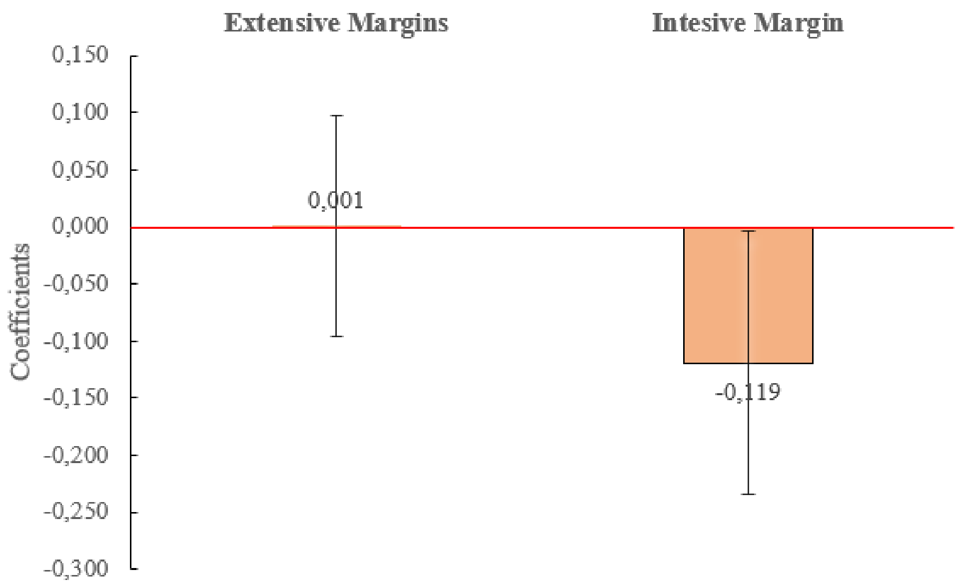


Figure B8: **IV estimates of Equation 11.**

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

G.2 Job Security

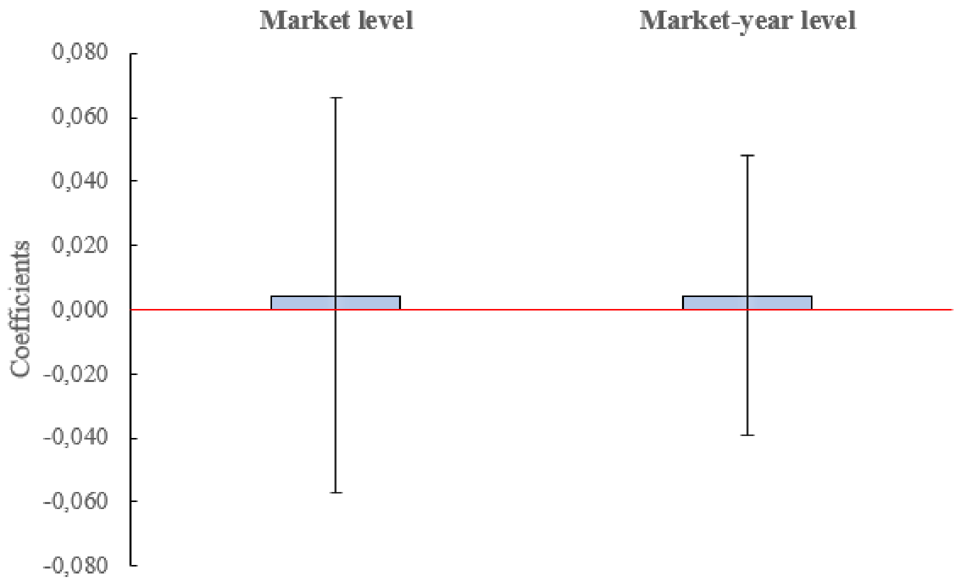


Figure B9: **IV estimates of Equation 12, clustering at a market and market-year level.**

Notes: The estimates are formally displayed in Equation 8. The two panels indicate a different clusterization level, on the left at a market level and on the right at a market-year level. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are 3,573,677 yearly spells between 2005 and 2018. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

G.3 Heterogeneity: Sex

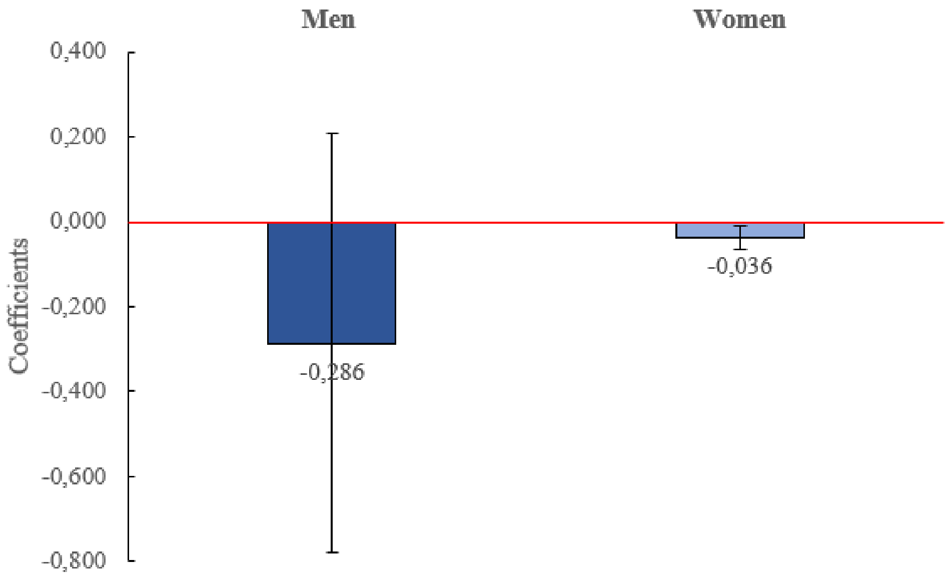


Figure B10: **IV estimates of Equation 4, by sex.**

Notes: The estimates are formally displayed in Equation 8. Estimates are obtained relying on two IVs. Errors are clustered a market level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

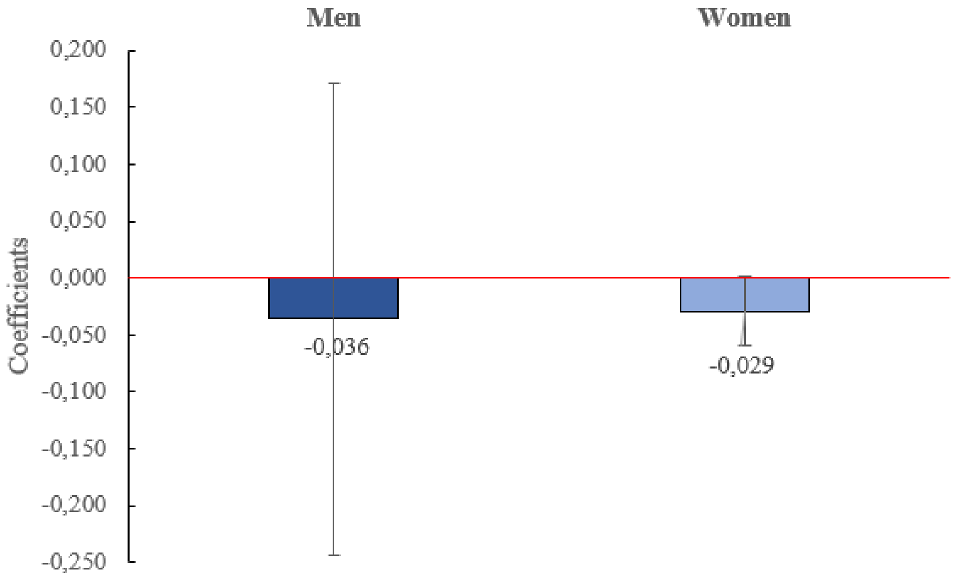


Figure B11: **IV estimates of Equation 12, by Sex.**

Notes: The estimates are formally displayed in Equation 8. Estimates are obtained relying on two IVs. Errors are clustered a market level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

G.4 Heterogeneity: Concentration levels

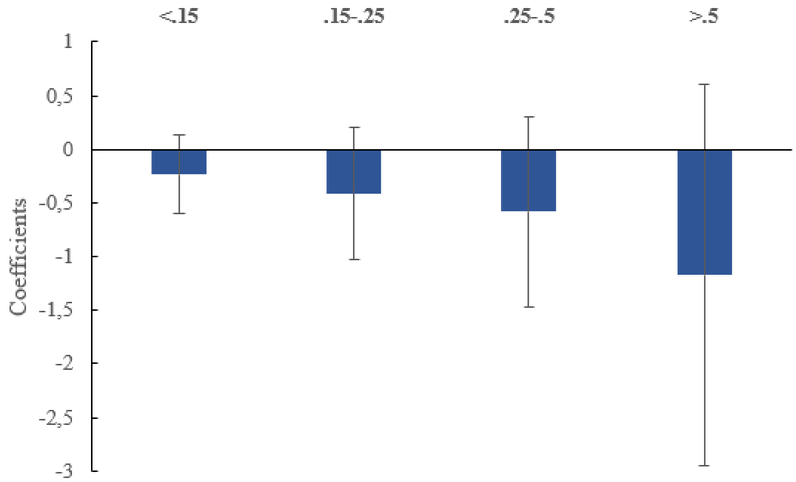


Figure B12: **IV estimates of Equation 4, for men.**

Notes: Concentration is divided in the standard bandwidths defined by the US Antitrust agency: <.15 indicate a weakly concentrated market, .15-.25 a medium concentrated market, .25-.5 a highly concentrated market while above .5 a very highly concentrated market. The estimates are formally displayed in Equation 8. Estimates are obtained relying on two IVs. Errors are clustered a market level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

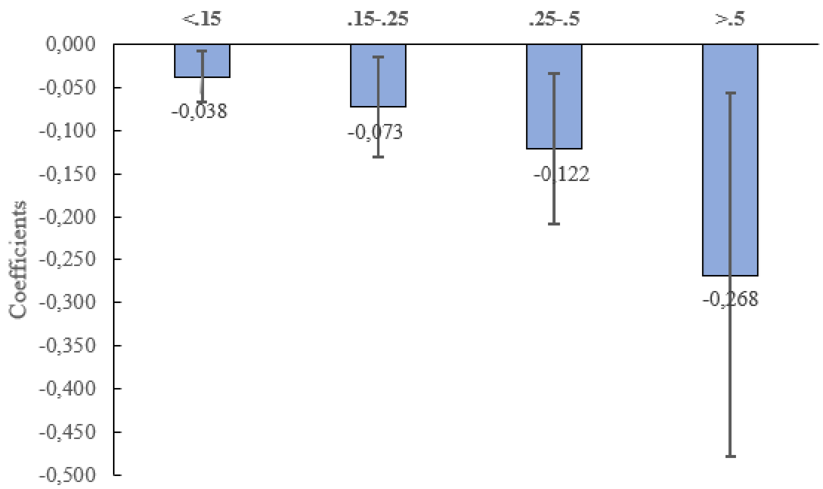


Figure B13: **IV estimates of Equation 4, by concentration levels, for women.**

Notes: Concentration is divided in the standard bandwidths defined by the US Antitrust agency: <.15 indicate a weakly concentrated market, .15-.25 a medium concentrated market, .25-.5 a highly concentrated market while above .5 a very highly concentrated market. The estimates are formally displayed in Equation 8. Estimates are obtained relying on two IVs. Errors are clustered a market level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

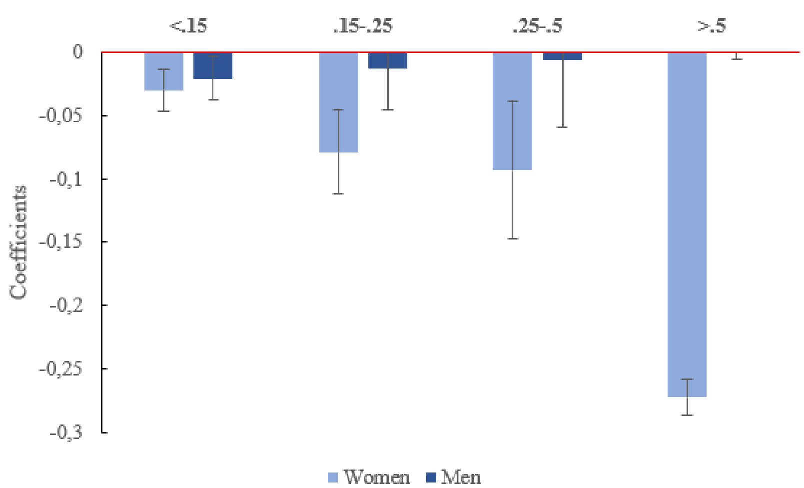


Figure B14: **IV estimates of Equation 12, by sex and concentration levels.**

Notes: Concentration is divided in the standard bandwidths defined by the US Antitrust agency: <.15 indicate a weakly concentrated markets, .15-.25 a medium concentrated market, .25-.5 a highly concentrated market while above .5 a very highly concentrated market. The estimates are formally displayed in Equation 8. Errors are clustered a market level. Observations are 3,573,677 yearly spells between 2005 and 2018. Results are obtained with the two IVs specification with the full set of control, so market, individual, industry, occupation-year, region-year, and size-year fixed effects. Observations are lower than in the full sample and differ across specifications because singletons are iteratively dropped when including worker and markets fixed effects.

Do Alternative Work Arrangements Substitute Standard Employment? Evidence from Worker-Level Data*

Bernardo Fanfani† Filippo Passerini‡

March 30, 2023

Abstract

This paper uses Italian administrative data to examine the impact of an Alternative Work Arrangement (AWA) called the "voucher" on income trajectories. Specifically, we investigate whether this form of work substitutes income from standard labor contracts and welfare transfers. We estimate cross-income elasticities using a fixed effects specification and an estimator that controls for the endogenous selection of individuals in the labor market selection. Results show that the vouchers increase overall labor income and substitute standard contracts. However, when selection is taken into account, the positive effect on total labor income is smaller, while the substitution effect becomes more pronounced. We also conduct an event study that takes into account an individual threshold on voucher earnings. We find that when the threshold is reached, voucher income decreases and so does overall income, while welfare transfers and labor income remain unaffected. Overall, our findings suggest that AWAs tend to substitute standard employment, with small positive net effects on earnings. The benefits are more significant for intensive users and those more likely to fall into the informal labor market.

Keywords: Alternative work arrangements, labor supply, cross-income elasticities, sample selection, event study.

JEL codes: J24; J22; D12; C13; C21.

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1 Introduction

A well-developed literature has analyzed the effects of fixed-term employment contracts on various labor market outcomes[1]. Italy, in this context, can be considered an interesting case study, as it transitioned from a rather rigid legislation towards a dual labor contract system during the late 1990s and early 2000s[2]. More recently, new forms of precarious and atypical employment have emerged in Europe and the U.S. besides fixed-term employment contracts. Following Mas and Pallais 2020, these new forms of employment are broadly defined as "Alternative Work Arrangements" (AWAs). AWAs may include workers hired by a temporary employment agency, independent contractors with multiple or single clients, or workers with an irregular or flexible schedule. The emergence of AWAs has been linked to firms' demand for increasingly flexible tasks and their relatively low cost. AWAs are typically characterized by low bureaucracy and almost null firing costs, and they are usually more widespread in low-wage segments of the labor market (Dolado, Lalé, and Turon 2021).

AWAs offer several advantages to firms, as they enable them to adjust to labor demand fluctuations in a quick and efficient manner (Chan 2018; Chen et al. 2019). Additionally, some studies have suggested that these arrangements may also benefit workers in weaker segments of the labor market, by reducing the duration of unemployment and facilitating their transition to more stable employment (Farber 1999; J.T. and C.J. 2006; Addison and Surfield 2007). However, the use of AWAs may also result in reduced worker welfare, particularly if employers exploit them to avoid sanctions related to undeclared work (Di Porto et al. 2022) or to coerce workers into less protected forms of employment when they hold considerable bargaining power (Dolado, Lalé, and Turon 2021; Datta, Giupponi, and Machin 2019; Boeri and Garibaldi 2007).

In our study, we focus on a particular form of AWAs that was introduced in the Italian labor market between 2008 and 2017, the *labor vouchers* or simply *vouchers* (Porelli 2006). With this type of arrangement, employers were allowed to purchase a given number of 10 euros vouchers from INPS (the Italian Social Security Agency) up to a yearly cap. These vouchers could then be used to pay workers without a standard employment contract. Workers, on the other hand, could redeem the vouchers for 75% of their face value, with the remaining 25% covering the cost of social insurance. Labor vouchers were intended for occasional activities involving irregular working tasks with no fixed schedule.

Compared to other types of labor contracts in Italy, vouchers involved considerably simplified bu-

[1]See Blanchard and Landier 2002; Boeri and Garibaldi 2007, Bentolila et al. 2012; Cahuc, Charlot, and Malherbet 2016; Hijzen, Mondauto, and Scarpetta 2017

[2]See Berton and Garibaldi 2012; Berton, Richiardi, and Sacchi 2009; Cappellari, Dell'Aringa, and Leonardi 2012; Berton, Devicienti, and Pacelli 2007; Berton, Devicienti, and Pacelli 2011; Ardito, Berton, and Pacelli 2019; Rizzica 2015, Daruich, Di Addario, and Saggio 2023)

reaucracy, which was expected to encourage employers to reduce the use of undeclared work (Tiraboschi 2010; Minenti and Winkler 2010). When they were first introduced in 2008, there were limitations on the activities for which they could be used and which workers could use them. However, in the following years, they were significantly liberalized, and their use continuously increased. Figure 1, which describes the market size covered by vouchers over time, indicates a clear growth trend following this liberalization process. Due to a widespread opposition from trade unions, they were finally abolished in 2017. However, plans to reintroduce them have been recently proposed, including in some drafts of the 2023 Italian financial maneuver.

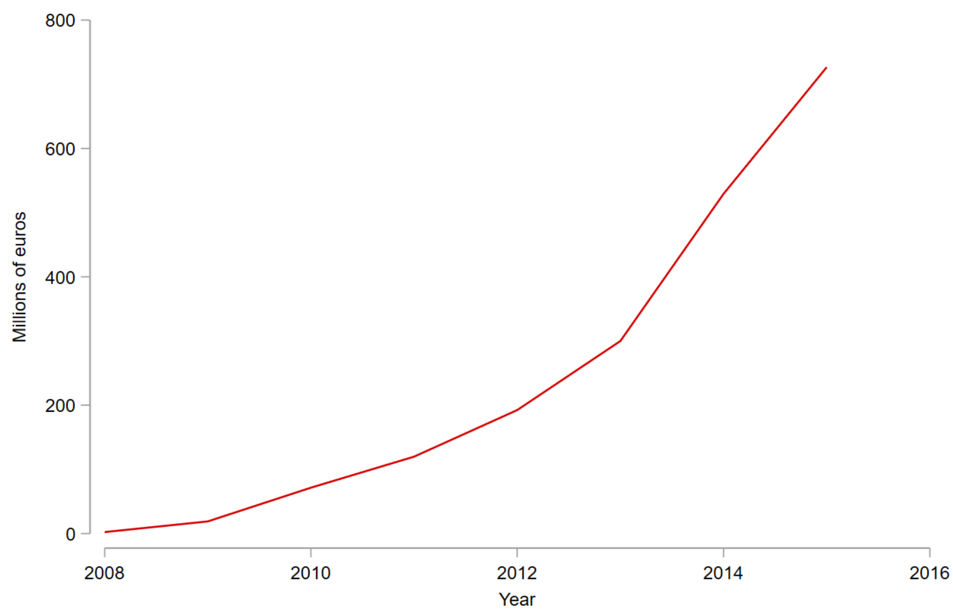

Figure 1: **AWAs market size in Italy, by year.**

Notes: Authors' calculation on INPS *Estratti Conto* archive. We calculate the market size as the total amount, in millions of euros, of voucher income paid to the workers in our sample multiplied by 10, as our sample covers $\sim$ 10% of the whole population of AWAs users.

In this paper, we combine two novel administrative dataset to study the effect of the use of vouchers on workers' income trajectories, namely overall income, income from more standard labor contracts, and cash transfers from social insurance programs. That is, adopting a worker-level perspective we test whether vouchers are complementary to more protected types of formal employment, or whether they tend to substitute such income sources. Relying on a standard labor supply model, we show that this elasticity of substitution is an essential parameter to establish whether workers' reliance on AWAs allows them to increase their welfare, as they are systematically constrained by limited employment opportunities with standard contracts, or whether vouchers are only a minor source of income that can be easily substituted with better jobs.

Relying on longitudinal data on the complete work history of voucher users within a fixed time window, we estimate the substitution elasticity between vouchers and other income sources. We compare results from a variety of estimation approaches, namely a pooled OLS model, a fixed effects model (FE), and the Semykina and Wooldridge 2010 and Wooldridge 1995 Correlated Random Effect (CRE) model. This estimator takes into account sample selection into different sources of income within a correlated random effects framework, where time-constant unobserved individual heterogeneity is accounted for through a parametric specification, while the sample selection process is allowed to vary across time.

Using the fixed effects specification, we find that the elasticity of substitution between vouchers and income from standard contracts is close to 0 (-0.02 percent for a 1 percent growth in voucher income). As a consequence, vouchers tend to significantly increase overall income, with an estimated elasticity of 0.74. However, when selection in the labor market is taken into account using the CRE model, the size of these estimates is radically affected: the negative substitution with income from standard contracts becomes much stronger, with an elasticity of -0.26, while the elasticity with overall income greatly reduces to 0.105.

Ignoring general equilibrium effects, this latter estimate suggests that, on average, voucher users would loose only 10% of their earnings after the abolition of this form of employment. This effect is found to be greater for men, immigrants, and older workers, likely indicating that the positive effects of AWAs tend to be stronger in segments of the labor market where the occasional activities covered by vouchers are more likely to be performed off the books[3].

We complement this analysis with an event study that exploits a legal threshold that was in force between 2012 and 2014 regulating worker's AWAs' maximum yearly income, set at 6,667 (5,000) euros gross (net). In this exercise, we hence focus on a smaller sub-sample of highly intensive voucher users, as the threshold is set at a relatively high level compared to the usual size of yearly income from vouchers for most workers. This restriction in the use of vouchers represents an *Employment Protection Legislation* (EPL so on) enhancement, as employers cannot exploit anymore this flexible and relatively inexpensive type of contract and must use more rigid and expensive ones if they want to employ the worker.

Results from this approach indicate that, after that workers reach their yearly cap, they experience a decrease in both voucher and overall income, which takes place gradually over time. Instead, welfare transfers and income from standard contracts remain stable. In summary, our findings indicate that the impact of vouchers is heterogeneous. On average, vouchers displace alternative income sources, particularly income from standard labor contracts. As a consequence they have only a very small positive effect

[3]Immigrants tend to be over-represented in the informal labor market. Similarly, workers with ongoing full-time contracts (that are more likely to be older and male) and retirees are usually not allowed to take up secondary jobs, unless through AWAs schemes.

on overall income. However, for individuals who heavily rely on vouchers, they represent the only source of formal income. For these workers, a reduction in the use of vouchers leads to a substantial reduction in formal income.

Due to limitations in data availability and the difficulties in capturing atypical types of employment in standard labor force surveys, AWAs have not been extensively studied.[4] One exception is Di Porto et al. 2022, who analyse the use of vouchers by Italian firms. They compare employers that increase the use of vouchers when random labor inspections occur, with those for which the use of vouchers is unrelated to inspections. They show that the former group of firms increased (relative to the latter) the use of standard labor contracts after the abolition of vouchers in 2017, suggesting that they were using vouchers to "hide" and potentially increase their reliance on undeclared work. We complement this study on two respects.

First, our sample is based on the recipients of vouchers, rather than firms. Moreover, we observe the complete work history of these individuals. This allows us to observe a comprehensive and fully representative sample of voucher users. Importantly, several types of employers that are not observable in the private-sector archive of INPS used by Di Porto et al. 2022 are instead included in our data, for example households, small business owners without employees and the agricultural sector.[5]

Second, our empirical approach recovers an elasticity of substitution between vouchers and income from standard employment, which is the crucial parameter to evaluate the impact of vouchers on workers' welfare in the absence of general equilibrium effects. By contrast, Di Porto et al. 2022 document a behavioral heterogeneity in the use of vouchers by firms, related to the underlying interaction between the reliance on undeclared work and labor inspections. However, this behavioral parameter represents a relative difference between groups of firms that used vouchers, thus it is not well suited to characterize the overall effect of vouchers on workers' welfare.

The rest of the paper is organized as follows:in Section 2 we provide evidence on the institutional background and the evolution of vouchers legislation in Italy, in Section 4 we describe the data and descriptive statistics, in Section 5 and 5.3 we present the empirical models and the heterogeneity, while in Section 5.5 the event study. We conclude in Section 6.

[4] Studies documenting several AWAs characteristics with detailed data include Farina, Green, and McVicar 2021; Adams, Prassl, et al. 2018; Anastasia, Bombelli, and Maschio 2016.

[5] Considering that vouchers were originally intended for occasional activities within households (e.g., cleaning, private tutoring, etc.) and in the agricultural sector, this is a quite important distinction.

2 History of Italian AWAs

In Italy, Alternative Work Arrangements (AWAs) are commonly referred to as "vouchers". Therefore, for the remainder of this paper, the terms "vouchers" and "AWAs" will be used interchangeably. We provide an overview of the timeline for the vouchers, from their introduction to their eventual abolition, in Figure 2. In 2008, Italy introduced vouchers as an Alternative Work Arrangement (AWA), but with a number of limitations. Employers were allowed to spend a maximum of 5,000 euros net, which translated to a third more in gross, on vouchers for each employee. Vouchers were restricted to students and retirees in the agricultural sector, and each voucher was worth 10 euros: 7.5 euros were net earnings for the workers, while 2.5 euros were the sum of pension contributions and working insurance.

Compared to standard employment forms, such as fixed-term and open-ended contracts, the costs for employers in terms of pension contributions and taxes were significantly lower. The purpose of vouchers was to provide employers with a tool to quickly adjust labor demand to supply for low-qualified and non-standard forms of work, especially for seasonal, touristic, and agricultural duties that were characterized by irregularity and flexibility, and to reduce undeclared work (Anastasia, Bombelli, and Maschio 2016; Passerini 2017). Over time, the conditions for vouchers were gradually expanded, as described in Figure 2 and shown in Figure B2.

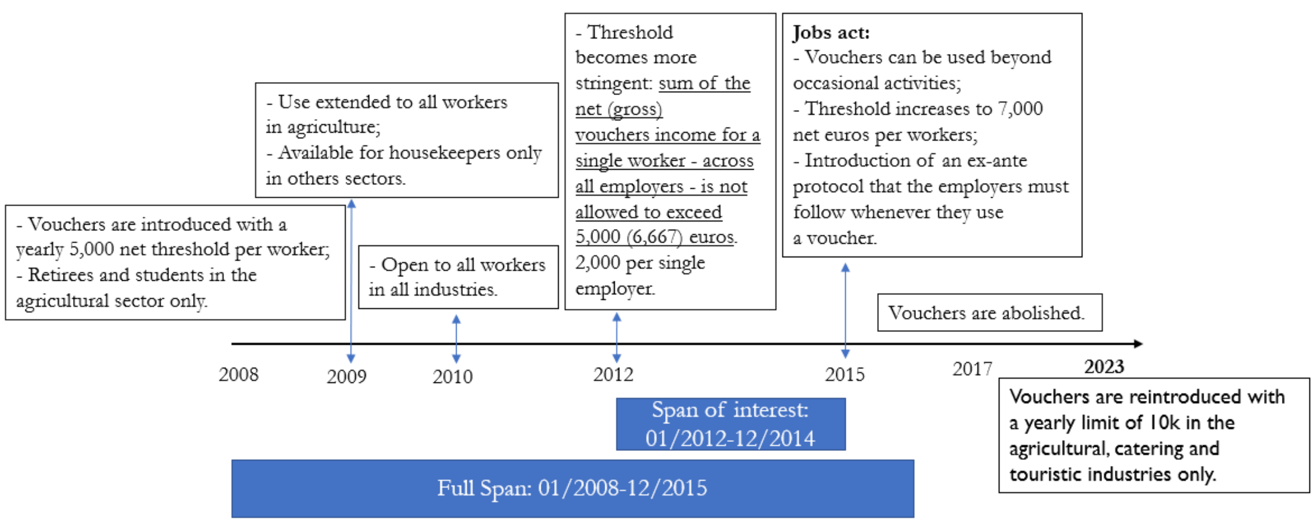

Figure 2: **Timeline of vouchers from the introduction to nowadays.**

Notes: Authors' realization.

The availability of vouchers was expanded to include all workers in the agricultural sector, not just students and retirees, and in 2009 they were made available in the retail, tourism, service, and housekeeping sectors. In 2010, the use of vouchers was completely liberalized to include all sectors and all workers. As shown in Figure 1, the number of workers using vouchers exponentially increased as the limitations were lifted. This resulted in most Italian employers being able to use this alternative form of employment, which entailed significantly lower costs, both monetarily and legislatively, and could be utilized in different contexts and for various purposes. This marks the peak of EPL reduction during the period analyzed in our study. However, due to concerns and opposition from the public, the trend began to reverse in 2012. The monetary threshold became more stringent, with the total gross (net) income from vouchers for a single worker (and across all employers) not allowed to exceed 6,667 (5,000) euros. The threshold was likely not strictly enforced, as authorities' controls were infrequent and not very strict[6].

However, in the same year, was promulgated the *Fornero* Reform, which limited the use of FT employment contracts by making their renewal more expensive for all firms. This likely increase also the opportunity cost of using AWAs as an alternative tool with respect to FT contracts for some kind of working performance.An important step in the use of vouchers in Italy occurred with the Jobs Act, which was approved in early 2015 by the center-left coalition led by PM Matteo Renzi. This act allowed vouchers to be used for activities beyond occasional work, increased the yearly net limit per worker to 7,000 euros, and introduced an ex-ante protocol that employers must follow whenever they use one. The protocol became more stringent as controls became more frequent, resulting in a dramatic increase in voucher use. This period marked the lowest point in EPL that the Italian labor market had ever experienced. However, since our data only covers the period from January 2008 to December 2015, we cannot use this reform to identify any exogenous variation in voucher use.

In 2017, due to unions and public protests claiming that vouchers were used as a tool to suppress workers' wages and increase career fragmentation, they were completely abolished. This episode was the first in a trend of increasing EPL in the Italian labor market, which culminated in the promulgation of the *Dignity Decree*[7] following the 2018 political election outcome[8]. This decree was not the only reform approved in the direction of higher social protections, as a form of universal basic income known as the *Citizenship Income* was also introduced. The debate around vouchers was reignited in 2020, when several policy-makers argued that they should be reintroduced to boost employment and help firms after the COVID-19 pandemic outbreak.

However, no action was taken until 2022 when the latest electoral outcome resulted in a right-wing

[6]For more information on this issue, refer to Di Porto et al. 2022.

[7]It wasn't the only reform approved that went in a direction of higher social protections as also the *Citizenship Income*, basically a form on universal basic income, was introduced.

[8]The election saw the skyrocket of two namely small parties, the 5 stars movement, which trespasses the 30% threshold, and the Northern League.

coalition winning the election[9]. This coalition planned to reintroduce vouchers in the 2023 financial manoeuvre for the touristic, food service, and agricultural sectors only, with a yearly limit of 10,000 euros per worker. In early 2023, vouchers were reintroduced as planned in the financial manoeuvre. Our data covers all major reforms affecting vouchers, with the exception of the Jobs Act, and spans from the beginning of 2008 to the end of 2015. For our empirical analysis, we will focus on the period from the beginning of 2012 to the end of 2014 to compare the results of the regressions with those of the event study.

3 Conceptual Framework

Before introducing our empirical analysis, we provide a brief conceptual framework to illustrate our research question. We rely on a standard labor supply model that describes workers' choices under two alternative scenarios. First, when both AWAs and regular contracts are available for workers, then when only regular contracts can be found. This model aims to illustrate under which conditions workers' welfare is improved in the presence of AWAs. The model abstracts from demand-side considerations, a choice motivated by the fact that our database is not representative of the firms' population. For this reason, it is not possible to establish *e.g.* whether and how much employers substitute AWAs with standard employment contracts.

In the model, we assume that AWAs are cheaper for employers, while standard employment contracts are preferred by workers. The motivations are straightforward: vouchers were introduced with the specific purpose of being a tool for firms to reduce costs and increase flexibility, while they entailed a reduction in employment protection for workers. Workers maximize the following utility function, which depends on leisure (L) and consumption (C):

$$\max_{L,C} \; U(L,C)$$
$$\text{s.t.} \qquad L + h \leq T$$

We assume that the first derivatives of $U()$ are positive with respect to both arguments, while the second derivatives are negative. T is the amount of time available in a given period, while h is the labor supply. Workers can be employed under an alternative work arrangement at a wage w_a, or with a standard employment contract at a wage w_c, and we always assume $w_a < w_c$. We assume that all income is spent on consumption and that workers initially face the following budget constraint:

[9] This coalition is formed by the following parties: two right-wing, Brothers of Italy, which additionally expresses the PM Giorgia Meloni, and the Northern League, and one centre-right, Forza Italia.

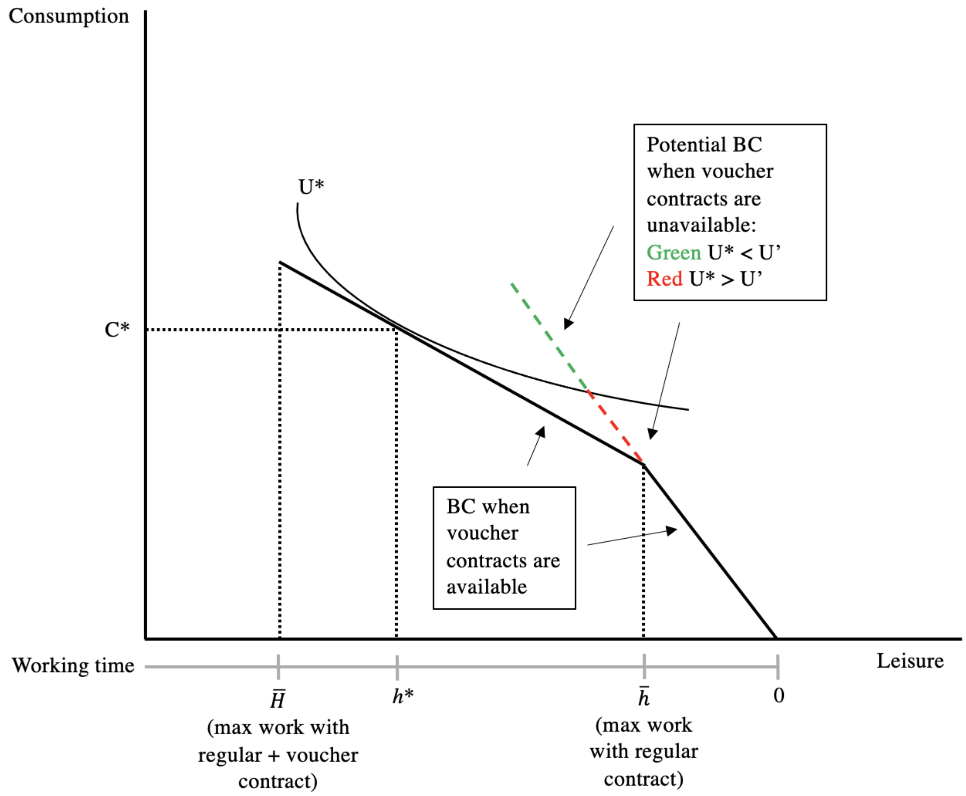

Figure 3: **Labor supply with regular contracts and vouchers, and relationship with contexts where AWAs are unavailable.**

Note: Authors' realization.

$$C = \begin{cases} w_c h & \text{if } \ h \leq \bar{h} \\ w_c \bar{h} + w_a(h - \bar{h}) & \text{if } \ \bar{H} \geq h > \bar{h} \end{cases}$$

where $\bar{h}$ is the maximum employment level available for the worker under a standard contract, while $\bar{H}$ is the maximum employment level under both types of contracts. We can interpret this model as a situation where workers can find only a limited amount of employment and only two types of jobs. Jobs under an alternative work arrangement are paid less. Thus, they are chosen only if there is no additional employment under a regular contract available.

Figure 3 shows an optimal solution such that the labor supply h^* is higher than $\bar{h}$. At this solution, workers choose both types of jobs in order to reach the preferred consumption level C^*, and they gain an utility level U^*. We now consider how this solution could be affected by the unavailability of alternative work arrangements. In particular, we assume that an unexpected labor demand shock hits the worker in a subsequent period, and only regular employment contracts become available.

If preferences are kept constant, the only mechanism inducing changes in the labor supply are shocks occurring in the labor demand faced by workers. The dotted line in Figure 3 represents a potential budget constraint after a labor demand shock, where we assume that alternative work arrangements become unavailable. We can identify two segments of this potential budget constraint. The part below the indifference curve corresponding to the utility level U^*, which is highlighted in red, represents a budget constraint such that workers' welfare is reduced after the labor demand shock. If instead the availability of employment opportunities under standard contracts is high enough, so that the budget constraint above the indifference curve U^* (highlighted in green) becomes feasible, workers' welfare is improved. Let U' represent the utility level reached by workers after the labor demand shock.

Notice that a sufficient condition to ensure that workers' welfare is improved by the unavailability of AWAs is that earnings from regular contracts are higher than total earnings at the previous solution. That is, any consumption level above C^* obtained without relying on AWAs is sufficient to guarantee that $U' > U^*$. More generally, workers' welfare improves if total earnings increase in response to any reduction in earnings from AWAs. This suggests that measuring the substitution elasticity between voucher income and other sources of earnings can inform whether workers' welfare systematically improves when they reduce their reliance on AWAs. However, this is only a sufficient condition, as in general it is possible to have higher welfare also if total earnings slightly decrease in the absence of AWAs.[10]

Given this discussion, estimating the substitution elasticity between voucher income and total earnings allows to evaluate the impact of AWAs on workers' welfare. If this elasticity is negative, then workers' welfare systematically improves when the use of AWAs reduces. A similar result would suggests that abolishing these types of contracts could be beneficial for workers, given the abundance of better employment opportunities usually faced by them. However, the test on the sign of the substitution elasticity imposes a stricter condition than what would be needed to conclude that workers' welfare improves without vouchers. Indeed, this hypothesis cannot be ruled out also if the elasticity of substitution is positive, but close to zero.

Notice that in this conceptual framework we have assumed that workers' preferences are always constant in response to shocks to the budget constraint. Thus, to conduct a meaningful welfare analysis the substitution of AWAs with standard contracts should be estimated using only shifts in the labor demand as a source of variation in vouchers' use. We discuss in more detail under which assumptions this substitution elasticity can be identified correctly when presenting the empirical approach.

[10] In Figure 3 this case is represented by solutions that lie on the green portion of the dashed budget constraint that lays below C^*.

4 Data

We combine two different sources of data:

Vouchers records We exploit a new matched employer-employee data set extracted from the Italian Social Security Agency (INPS) archive, providing information on a random population of voucher users between January 2008 and December 2015. Individuals are sampled as those born on days 1 and 9 of any month and year, using vouchers at least once in the period of analysis. Therefore, our sample covers approximately 10% of the whole population of voucher users, at least once, between 2008 and 2015. Individuals are 155,861 and uniquely identified by their anonymized fiscal code. The number of vouchers recorded is 1,478,722. For each spell paid with vouchers, we know the gross remuneration (*i.e.,* before taxes and including social and security contributions), the province and macro-industry of use, and a univocal firm identifier.

Estratti Conto We rely on matched employer-employee data up to a monthly level coming from the *Estratti Conto* extracted from the INPS archive. It provides information on all income sources of the same population of voucher users, matched through their anonymized fiscal code, between January 2008 and December 2015. It contains approximately 680,00 records, split between welfare transfers, as provided by INPS, and standard labor spells. The former are mobility income, layoff income (the so-called *"Cassa' Integrazione Guadagni"* or *"CIG"*), which further disentangle in "CIGO" (designed for short-time transitory shocks[11]) and "CIGS" (designed for more prolonged shocks[12]), and unemployment benefits. The latter are instead all other standard labor contracts (mainly FT, open-ended, and apprenticeship), for which we know gross earnings, type of contracts, unit of contribution, contribution funds, type of contribution, time schedule, firm id, province of residence, sex and nationality of the worker[13]. Unfortunately, we don't have detailed information on the industry and on the geographical location of these spells.

We merge these two data sources and we fill in incomes in all missing months, namely those in which workers formally don't work, or work undeclared, with zeros. We derive a perfectly balanced worker-level panel made of 14,473,440 worker-month tuples describing the income trajectories of the 155,861 workers (132,000 with the full set of information) up to at a monthly level from January 2008 to December 2015.

Cleaning steps First, we trim all the observations above the 99^{th} percentiles in voucher and standard

[11]It is provided for 13 weeks that can be prolonged up to 52.

[12]It is provided for 12 months for industrial reorganization (and up to 24 months for industrial crisis and failures.

[13]Regarding, in particular, the type of contract and the contribution funds, we have very detailed information, as always provided by *Estratti Conto*. However, we do not fully explore it as it is irrelevant to answer our research questions.

labor earnings separately. We did so from the beginning because they are abnormal values which would severely bias any estimates. We thus derive the first panel (Panel (a), Table A1) made of 14,096,928 worker-month records associated with 146,843 workers, covering the income trajectories of approximately 10% of all vouchers users in Italy at least once from January 2008 to December 2015. We first select the period between 2012 and 2014, when the threshold set by *INPS* is mandatory, making thus the estimates comparable in terms of legislation between the FE and CRE specifications and the event study. The number of observations decreases thus to 5,286,348 (Panel (b)). However, only a share of these individuals work with vouchers between 2012 and 2014: we keep only them. We additionally keep only those individuals of age between 16 to 64 years (the standard active population sample). The number of observations decreases to 2,929,392 worker-month tuples and 82,055 workers. For the regressions, we collapse the dataset at a quarter level. We finally obtain a dataset of approximately 1,000,000 worker-quarter tuples associated with 82,055 workers. We provide more evidence in A1 in the Appendix.

Outcomes and covariate The outcomes of interest are *total income*, sum of all sources of income, *standard labor Income*, sum of all incomes that come from working activities, *welfare transfers*[14], the sum of all monetary INPS transfers. The covariate of interest is *AWAs income*, which is the quarterly/monthly income a worked gained with vouchers. Table A1 indicates that there is a clear positive relationship between total income and voucher income in the sample of workers, as AWAs account for the highest share of total income. On one hand, there is a negative relationship between labor and voucher income. This evidence can be interpreted in two ways: There is a complementarity between total and voucher income, as most of the individuals earn for living mostly relying on occasional working performances, or voucher income is a substitute for standard labor income, as vouchers are used by employers as a tool to compress workers' welfare.

Descriptive evidence Table A1 shows that our sample is predominantly composed of young individuals (with an average age of 36), men (55%), and Italians (77%). However, there is a significant representation of foreigners (23%)[15]. We are aware that, in the Italian labor market, immigrants are more likely to be concentrated in low-value-added and occasional activities, as well as in undeclared working performances, which are more prevalent in the southern provinces. Additionally, women make up 44% of the sample.

Our sample includes data from 100 out of 107 Italian provinces, with a higher representation of northern provinces, such as Lombardy, Veneto, and Emilia-Romagna, compared to other regions (Figure

[14]We will refer to it for the rest of the paper also as *subsidies* or *other income*.

[15]The most common nationalities are Morocco, Tunisia, Albania, Romania, Moldova, Ukraine, and Bangladesh, with Romania, Morocco, and Albania being the top three.

B3, Appendix). The figure also reveals that many vouchers were used outside the province of birth, particularly in northern Italy. This implies that firms used vouchers to find a labor force that was not easily accessible in their vicinity, indicating that AWAs may have been more effective in reducing labor market frictions in northern provinces than in other regions (Cavallotti et al. 2020).

Compared to the average employee, voucher users tend to be younger and more likely to be foreign, although there is no significant difference in gender. However, the differences become more pronounced when examining outcomes. Our sample consists of 11,000,000 observations over 14,500,000, and most of the workers in our sample do not have any income. Specifically, 89,000 out of 155,861 workers only appear in our dataset while working with AWAs and have no standard labor working spells between 2008 and 2015. The average monthly wage in the entire sample is just under 400 euros. It rises to approximately 1700 euros when only considering the months in which workers appear in our data. According to Table A2, the average monthly gross salary of a standard employee during the period of analysis was approximately 1700 euros, which is significantly higher. In summary, the average voucher user is much more likely to be unemployed (or working without declaring), she earns significantly less, and she is more likely to be a foreigner compared to the average employee in the private sector.

5 Empirical Strategy

5.1 Fixed Effects Specification

As outlined in Section 3, our primary goal is to identify an elasticity of substitution between AWAs and other sources of income. Thus we consider the following fixed effects specification:

$$Y_{i,q} = \theta X_{i,q} + f(Age_{i,q}) + \delta_i + \beta_q + e_{i,q} \tag{1}$$

where i index individuals and q index quarters. $Y_{i,q}$ represents the outcome, defined alternatively as total income, income from standard labor contracts, and cash transfers from social insurance programs (unemployment benefits and sick leave). $X_{i,q}$ is the independent variable of interest, representing voucher income. We transformed both of these variables by the Inverse Hyperbolic Sine function to deal with zero income cases[16]. δ_i and β_q represent an individual and quarter fixed effect, respectively. $f(Age_{i,q})$ is a cubic polynomial in workers' age, which we assume to be flat at 45 years old following the approach of Card et al. 2018 to deal with its multi-collinearity with worker and time fixed effects. We estimate

[16]The use of the IHS transformation allows for consistent estimation of elasticities even when variables are inflated with many zeros, as is the case in our dataset (Bellemare and Wichman 2020).

standard errors by clustering at the worker level.

The parameter of interest, denoted by θ, is the elasticity of substitution between voucher income and the income variable defined by Y. Since both variables are transformed using the IHS function, θ should be interpreted as an asymptotic elasticity. As discussed in Section 3, this parameter should be estimated using only shifts in the use of vouchers that are driven by labor demand shocks faced by workers. Instead, changes in the labor supply that are driven by workers' preferences should be controlled for by the regression model. On this respect, the inclusion of worker fixed effects controls for any time-constant individual heterogeneity in workers' preferences. The nonlinear age effect and quarter fixed effects further control for time-varying shifts in individual preferences, as long as they are common across age groups and time.

A particular form of time-varying shock in preferences could be generated by intertemporal optimization. For example, if only vouchers are available in the current period, while workers correctly expect greater job opportunities with standard contracts in the future, they may increase leisure today and work more when better jobs are available. A similar mechanism would negatively bias the elasticity of total income to vouchers, since labor supply would partly drop because of a shift in workers' preferences when vouchers are the only income source. While this form of adjustment would be efficient from a theoretical point of view, recent micro-based estimates of this intertemporal elasticity show that employment (both at the extensive and intensive margin) is not responsive to temporary wage shocks (Martínez, Saez, and Siegenthaler 2021)[17].

A second identification problem is related to reverse causality. In particular, the size of income opportunities from standard contracts and from vouchers could be jointly determined. For example, Addison and Surfield 2007 have shown that AWAs are more common among workers segregated in low-productivity and low-value-added industries, which would generate a cross-sectional negative correlation between voucher income and standard labor income. In this regard, we assume that worker fixed effects can account well for individual market opportunities and earning potential. A further identification problem is given by correlation in shocks to the labor demand of vouchers and standard contracts. In this respect, time fixed effects should account for market-level fluctuations in the availability of job opportunities.

5.2 Semykina and Wooldridge 2010 estimator

The econometric literature has widely studied methodologies to take into account the extensive and intensive margin of individuals into the labor market depending on their characteristics (Heckman 1993).

[17] Using a staggered Swiss tax holiday (two years of income that never formed the basis for taxation) Martínez, Saez, and Siegenthaler 2021 show that extensive margin employment and hours of work were not positively affected by this event.

If the determinants are time-invariant, the non-random selection of individuals is ruled out relying on individual FE. However, when they are not, the estimates are biased. This is for instance the case of Jäckle and Himmler 2010 when estimating the effect of health status on wages in the US. Unobserved time-varying health determinants, such as individual lifestyle[18] or motivation, affect the selection and are not captured by the individual fixed effects. These drivers influence wages through the error term and therefore lead to inconsistent estimates. Our case is similar.

Semykina and Wooldridge 2010 derives an asymptotically consistent estimator in the case of endogenous selection of individuals in the sample depending on observable time-varying and unobservable time-invariant characteristics. This estimator improves the Wooldridge 1995's one as it controls for sample selection in a fixed effects model with endogeneity. This is our case, as there might be unobserved time-varying worker characteristics influencing workers' capability to enter the job market and influencing workers' choice to exploit vouchers. Employing a within estimator such as the fixed effects is thus a reliable approach only when it is sure that the decision to participate in the labor market is random, or fully captured by the observable variables and the set of fixed effects.

This is not our case, and hence we need to address the sample selection bias relying on a proper estimator. We then estimate a Correlated Random Effects (CRE) model in two stages, where in the first individuals' selection into the labor market is modeled ex ante at each point in time, while in the second it estimate the cross-income elasticity of interest. The CRE model is similar in spirit to the FEmodel: time-varying workers' unobserved heterogeneity must be exogenous in the regression and selection equations (Mundlak 1978). As first stage we estimate the following regression:

$$\forall q : W_{i,q} = \eta M_i + f(Age_{i,q}) + \gamma Sex_i + \delta Nat_i + \beta WE_{i,q} + \Theta WE^2_{i,q} + \theta X_{i,q} + \epsilon_i \tag{2}$$

where W_i is a dummy equal to 1 if the individual i works in quarter q, 0 otherwise:

$$W_{i,q} = \begin{cases} 1 & \text{if Total } Income_{i,q} > 0 \\ 0 & \text{if Total } Income_{i,q} = 0. \end{cases} \tag{3}$$

$WE_{i,q}$ denotes the cumulative months of working experience of worker i in q during the previous two years, which represents the explanatory variable included only in the selection equation[19](Jäckle and

[18] *e.g.,* physical activities, smoking.

[19] As is standard for the selection model, the selection equation is modelled using additional explanatory variables, in our case a linear and a quadratic in short-term working experience, which are not included at the wage equation instead. This approach allows to limit the multi-collinearity between the inverse Mills ratio and the covariates included in the wage equation.

Himmler 2010). The covariates included only in the selection equation, Equation 2, must drive the decision to participate, but at the same time can be omitted from Equation 4 as they have a low explanatory power for wage levels. In our specification, the excluded covariates are a linear and a quadratic polynomial in short-term working experience. We know that working experience during the past two years is a powerful predictor of workers' labor market participation, but, at the same time, it is unlikely to be a strong predictor for wage levels, considering that we simultaneously control for the effect of age ($f(Age_{i,q})$ is a cubic polynomial) and for individual heterogeneity.

We measure working experience at each q as the sum of quarters worked[20] by each individual in the previous 2 years (8 quarters). However, in this case, we might under-weight those workers more likely to be in the labor market because older. For this reason, we extend the size of the rolling window to 3, 4 years, and up the first quarter of observation of our data (q1-2008). We do that in order to avoid under-weighting those workers more likely to be selected in the labor market because of their age or other unobservable drivers. Results are displayed in Figure B4 in the Appendix.

ϵ is an idiosyncratic error term clustered at an individual level and estimated through bootstrapping. X is the voucher earning (transformed with the IHS) of worker i in quarter q. M_i instead is a matrix containing the within-time mean of all individuals covariates and represents a parametrically way to model the individual FE (Mundlak 1978, Wooldridge 1995)[21]. The estimator controls hence for individuals' endogenous selection into the labor market depending on both time-varying and not determinants. Among the predictors, besides the polynomial in working experience, we also include sex, nationality, and a cubic polynomial in age. The estimator additionally includes all the covariates of the second stage, so voucher earnings, plus for each of the time-varying covariates the individual mean across time, which act as proxies for the individual FE.

The equation is estimated separately for each q with a probit model, and the estimator produces for each q an individual-specific Inverse Mills Ratio ($IMR_{i,q}$) that becomes a covariate at the second stage. The IMR is a probability predicting individuals' likelihood of participating in the labor market, and hence its inclusion at the second stage adjusts the estimates for the sample selection bias. At both stages, we do not include individual fixed effects and thus we do not replace age with 0s those values below 45 years. In the second stage, we run the following regression:

$$\mathrm{Y}_{i,q}=\eta M_i + f(Age_{i,q}) + \gamma Sex_i + \delta Nationality_i + \mathrm{IMR}_{i,q} + \theta\, \mathrm{X}_{i,q} + v_{i,q} \tag{4}$$

[20] A quarter is defined as a working one if the worker has a positive earning.

[21] They write the individual fixed effect M_i as a linear projection onto the time averages of all time-varying controls.

, where the outcome Y, the covariate of interest X and the estimated parameter θ is econometrically unchanged with respect to Equation 1[22]. Controls are the same as in Equation 3, with the only exception of (the polynomial in) working experience which is the exogenous variable identifying the first stage. The equation is estimated as a standard OLS. What differs instead is that the CRE model estimates an elasticity for the whole Italian working population, so regardless of labor market participation, while the previous specification is valid only for the sub-sample of voucher users. Thus, as long as the first stage is correctly specified and the assumptions hold, the estimator computes an *Average Treatment Effect (ATE)* that should be interpreted as the average effect of an additional euro earned through vouchers on the outcome of interest regardless of the selection of individuals in the labor market.

5.3 Baseline Estimates

The results of the wage regression are contained in Table 1. Those of the selection stage of the CRE model are instead displayed in Appendix 6. According to previous predictions, results do change considerably across specifications, especially when moving from the FEto the CRE specification. Estimates contained in Column (1), those not including individual and time-fixed effects, show all positive and significant coefficients. This indicates that vouchers are a complementary source of all sources of income whose use increases total earnings but also enriches all welfare transfer sources. These results support the hypothesis according to which AWAs are a tool used for workers at the margins to enter the labor market and access alternative sources of income.

When the set of fixed effects is included, however, the elasticity with respect to standard labor income becomes negative and significant (-0.02 p.p.). This indicates that previous estimates were likely driven by the presence of unobserved time-invariant worker heterogeneity that the fixed effects rule out. Results of the fixed effects specification indicate instead that vouchers substitute alternative income sources, while they increase the overall one. This happens because most of the workers included in our samples rely on AWAs to generate their income, suggesting that they belong to the weakest segments of the labor market.

Overall, our findings indicate that voucher use increases workers' quarterly income, and thus their abolition likely had a sudden negative effect on their earnings. However, the FE model still do not take into consideration the role played by worker heterogeneity driving their selection into the labor market. We address it with the CRE model, whose results are displayed in Column (3).

[22] Both the outcome and the covariate are transformed with the IHS function and hence θ is an asymptotic elasticity.

Dependent variable	(1) OLS	(2) FE	(3) CRE
Standard Labor Income	.021***	-.020***	-.259***
	(.00205)	(.00123)	(.00429)
Welfare Transfers	.016***	-.0007	-.049***
	(.00116)	(.00085)	(.00299)
Total Income	.780***	.741***	.105***
	(.00151)	(.00181)	(.00238)
Observations	981,092	981,092	970,470
No. of workers	82,055	82,055	82,055
$f(Age_{i,q})$	√	√	√
quarter FE	√	√	√
Sex	√	-	√
Immigrant	√	-	√
Worker FE	-	√	√

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

Table 1: **Baseline estimates of Equations 1 and 4.**

Notes: The specifications are estimated on the active population sample, individuals ranging from 16 to 64 years according to OECD, of those working at least once with voucher in 2012-14. Column (1) contains standard OLS estimates with no fixed effects at a quarterly level, Column (2) FE estimates at monthly level while Column (3) contains quarterly-level estimates of the correlated random effect (CRE) model of Wooldridge 1995 and Semykina and Wooldridge 2010 and based on Mundlak 1978. (CRE) Worker FErow indicates the inclusion of the standard worker, or the CRE model, fixed effects. In the CRE specification observations are lower because few are dropped due to collinearity issues at the first stage. Coefficients of Column (3) should be interpreted as average marginal effects for the whole population, regardless of labor market participation. The dependent and independent variables are all transformed with the Inverse Hyperbolic Sine (IHS) function, and hence should be interpreted as asymptotic elasticities. First stages predictors in Column (3) include sex, age squared, nationality, and individual working experience in quarter. Individual-mean for all controls are included in Column (3). Period goes from q1-2012 to q4-2014. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings. The samples are made of 981,092 worker-quarter tuples each.

Results do change significantly: vouchers become seven times more detrimental for labor and welfare transfers than in the FE specification, while their upward effect on total income vastly reduces as the elasticity goes down to 0.105 p.p.. This variation in our estimates indicates that workers' unobserved heterogeneity, especially among marginalized workers, does play a relevant role in predicting their participation in the labor market and the income sources they exploit. Furthermore, a check for the presence of selection bias can be carried out by Wald tests on the joint significance of the inverse Mills ratios. Results, not attached, show that the IMR are always significant and prove that selection bias is very relevant hence. Hence, the use of the CRE model is necessary for this framework.

Average marginal effects in the active sample range between 0.105 for total and -0.26 p.p. for standard labor income. Each euro earned through vouchers in our sample increases overall income by 10 cents, while it reduces alternative standard labor income by 26 cents. It simultaneously reduces welfare transfers by 5 cents for each euro. Estimates obtained with the fixed effects specification indicate instead that an additional euro earned through voucher on average in our sample increase overall income by 75 cents, while it reduces standard labor income by 5 cents with no effect on welfare transfers. On average, the abolition of vouchers would lead to a 10% reduction in total earnings in the preferred estimates, ignoring general equilibrium effects.

Robustness check To test the robustness of our baseline estimates, we conducted an alternative analysis in which we calculate the working experience in a rolling time window of three, four years, and up to the first quarter available in our data. We find that the coefficients remained strongly significant, becoming smaller in magnitude as the time window enlarges. This is due to the fact that we weighed more older workers and less younger workers who are more likely to rely on vouchers to gather an income and less likely to have previously exploited standard working arrangements. The elasticity with respect to overall income becomes 0.082, 0.074, and 0.04 as the rolling window size increases. The elasticity with respect to standard labor income becoes -0.32, -0.34, and -0.35 and respectively, while the elasticity with respect to welfare transfers becomes -0.059, -0.065, and -0.076. These results slightly change in magnitude with respect to the baseline estimates, but overall the interpretation remains the same.

5.4 Heterogeneous Effects

Based on the evidence provided in Section 6 we augment the preferred specification, the CRE model, investigating heterogeneity by sex, nationality, and age. There is indeed evidence that AWAs are used differently across groups of individuals. Anastasia, Bombelli, and Maschio 2016 find that vouchers are more intensively exploited by immigrants and men. Moreover, if we assume that vouchers are used as a tool to cover black jobs or if they are used by employers to compress workers' wages (Di Porto et al. 2022; Datta, Giupponi, and Machin 2019) we would expect to find different results across workers and areas.

The reason is that in the Italian labor market immigrants and women tend to be more concentrated in the low-wage sectors of the labor market, where employer power is stronger, illegal practices are more widespread, and the presence of crime is more intense.

If instead vouchers create new job opportunities, serving as an entry tool into the labor market, we would then expect the opposite effect: for those workers more at the margins of the labor market, vouchers increase their working opportunities more than for the others. It means that according to our framework, we should expect higher elasticities for women and migrants with respect to total income and less negative instead for standard labor and welfare transfers. Furthermore, we might also expect employers' power to be more detrimental for younger workers, according to the first interpretation. At the odd, younger workers, whose frictions entering the job market are considered high especially in Italy, could find vouchers more effective as a tool to access the job market and increase working hours. We provide more references and a formal empirical specification in Equation 6 in Section 6 in the Appendix.

Sex Results are displayed in Figure B5 in the Appendix and are all precisely estimated as the standard errors are very small. Total income elasticity for men is slightly higher than that of women. Overall, voucher income increases total income for men more than for women. Additionally, there is no significant heterogeneity instead for standard labor income and welfare transfers. It means that voucher income does not substitute labor and subsidies incomes mostly in a different way for men and women. These two findings indicate that vouchers increase total income for men more than for women, but at the same time crowd out alternative income sources with the same intensity. Vouchers are thus more useful for some groups of individuals than for others.

Nationality Results are displayed in Figure B6 in the Appendix and are again all precisely estimated as the standard errors are very small. Estimates indicate that the elasticity for immigrants are slightly higher than that of Italians.s As for sex, there is no, or little, heterogeneity for labor and welfare transfers also by nationality. The interpretation is that vouchers represent a tool for the weakest workers, those that are marginalized and relying exclusively on standard employment contracts, to formally access the labor market and gather an income. This exercise again indicates that vouchers' utility is very heterogeneous depending on the characteristics of individuals exploiting them. Consider for instance an immigrant man and an Italian woman: our estimates indicate that vouchers' effect on their income would significantly differ, resulting in turn in very different effect on their earnings trajectories.

Age We divide our active working population into four discrete brackets, 16-25 years, 26-35, 36-55, and 56-64. Results are displayed in Figure B7 in the Appendix. The elasticity on overall income for

16-25 years old workers, presumably students involved in some kind of occasional working activities, is significantly lower than the baseline (0.083 vs 0.105 p.p.). For workers of age 26-35 it increases up to 0.093 p.p., which is still lower than the baseline, while for those of age 36-55 the elasticity is slightly higher than the baseline and equal to 0.11 p.p.. Finally, for oldest workers, the elasticity substantially increases up to 0.14 p.p., almost doubling that for younger workers. As for sex and nationality, the elasticities for labor income and welfare transfers are no or slightly different from the baseline.

We thus find that vouchers increase income monotonically with age while their substitution effects on other income sources remain basically unchanged. This evidence answers policy-driven concerns regarding vouchers' role in the labor market: rather than being a tool for younger individuals, outside of the labor market, to enter and earn, they seem instead more useful to help marginalized workers access and remain in the labor market, increasing their income over time.

5.5 Event Study

In this section, we identify a shock by leveraging a kink in the Italian legislation that regulates the income workers can earn through vouchers. Specifically, between 2012 and 2014, the Italian social security agency, INPS, set a yearly limit of 6,667 euros (5,000 net) on the gross cumulative voucher income that each worker could earn across all employers and industries. Non-compliant firms were fined an unspecified amount and were prohibited from using vouchers for the rest of the year[23]. To conduct the experiment, we compare the incomes of workers who have reached the threshold in a given time period to those who have not yet reached it but are expected to by the end of the year. In short, we exploit the timing, as some workers might reach the threshold before others. We then compare their AWAs' income trajectories, relying on the fact that controlling for a full set of fixed effects, they are not statistically different in the pre-period. In the study, approximately a thousand workers were observed each year between 2012 and 2014 who reached or exceeded the threshold.

[23]The likelihood of controls is presumably higher in the northern regions, where social capital is higher and the rule of law is stronger.

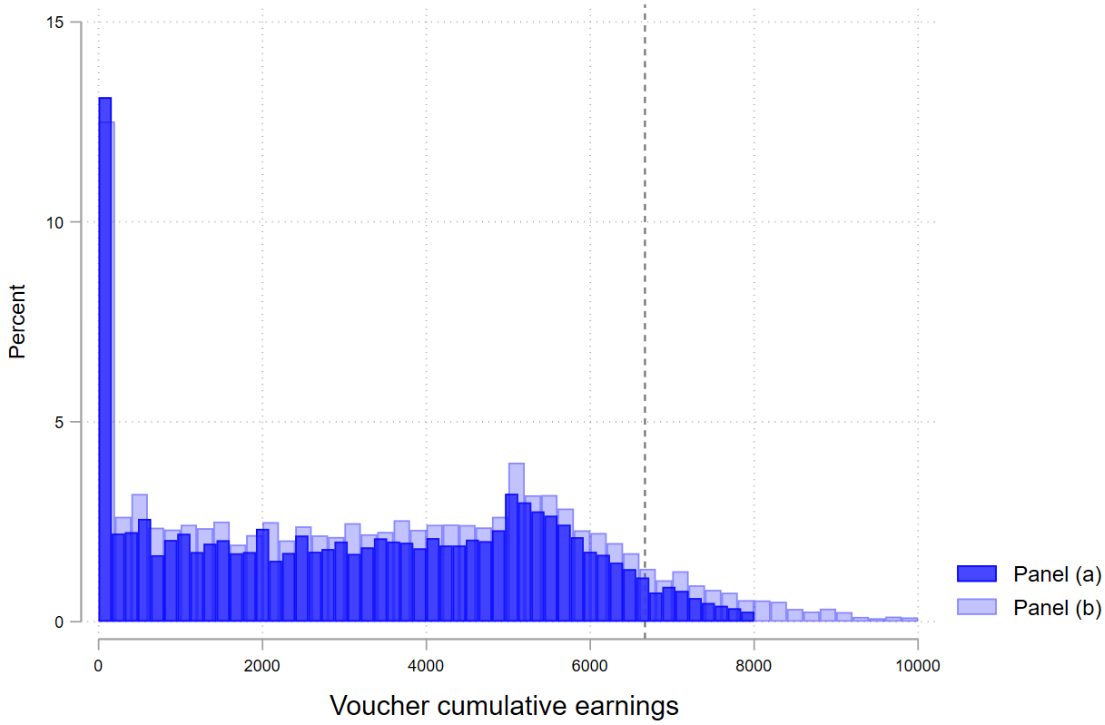

Figure 4: **Distribution of monthly cumulative AWAs earnings in two different samples.**

Notes: The populations are made of those earning between 5,000 and 8,000 euros in Panel (a) and between 5,000 and 10,000 in Panel (b) in a year in 2012-14 with vouchers. The dotted vertical line represents the legal threshold set by *INPS* of yearly cumulative gross voucher income per worker of 6,667 euros.

Indeed, focusing on the workers who are more likely to reach the voucher income threshold provides important insights into the potential impact of voucher policies on the most vulnerable segments of the labor market. While the results may not be as externally valid as those obtained with the CRE model, they provide valuable information on the effects of voucher policies on workers who rely heavily on alternative work arrangements. Policymakers can use this information to design targeted policies that help improve the labor market outcomes of these vulnerable workers.

Figure 5.5 shows a histogram of the earnings of workers in the two panels, with a vertical line indicating the threshold of 6,667 euros gross earnings. The figure shows that there is a clear discontinuity in the distribution of earnings at the threshold, with a sharp drop in the number of observations beyond the threshold. The majority of observations are concentrated within a bandwidth of 5,000 euros, which indicates that the threshold does have some bite even though it may not be strictly binding. The fact that the threshold appears to bite at 5,000 euros and not at 6,667 euros is attributed to the confusion among employers about the rules regarding the threshold.

Staggered treatment There is a growing body of literature that explores the bias resulting from staggered treatment adoption and the misspecification of control groups in a difference-in-differences framework[24]. Nonetheless, studies have shown that Two-Way Fixed Effect (TWFE) estimators can be unbiased for the Average Treatment Effect (ATE) if the Parallel Trend Assumption (PTA), among other assumptions, holds and the treatment effect remains constant over time and between groups. However, these assumptions are unlikely to hold in our context, as our control group comprises workers who become treated later and treatment adoption is staggered. Consequently, we rely on the Chaisemartin and D'Haultfoeuille 2020 estimator.

To further validate the Parallel Trend Assumption (PTA), which is a crucial assumption in the Difference-in-Difference strategy, we conduct an event study analysis across different sub-samples of workers. Specifically, we focus on workers earning between 5,000 and 8,000 euros in Panel (a) and between 5,000 and 10,000 euros in Panel (b), with vouchers only up to December of a year between 2012 and 2014. By restricting the population to individuals with similar AWAs income dynamics in the pre-treatment period, we aim to compare individuals who are comparable in their income trends before the treatment. This way, we can verify whether the absence of the treatment would have led to the same income dynamics in the absence of the treatment. If our results hold, this would indicate that our specifications are robust to miss-specification of the control group and would mitigate concerns about sample selection bias.

Empirical specification To improve the identification of the discontinuity, we employ a monthly-level panel. Specifically, we compare the incomes of workers who have already reached the threshold in each month of each year with the incomes of those who have not yet reached the threshold but will by the end of the year. We provide a sketch of the event study structure in Figure B8 in the Appendix. To identify this effect, we estimate the following equation:

$$Y_{i,m} = \delta_i + \beta_m + \sum_{m=M_i-k}^{M_i-1} \gamma_m I_m + \sum_{m=M_i}^{M_i+h} \alpha_m I_m + v_{i,m} \tag{5}$$

, where i indexes workers, while m and y months and years. Y stands for AWAs income, standard labor income, welfare transfers and total income (transformed with the IHS function as standard). Estimates are hence semi-elasticities. δ_i and β_m are instead worker and month FE. We cluster standard errors at an individual level and set the number of bootstrapped replications for the baseline estimates at 50. We don't include the standard cubic polynomial in age as in the previous specification because the estimator of Chaisemartin and D'Haultfoeuille 2020 does not support the inclusion of time-varying unit-level controls.

[24]We direct to De Chaisemartin and D'Haultfoeuille 2022 for an extended discussion.

I_m is a dummy variable equal to 1 in month m and 0 otherwise. M_i indicates instead the specific month in which the individual experiences the event, that is the month in which the individual i reaches the 6,667 euros threshold. H and k are the number of months following and foregoing the event: γ_m and αm thus estimate respectively the pre-trends in k months prior to the event and the dynamics effects up to h months after that the event has occurred. Workers might need several months to seek new income sources, especially considering the well-known frictions characterizing these segments of the Italian Labor Market, and they would not even find them if AWAs actually are an effective tool to enter the labor market.

Ideally, all γ_ms must not be different from 0, as we need to address the pre-trends in order to check the validity of the Parallel Trend Assumption. In other words, we test whether individuals experience any reduction in AWAs income before the event happens. If we are able to reject the hypothesis, we can reasonably assume that in absence of the treatment the trends in the two groups would have been the same, which is equivalent to test the validity of the PTA in a standard Difference-in-Difference framework. This is the only, crucial, assumption underlying our strategy.

Results Results, displayed in Figures 5.5 and 5.5, point in the same direction. The pre-trends in both the figures hold always, unless for AWAs income in Panel (b). This happens as we are focusing on a sample of individuals that lay on the right tail of the AWAs income distribution, and thus increasing the bandwidth easily translates into a change in the results because we compare individuals with very different values of voucher income. This ends up in the violation of the pre-trends that is absent in Panel (a), where indeed we focus on a more homogeneous population in terms of AWAs earnings. However, for our main outcome of interest, overall income, all the placebo dummies are not different from zero. All in all, the pre-trends are respected, and hence the main assumption on which our identification strategy relies on holds.

Results in both panels indicate that, when the event occurs, workers experience a decrease in voucher income that shortly after translates in a loss of overall income. The effect monotonically increases over time, with basically no effect on labor income and welfare transfers. The sudden drop in voucher income that shows up in both figures when the event occurred ensures that, even though the threshold is not strictly binding, we rely on an exogenous shock. The only difference between the estimates across the two panels is that, in (a), overall income displays a U-shape, as it starts to climb up after several months since the event occurred.

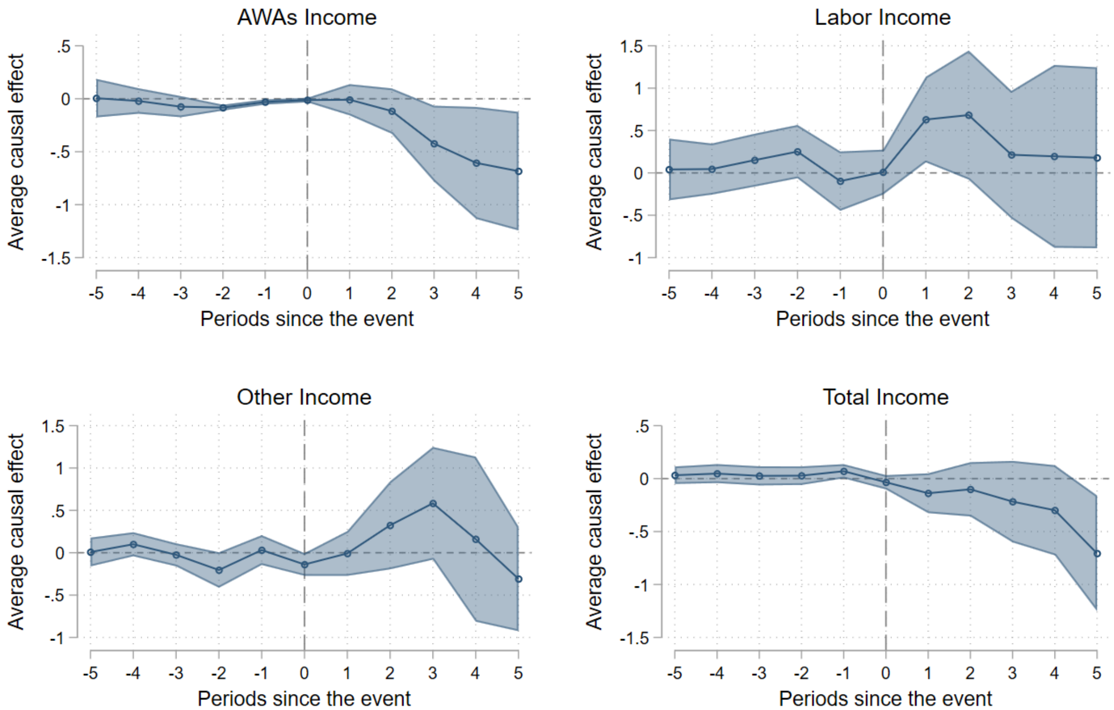

Figure 5: **Chaisemartin and D'Haultfoeuille 2020 estimates of Equation 5 in Panel (a).**

Notes: Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2012 and 2014 with vouchers. Estimates are semi-elasticities. Model is estimated with 50 bootstrap replications. Observations are 13,085 workers-month tuples associated to 1,151 workers. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

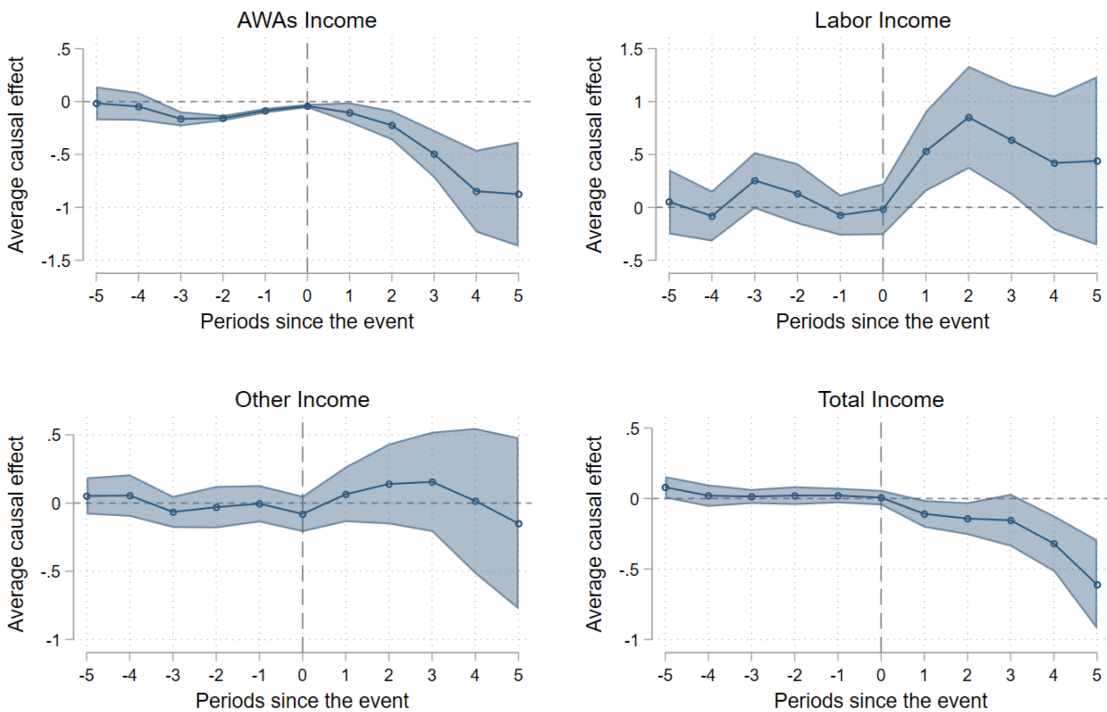

Figure 6: **Chaisemartin and D'Haultfoeuille 2020 estimates of Equation 5 in Panel (b).**

Notes: Population is made of those individuals whose earnings are between 5,000 and 10,000 euros in a year between 2012 and 2014 with vouchers. Estimates are semi-elasticities. Observations are 13,085 workers-month tuples associated to 1,151 workers. Model is estimated with 50 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

This is presumably caused by the slight increase, in the bottom-left graph of Figure 5.5 and 5.5, that appears in the short-run ($\sim$ the first three months) for labor income and welfare transfers[25]. This fact suggests that social protections cannot keep up the loss in income of these individuals over time and they are ineffective to protect them. Considering labor income, in the upper-right panel of both figures, it appears that after the event coefficients tend upward, and in Panel (b) are even significant in the short-run. This can be interpreted as a sign of individuals trying to find alternative income sources by exploiting standard employment forms anticipating the attainment of the threshold, but with no, or little, success.

To test the robustness of the results, we perform several exercises on two directions: first, we increase the number of bootstrap replications to improve the estimates of the standard errors, and, second, we increase the number of periods preceding and head of the event to further asses the validity of the PTA

[25]This drop is presumably due to the fact that the Italian INPS-provided CIGO ("Cassa Integrazione Guadagni Ordinari"), the main short-term unemployment transfer in the Italian legislation that we observe in our data, is provided in its basic scheme for 13 weeks ($\sim$ 3 months), that can be prolonged up to a maximum of 52. Unfortunately, we are not able to disentangle the two components of the CIG and hence perfectly assess whether this is the case.

and the long-run effects. Estimates, displayed in several figures in Section 6 in the Appendix, confirm previous ones. In this exercise we found that when vouchers access is denied workers cannot replace them. It seems that workers struggle to remain in the labor market but are inexorably pushed out: vouchers are no longer feasible but alternative income sources cannot be exploited and welfare transfers cover only the short-run. The combined effect after few months is the decrease in overall income that becomes larger over time, and can be followed by the exit from the labor market or by the shift towards illegality[26].

This evidence enriches the results obtained in the previous section, as it identifies a different use of vouchers by different sub-samples of workers. In other words, AWAs seem hence to be the only tool for weakest workers to stay in the labor market and gather an income. Our results indicate that limiting by law the possibility to rely on AWAs beyond a given threshold, a choice motivated by the concern that employers exploit them to replace standard employment, does not help workers to find alternative income sources but rather shuts down their main income pipeline. As a consequence, workers inexorably lose income until they possibly exit from the formal labor market.

Robustness check In order to test the robustness of the results obtained from the event study, we conduct two exercises.

Placebo test. The first exercise consists in estimating the event study when the threshold is not in force. To accomplish this, Equation 5 is estimated for the period spanning from January 2008 to December 2011[27]. Assuming that the kink in voucher earnings is an exogenous shock, we would not anticipate observing any impact in the preceding period. Nonetheless, the equation is estimated on a smaller subsample since only a limited number of individuals reached the maximum earnings threshold in the early years of the voucher program, with the majority of observations concentrated in 2011 and none in 2008. This could potentially pose a challenge for the estimator employed by Chaisemartin and D'Haultfoeuille 2020, given that it relies on the bootstrap technique.

In order to further test the robustness of our findings, we estimate the model using a standard Two-Way

[26]Unfortunately, we cannot investigate with our data whether these individuals remain in the labor market informally, namely working undeclared. This presumably happens in many cases, as indicated by empirical evidence (Di Porto et al. 2022).

[27]The time span under consideration does not include the last available year, 2015, even though the threshold is no longer valid. This is due to the likelihood of a persistence effect, particularly in the first few months of the year, which would invalidate the placebo exercise. The reason for this is that the elimination of the threshold for voucher earnings was part of a much larger and significant set of labor market reforms that were introduced with the Jobs Act Reform, which was finally approved in March of 2015. This resulted in vouchers being made available for all types of activities, leading to an exponential increase in their use. Thus, the first empirical issue is the need to disentangle the two effects, which would be quite challenging and may ultimately not be possible. Moreover, the threshold was still in effect until March, and following the abolition, firms require time to adapt to the changes, particularly in a context where numerous significant changes were introduced. As a result, the year 2015 is not included in the placebo estimates.

Fixed Effect estimator. With respect to the Chaisemartin and D'Haultfoeuille 2020 estimator, a TWFE model would underestimate the size of the standard errors induced by both the staggered treatment and the absence of the never takers in this framework. Thus, not rejecting the null hypothesis of non-significance in this case enhances the reliability of our robustness check. The results, which are presented in Figure B15 in Appendix 6, demonstrate that the estimates are not significant, even though the estimator underestimates the magnitude of the standard errors.

The left panel of the figure, which pertains to voucher earnings, reveals a declining trend, possibly due to the fact that voucher income tends to decrease when it is very high, owing to the types of work activities covered by vouchers. However, a comparison of these estimates with those obtained using a more conservative estimator for the period 2012-2014 exposes a significant disparity: the coefficients are always significant and range between 0.5 and 1 in Figure 5.5, whereas they are consistently and considerably insignificant and disproportionate in Figure B15.

Aggregated coefficient. The second concern pertains to the possibility of the estimate becoming non-significant as the number of bootstrap replications increases. Furthermore, the event study estimates a distinct coefficient for each month, reducing the total amount of variability that the estimator can employ to compute standard errors. This results in a scenario similar to that depicted in Figure 5.5. To address this issue, we estimate a single post-coefficient as the average of all post-period dynamics effects, while increasing the number of bootstrap replications[28]. Our results, presented in Figure B16 in the appendix, indicate that, when the total amount of variability is utilized and even with large confidence intervals, the estimates are significant.

To conclude, the results of these exercises indicate, in our view, that the estimates are robust.

6 Conclusions

This article contributes to a new and unexplored area of literature that investigates the role of AWAs in the labor market, using a sample of Italian AWA users' vouchers between 2008 and 2015. We address an unanswered question in this literature: whether AWAs act as substitutes or complements to welfare transfers. This question is relevant as AWAs have become increasingly popular in many European countries, particularly among low-wage workers and those who are marginalized. Moreover, we explore whether the abolition of vouchers in 2017 increased or decreased workers' earnings. Our findings suggest that the role

[28]We set the number of bootstrap replications at 400 and 500. However, we perform the exercise also increasing the number of replications up to 999 and the results are unchanged (7 periods head, AWAs income, DD coefficient=-0,416, SE=0.239).

of AWAs largely depends on workers' labor supply and the legislation regulating their use.

We make a methodological contribution by utilizing an estimator developed in Semykina and Wooldridge 2010 and Wooldridge 1995 that addresses the issue of unobserved, time-varying individual heterogeneity influencing workers' choices between AWAs and standard employment forms. This allows us to account for sample selection bias arising from the fact that we observe earnings dynamics of a representative population of AWAs users who have different elasticities compared to the entire Italian working population. Our Fixed Effects estimates indicate that AWAs complement overall income with an elasticity of approximately 0.74 p.p. and substitute standard labor income with an elasticity of -0.02 p.p.. We thus find that AWAs increase overall income and marginally reduce labor income.

However, the positive effect of AWAs on overall income is significantly reduced when implementing the CRE estimator, as the elasticity drops to 0.105 p.p., while the negative effect on standard labor income becomes much more pronounced at -0.26 and -0.05 p.p. This highlights the importance of taking into account sample selection bias in our analysis. Moreover, we find that the effects of AWAs are more pronounced for immigrants and men, and increase with age, indicating that the utility of vouchers is higher for workers with more working experience. In a simple simulation based on our estimates, we find that the abolition of vouchers led to an average loss of only 10% in overall income, but an increase in labor income and welfare transfers by 26% and 5%, respectively. Considering the lower social protection provided by vouchers and the higher fragmentation of working careers, our results suggest that the abolition of vouchers did not significantly harm workers' welfare.

We complemented our analysis by conducting an event study that exploits a discontinuity in voucher income, targeting a sub-sample of highly intensive AWA users between 2012 and 2014. Our results indicate that, after reaching the threshold, workers experience a sudden fall in AWA earnings, leading to a time-increasing loss in overall income. However, we did not observe any significant increase in welfare transfers or labor income in the aftermath of the event, indicating that the welfare system cannot adequately protect these individuals when the legislation forbids the use of AWAs.

Overall, we found that AWAs did not enhance workers' earnings, and for the specific sub-sample of high-intensive users, their abolition did not lead to an increase in welfare transfers or labor income either. We believe that the policies applied to vouchers have been proven wrong, and instead of liberalizing and then abolishing them, legislators should have focused on targeting workers and areas in which vouchers have been more useful while limiting their spread in the whole population. Our findings are particularly relevant from a policy standpoint, given that the Italian government has reintroduced vouchers since 2023. In conclusion, we suggest that future research should focus on opening the "black box" of unobserved heterogeneity that drives workers' use of AWAs, address AWAs' demand and supply jointly, and identify how employers' power drives AWAs' use (Di Porto et al. 2022; Datta, Giupponi, and Machin 2019).

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Contents

Appendix

A Literature

The literature on AWAs is generally scarce, but few works have been done, with different approaches and addressing different research questions. They all find common patterns: The use of AWAs has increased sharply in recent years, Italy included, and is associated with a reduction in workers' wages and an increase in career fragmentation.

Katz and Krueger 2019 find that, over the 10 years between 2005 and 2015 in the US, the proportion of workers engaged in some form of alternative work arrangement grew by 10-20%. Adams, Prassl, et al. 2018 provide a picture of AWAs in the UK, highlighting how they become widely used in 2011, the dumping effect on earnings, the sectors where they have been mostly used, and that among those people relying on them, there are many students and foreign performing low value-added and occasional duties.

Boeri and Garibaldi 2007 find that AWAs are relevant in several labor markets, that they can be hardly measured and definable because they share features with both standard employment and self-employment. Moreover, they also find that they are characterized mainly by a unique employer making workers vulnerable to idiosyncratic shocks and that most of the workers ask for higher social protection.

They also prove that both in Italy and UK AWAs substitute fixed-term contracts, weakening workers' careers and earnings with a negligible effect on employment and aggregate welfare. In short, these contracts pose challenges to institutions and might have several impacts on workers' welfare.

Another stream of research focused on the causes of AWAs widespread. Katz and Krueger 2017, analyzing atypical labor offers in the US, find that the main factor driving the supply side of the atypical labor market is a weakening of labor market institutions and that the experience of unemployment raises the likelihood that workers exploit alternative work arrangements later on. Garen 2006 analyzes AWAs in the U.S. labor market and finds that is a combination of unskilled work and the optimality of firm control of the work routine that leads to their widespread. Another stream of research focuses on whether AWAs are useful in reintroducing jobless workers into the labor market, finding positive effects.

Farber 1999 examines the rates of AWAs employment among displaced workers: results support the use of AWAs as a means for these workers to acquire at least a degree of employment continuity either through the direct conversion of temporary into regular work or by increasing the individual's employability. J.T. and C.J. 2006 find that AWAs are in part a stepping stone into regular employment providing employers with either a direct or indirect screen. Addison and Surfield 2007 study the wage gap between AWAs users and not in the US: results find a wage differential of 7-12 p.p.. They mainly attribute the latter disparity to unobserved worker heterogeneity, as they do not set up a bullet-proof identification strategy. This suggests that unobserved workers' characteristics do play a relevant role in influencing the use of AWAs: this is then a primary concern in our framework as the estimated income elasticities would thus be biased in a standard fixed effects regression model.

Heinrich, Mueser, and Troske 2009 investigate the temporary help service (THS), a particular type of AWAs available in the US. Two arguments play out: (i) employment through THS firms may provide a path to permanent and stable employment for workers excluded from the labor market; (ii) THS substitute productive employment search and reduce access to better employment opportunities and reduce workers' wages. The authors find that (i) coming through the employment exchange, individuals fare better in terms of earnings and earnings growth when they take jobs with the temporary help service; (ii) THS expand access to employment networks for individuals seeking jobs.

Some researchers study instead the composition of AWAs users and the consequences of AWAs exploitation on their careers and income dynamics. Overall, findings raise concerns on AWAs widespread. Dolado, Lalé, and Turon 2021 focus on the UK AWAs, the Zero-Hours Contract (ZHCs), finding that they increase the unemployment rate in the low-pay labor market of 2-3 p.p. and reduce the employment rate of the low-pay labor market. This difference comes from the labor force participation effect of ZHCs. They also find that some workers in the low segment of the labor market would prefer not to participate if there were no flexible contracts, such as the ZHCs, providing access to flexible arrangements. They additionally find that regular employment expands and that the average duration of spells decreases. They believe that these instruments serve as a stepping stone towards regular employment, implying however more unstable trajectories. Overall, they concluded that a ban on ZHCs would generate welfare losses among low-wage workers.

Farina, Green, and McVicar 2021 investigate the population of UK AWAs (known as "Zero Hours Contracts") users exploiting administrative data: they find that ZHCs jobs have become increasingly concentrated among young workers, full-time students, migrants, black and minority ethnic workers. We will also explore this heterogeneity in our framework. They also find that (ii) median wages in ZHCs jobs have also fallen over time; (ii) part of the reported growth in ZHCs has been driven by a reclassification of existing employment relationships. These latter two facts could also be tested in our framework, as we can observe workers' income dynamics and contract types before and after AWAs use. They also address the extent to which the growth in reported ZHCs has been driven by increased awareness, proxied by national newspaper articles and Google searches for ZHCs, claiming that it cannot be rejected.

They conclude by listing several unanswered questions regarding AWAs, from whether banning ZHCs might simply displace workers into alternative forms of job or not, or what the relationship with labor market features such as minimum wage. We address the former question by investigating workers' income dynamics and how AWAs interact with fixed-term contracts. Datta, Giupponi, and Machin 2019 focus on the UK investigating the relationship between minimum wage introduction, monopsonistic power and AWAs widespread. They highlight that a minimum wage enhancement is positively correlated with higher use of AWAs, suggesting thus that they are considered by firms as an escape strategy to a higher fixed cost of labor. They also highlight that AWAs use is concentrated in those sectors characterized by higher labor concentration and lower unionisation rate.

Specifically in Italy, where AWAs are known as the vouchers, only two works have been done. Anastasia, Bombelli, and Maschio 2016 provide a picture of voucher use, finding that they have been used more in northern Italy and industries characterized by a higher share of illegal work. This seems to suggest that vouchers have contributed to a partial emersion of illegal jobs. Di Porto et al. 2022 rely on random labor inspections combined with administrative microdata finding that firms tend to use vouchers particularly after labor inspections. They interpret this fact as a proof that vouchers were used by employers as a tool to cover non-legal job performances, and that they crowd out FTs spells. The main contribution of this work is that it is one of the few to set up a strong identification strategy.

In summary, the literature shows that the effect of AWAs is not straightforward, as it depends largely on the characteristics of the labor market and of the workers. Therefore, their impact on workers' careers, welfare and undeclared jobs (in terms of earnings and pensions also) still must be investigated in different frameworks focusing carefully on feasible heterogeneity across individuals and labor markets with different features.

B Jobs Act and vouchers' traceability

The Jobs Act introduced in 2015 a strict protocol to be followed by employers whenever they used a voucher, aimed at reducing the abuse of vouchers and better targeting controls to check whether they actually were used for the purposes for which they were designed. Non- agricultural employers must communicate by email or SMS to the local headquarter of the "Ispettorato nazionale del lavoro" the details of the voucher's user, the place, and the length of the spell. Agricultural employers must instead provide the information in a span of 7 days. In case of violation, they might be fined by an amount ranging between 400 to 2,400 euros. This procedure was aimed at hindering the abuse in vouchers use as a tool to cover illegal work and reduce costs.

C Additional Materials

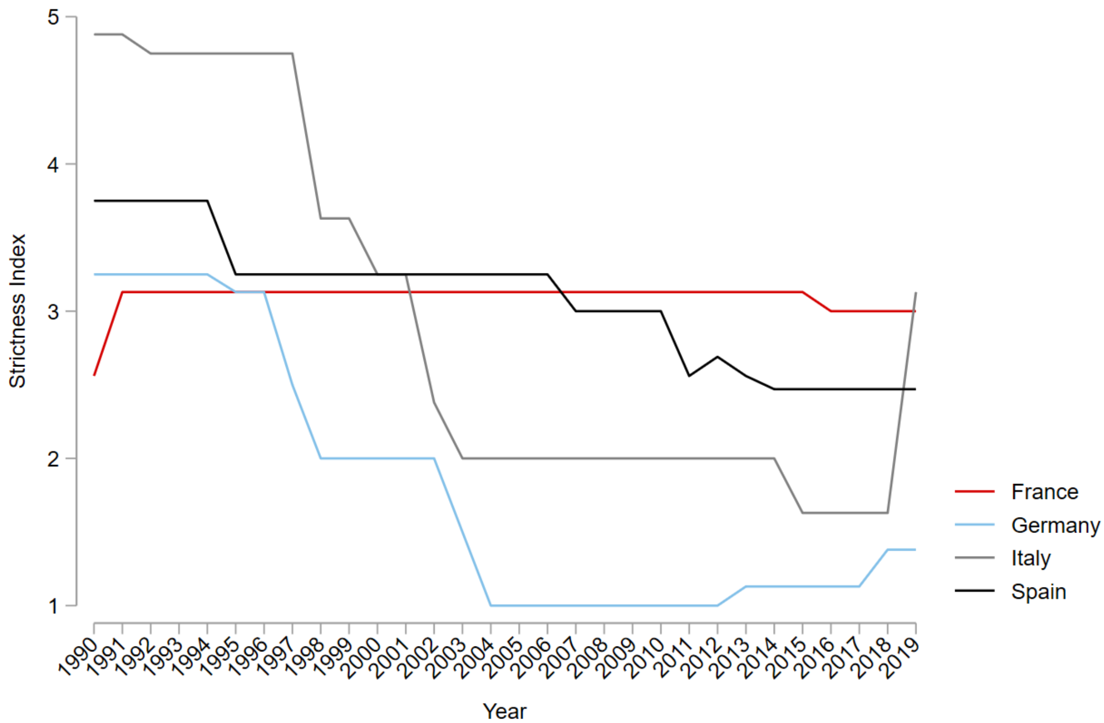

Figure B1: **Strictness of EPL on temporary contracts in France, Germany, Italy and Spain between 1990 and 2019.**

Notes: Authors' realization on OECD data.

C.1 Descriptive Evidence - Additional materials

In this section, we attach the figures and the tables to which we refer in the Descriptive section in the main text. For a more detailed discussion, and more descriptive evidence, on vouchers' users and their characteristics, we redirect to Passerini 2017 and Anastasia, Bombelli, and Maschio 2016.

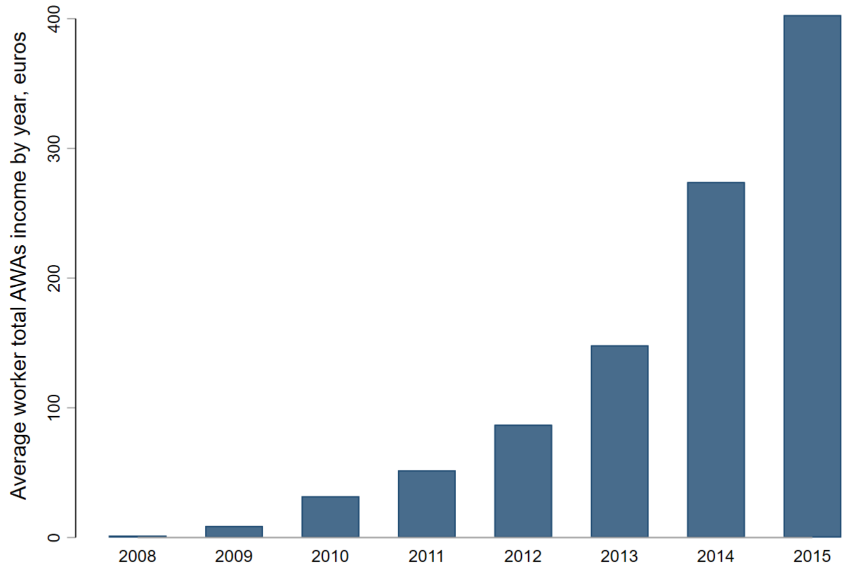

Figure B2: **Average worker voucher income, by year**.

Note: Our sample goes from 2008 to 2015. Calculations are based on the full population, so regardless of workers' age. Data have previously been cleaned to remove all workers above 99^{th} percentiles in vouchers and labor earnings. The panel is made up of 14,096,928 worker-month tuples associated to 146,843 workers.

Variables	Observations	Mean	St.Dev.
Panel (a): 146,843 workers			
Total Income	14,096,928	392.083	991.318
Standard Labor Income	14,096,928	342.912	925.177
Welfare transfers	14,096,928	38.690	308.544
Voucher Income	14,096,928	10.480	79.758
Total Income (less voucher)	14,096,928	381.602	989.520
Total Income, >0	3,244,214	1,703.698	1,426.720
Age	14,096,928	36.017	14.655
Male	-	56%	-
Italians	-	77%	-
Panel (b): 146,843 workers			
Total Income	5,286,348	397.318	987.486
Standard Labor Income	5,286,348	327.429	895.663
Welfare transfers	5,286,348	55.743	378.390
Voucher Income	5,286,348	14.145	92.042
Total Income (less voucher)	5,286,348	383.172	984.950
Total Income, >0	1,336,057	1,572.058	1,418.277
Age	5,286,348	37.517	14.497
Male	-	56%	-
Italians	-	77%	-
Panel (c): 82,055 workers			
Total Income	2,929,392	384.319	957.172
Standard Labor Income	2,929,392	308.902	866.662
Welfare transfers	2,929,392	51.887	360.642
Voucher Income	2,929,392	23.530	117.660
Total Income (less voucher)	2,929,392	360.789	952.285
Total Income, >0	535,002	1,390.399	1,384.096
Age	2,929,392	36.837	13.056
Male	-	60%	-
Italians	-	75%	-

Source: Authors's calculation on *INPS Estratti Conto* archive.

Table A1: **Summary Statistics at a monthly-level in three different samples.**

Notes: Regarding the variable age some values are extreme as we fill the panel with all missing months in the period 2008-2015. Several workers appear thus in our dataset even though they do not have any income at all. Data have been previously cleaned to delete all workers above 99^{th} percentiles in vouchers and labor earnings, and for this reason the number of workers is lower than in the original data source. Panel (a) contains all workers between 2008 and 2015, (b) only between 2012 and 2014 while in (c) we select only the workers with at least a voucher spells between 2012 and 2014 belonging to the active population, so of age ranging between 16 and 64 years.

Year	Mean	Median
2008	1646.247	1475
2009	1642.696	1475
2010	1674.405	1508.333
2011	1696.467	1533.333
2012	1697.849	1525
2013	1727.449	1558.333
2014	1742.283	1558.333
2015	1746.868	1558.333
Total	1696.075	1516.667

Table A2: **Summary statistics of gross monthly wages in the Italian private sector.**

Notes: Authors' calculations based on the INPS *LoSaI* archive, which describes the income careers of approximately 10% of the Italian workers' population of the private sector from 1985 to 2018. *LoSaI* contains all working spells of a sample of workers. We compute the average and median wages first by collapsing the dataset at a worker and yearly level summing the remuneration of all spells within the same year for each worker separately and then dividing the gross yearly remuneration by 12.

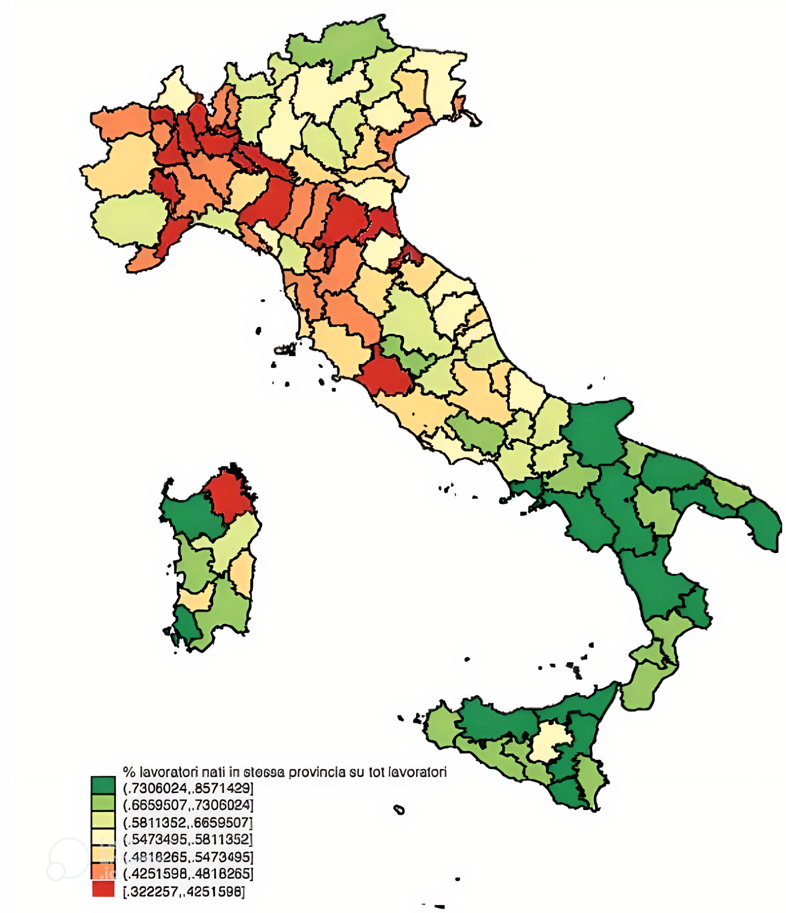

Figure B3: **Map of vouchers use with respect to workers' province of birth between 2008 and 2015**.

Notes: Dark green colour indicates provinces in which the provinces of birth of workers coincide with those in which the vouchers are used. The opposite is true for lighter colours. *Source:* Cavallotti et al. 2020.

C.2 Selection Stage

The estimator is coded in Stata in Rios-Avila 2020. We start by commenting the main predictor at the 1^{st} stage: individual working experience. As previously explained, the model automatically includes at both stages the individual mean of each variable, and hence joint with the individual's working experience, there is also the mean, across individuals and within time. Interestingly, the two predictors of interest switchs signs in the second quarter of 2013. This happens for a simple reason: in that quarter the number of quarters worked by individuals on average overcomes the individual mean of working experience, and thus for a statistical reason the Probit model the two coefficient switch signs.

To understand further these results I run directly several logit and probit models including the two covariates in exam. When included separately in the models the two variables always show positive and sizeable coefficients, indicating thus that individual working experience and individual-mean working experience are powerful predictors of individuals' selection into the labor market, as predicted by a vast literature, throughout the period of analysis. Additionally, in line with theoretical predictions, as long as we move on over time, the more intense coefficients become, indicating thus that as workers gather experience they are more likely to stay in the labor market. In other words, the more workers acquire experience, the more it becomes capable of predicting their participation in the labor market.

We do not attach the results, but all the variables coherently predict the probability of accessing the labor market: men have a higher likelihood than women, Italians higher than immigrants, and finally it increases in age squared and decreases in the linear term. The cubic term has instead a negligible effect. In this framework, a preliminary check for the presence of selection bias can be performed by looking at the significance of the inverse Mills ratios across all specifications. They are all significant, so the null hypothesis is always rejected. Moreover, their significance and intensity increase over time, suggesting that workers' endogenous selection in the labor market becomes more relevant as far as we move on over time.

These findings serve as a test to check the presence of selection bias in both the OLS and FE specifications, as in a context of random selection, where individuals select into the labor market independently of their own and labor market characteristics, they would all be not different from zero. These findings corroborate the need to exploit this method also. All in all, previous evidence indicates that selection into the labor market is a dynamic that needs to be modelled in our framework and that it can be explained by a set of time-varying and not, observed and not, individual-specific characteristics. We thus expect the estimates to differ with respect to those obtained with OLS and FE models.

C.3 Robustness check: different definition of working experience

We assess the results' robustness by changing the way we define workers working experience. So far, we have calculated working experience as the cumulative sum of worked quarters by each worker from the beginning of the sample, q1-2008, to the one considered. We now define instead a rolling window in which we calculate working experience, respectively of two, three, and four years prior to each quarter considered. We want to check whether our results are driven by the presence of workers more likely to be in the sample because of unobservable determinants or age. By considering a rolling window, we likely mitigate the presence of this feasible bias.

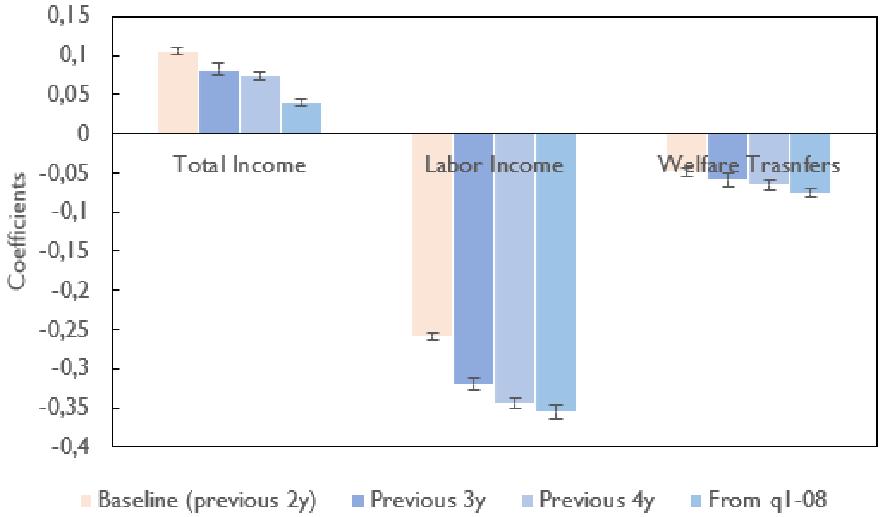

Figure B4: **CRE estimates with alternative definitions of working experience.**

Notes: The bars refer to different types of working experience: from left to right, we extend the rolling window of the individual cumulative working experience to 3, 4 years, and from q1-2008. The specification is estimated on the active population sample, individuals ranging from 16 to 64 years according to OECD, of those who worked at least once with vouchers in 2012-14. Estimates are obtained with the CRE estimator. The dependent and independent variables are all transformed with the inverse hyperbolic sine function, and hence should be interpreted as asymptotic elasticities. First stage predictors include sex, a cubic polynomial in age, nationality, and a polynomial in individual working experience. Individual-mean for all controls are included. Period goes from q1-2012 to q4-2014. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

D Heterogeneity

D.1 Context and empirical specification

Based on the empirical evidence contained in Section 1, we improve the specification by exploring heterogeneity by sex, nationality, and age cohorts. Anastasia, Bombelli, and Maschio 2016 prove that voucher use has been heterogeneous across different sub-samples of individuals, especially by age and nationality, while Datta, Giupponi, and Machin 2019 find that AWAs become more relevant in low-wage segments of the labor market, those with more immigrants and women, after an increase in the minimum wages in UK. In general, the stream of research focusing on the Italian Labor Market has highlighted that Italy is characterized by a relevant spatial and industrial in productivity and wages. The less productive local labor markets are also those where mostly immigrants concentrate, and thus those in which we might expect, according to theoretical predictions, that AWAs play the most relevant role.

These predictions are confirmed by Di Porto et al. 2022 when they find vouchers to cover undeclared jobs, especially in southern provinces and in less productive industries, those where more immigrants and women are concentrated. We hence expect vouchers to have a more detrimental effect for women and migrants. In case instead the opposite is valid, so that vouchers help to uncover undeclared work and as a way to reenter the jobs market after an unemployment period, we expect to observe a less negative effect for women and immigrants than for men and Italians. We then estimate the following equation:

$$Y_{i,q} = \delta_i + \phi_q + \sigma Age_{i,q} + \alpha Age_{i,q}^2 + \lambda Age_{i,q}^3 + \sum_k \theta_k \left(X_{i,k,q} \times 1\{i \in k\} \right) + v_{i,q} \tag{6}$$

, which is identical to the previous but estimates separate specifications by workers' types. θ_k estimate then the average partial correlation - expressed as an elasticity - between the outcomes of interest Y and the voucher income separately for workers belonging to each group k over the period of analysis, 2012-2014.

D.2 Sex, Nationality, and Age

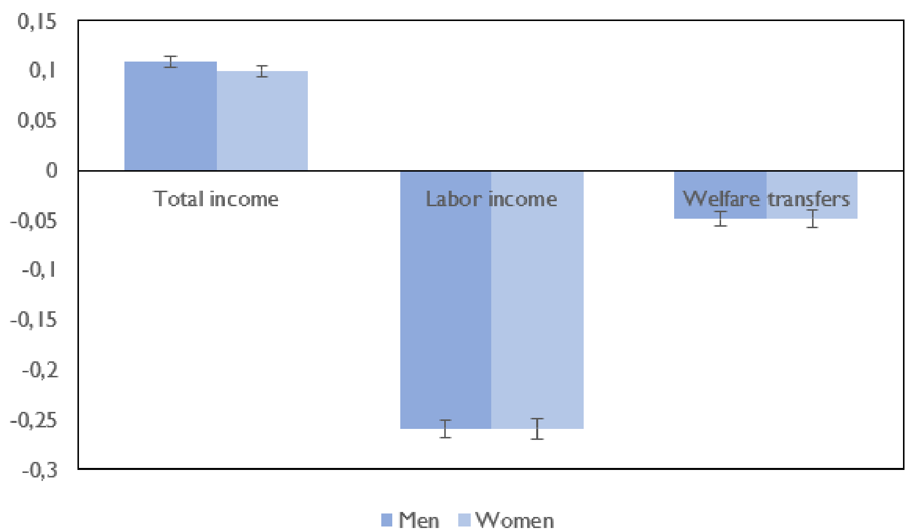

Figure B5: **CRE estimates of the elasticity of voucher income on all outcomes, by sex.**

Notes: The specification is estimated on the active population sample, individuals ranging from 16 to 64 years according to OECD, of those working at least once with voucher in 2012-14. The sample is further split into men and women. Estimates are obtained with the CRE estimator. The dependent and independent variables are all transformed with the inverse hyperbolic sine function, and hence should be interpreted as asymptotic elasticities. First stage predictors include sex, a cubic polynomial in age, nationality, and a polynomial in individual working experience. Individual-mean for all controls are included. Period goes from q1-2012 to q4-2014. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

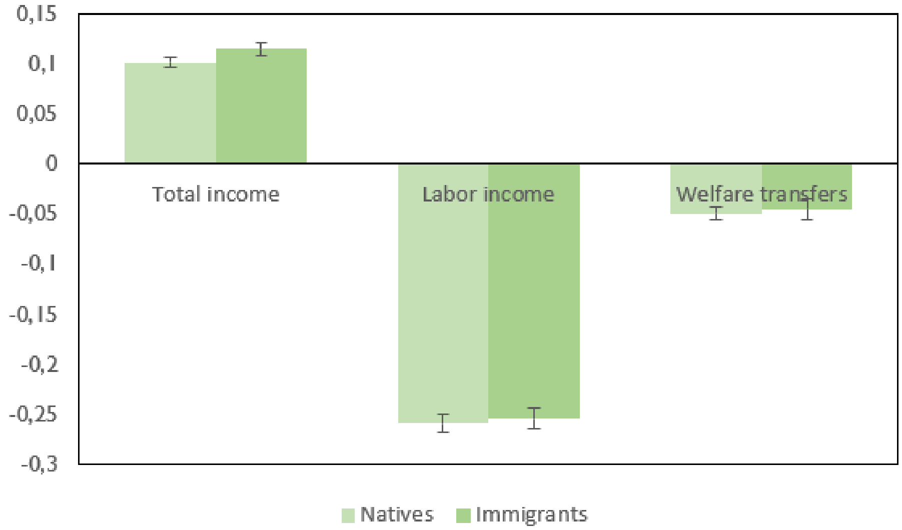

Figure B6: **CRE estimates of the elasticity of voucher income on all outcomes, by nationality.**

Notes: The specification is estimated on the active population sample, individuals ranging from 16 to 64 years according to OECD, of those working at least once with voucher in 2012-14. The sample is further split into Italians and non-Italians. Estimates are obtained with the CRE estimator. The dependent and independent variables are all transformed with the inverse hyperbolic sine function, and hence should be interpreted as asymptotic elasticities. First stage predictors include sex, a cubic polynomial in age, nationality, and a polynomial in individual working experience. Individual-mean for all controls are included. Period goes from q1-2012 to q4-2014. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

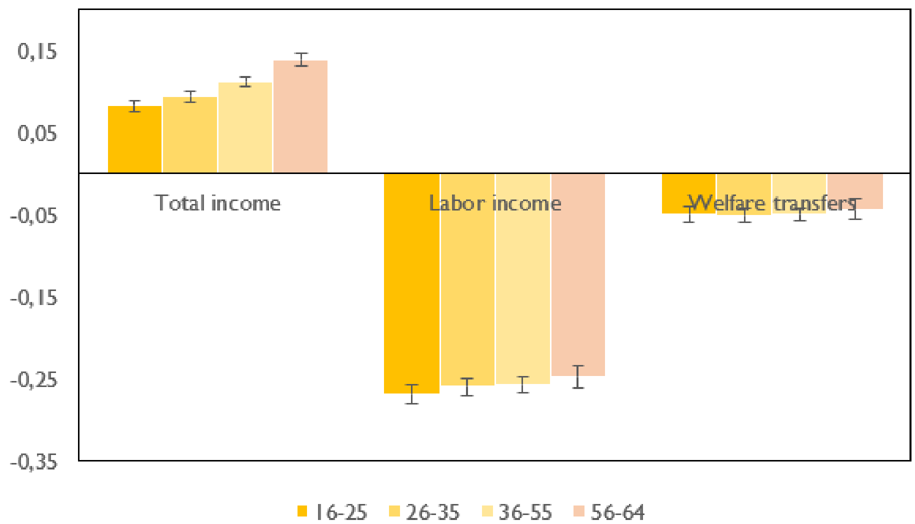

Figure B7: **CRE estimates of the elasticity of voucher income on all outcomes, by age cohorts.**

Notes: The specification is estimated on the active population sample, individuals ranging from 16 to 64 years according to OECD, of those working at least once with voucher in 2012-14. The sample is further split by age cohorts. Estimates are obtained with the CRE estimator. The dependent and independent variables are all transformed with the inverse hyperbolic sine function, and hence should be interpreted as asymptotic elasticities. First stage predictors in Column (3) include sex, a cubic polynomial in age, nationality, and a polynomial in individual working experience. Individual-mean for all controls are included. Period goes from q1-2012 to q4-2014. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

E Event Study

Here we provide a sketch of the identification strategy and the results of several robustness exercises that we perform in order to ensure the validity of the empirical strategy. Namely, we increase the number of bootstrap replications and the number of months in which we look at the pre-trends to ensure the validity of the PTA and those in which we test the dynamics effect of the treatment. The coefficients displayed in the Figures are semi-elasticities.

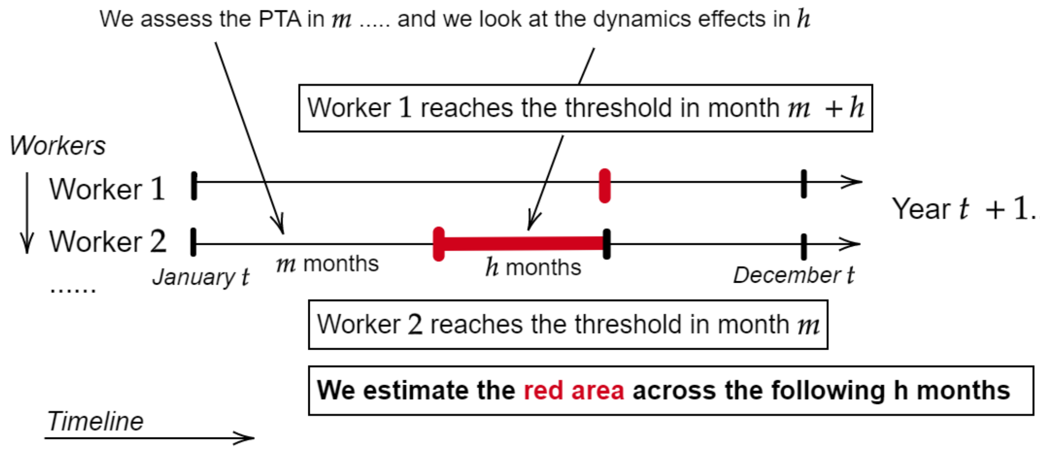

Figure B8: **Sketch of the Event Study identification strategy with 2 workers and 1 year.**

Notes: Authors' realization. The rows are the workers, while the continuous horizontal line represents the timeline. The event study starts when the first workers reach the threshold, month m, and ends when all workers reach the threshold (are treated), month $m+h$. The effect of the event, dynamically split by month, is graphically represented by the red highlighted area. PTA means Parallel Trend Assumption.

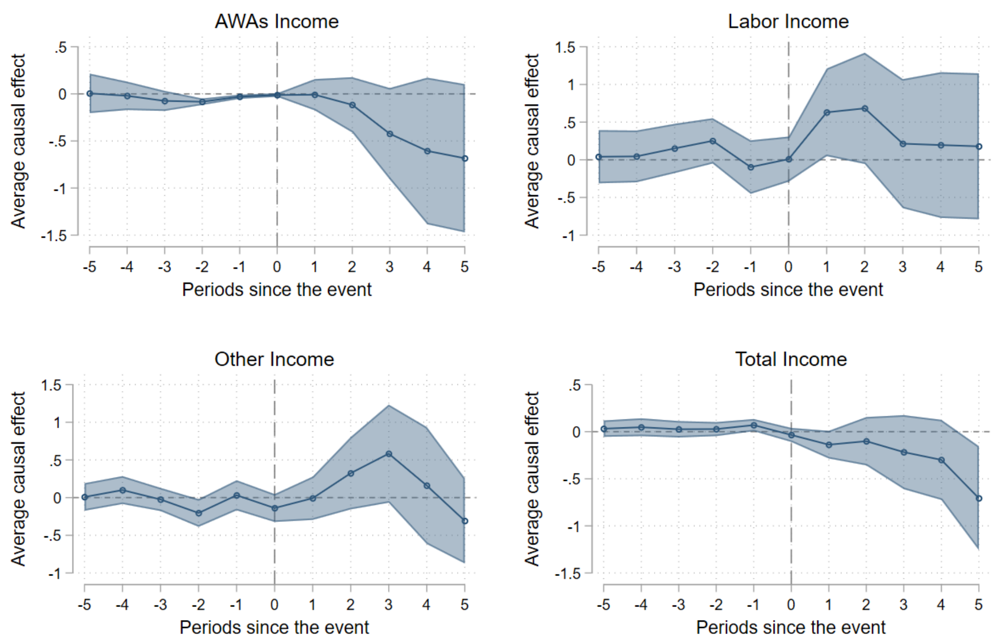

Figure B9: **Chaisemartin and D'Haultfoeuille 2020 estimates in Panel (a), 5 periods head and 200 bootstrap replications.**

Notes: Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2012 and 2014 with vouchers. Estimates are semi-elasticities. Observations are 13,085 workers-month tuples associated to 1,151 workers. Model is estimated with 200 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

Figure B10: **Chaisemartin and D'Haultfoeuille 2020 estimates in Panel (a), 5 periods head and 5 bootstrap replications.**

Notes: Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2012 and 2014 with vouchers. Estimates are semi-elasticities. Observations are 13,085 workers-month tuples associated to 1,151 workers. Model is estimated with 5 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

Figure B11: **Chaisemartin and D'Haultfoeuille 2020 estimates in Panel (a), 7 periods head and 5 bootstrap replications.**

Notes: Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2012 and 2014 with vouchers. Observations are 13,085 workers-month tuples associated to 1,151 workers. Model is estimated with 5 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

Figure B12: **Chaisemartin and D'Haultfoeuille 2020 estimates in Panel (a), 7 periods head and 20 bootstrap replications.**

Notes: Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2012 and 2014 with vouchers. Estimates are semi-elasticities. Observations are 13,085 workers-month tuples associated to 1,151 workers. Model is estimated with 20 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

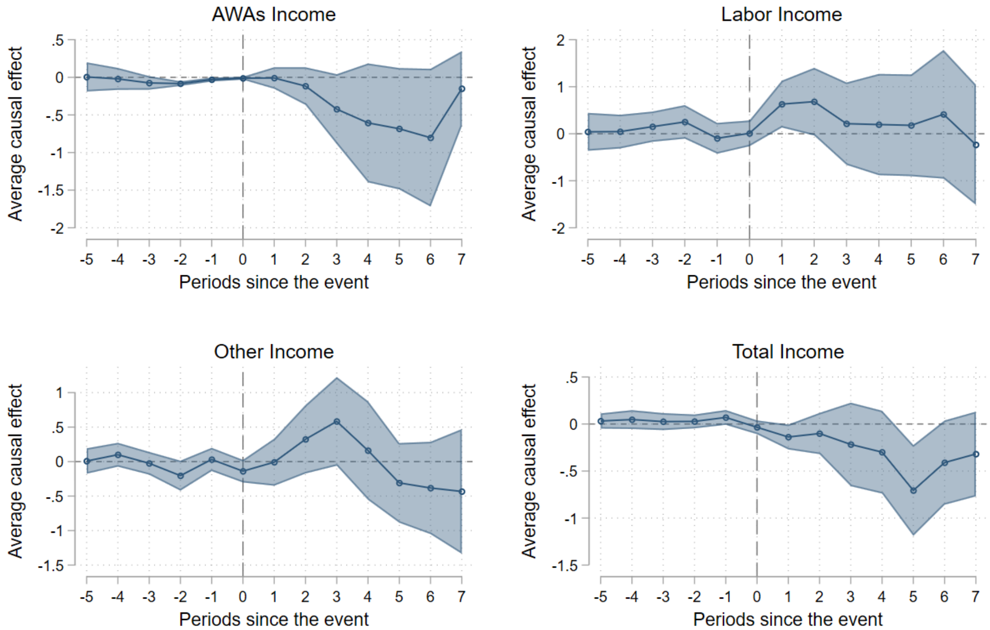

Figure B13: **Chaisemartin and D'Haultfoeuille 2020 estimates in Panel (a), 7 periods head and 100 bootstrap replications.**

Notes: Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2012 and 2014 with vouchers. Estimates are semi-elasticities. Observations are 13,085 workers-month tuples associated to 1,151 workers. Model is estimated with 100 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

Figure B14: **Chaisemartin and D'Haultfoeuille 2020 estimates in Panel (b), 7 periods head of the event and 50 bootstrap replications.**

Notes: Population is made of those individuals whose earnings are between 5,000 and 10,000 euros in a year between 2012 and 2014 with vouchers. Estimates are semi-elasticities. Observations are 13,085 workers-month tuples associated to 1,151 workers. Model is estimated with 50 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.

E.1 Robustness Checks

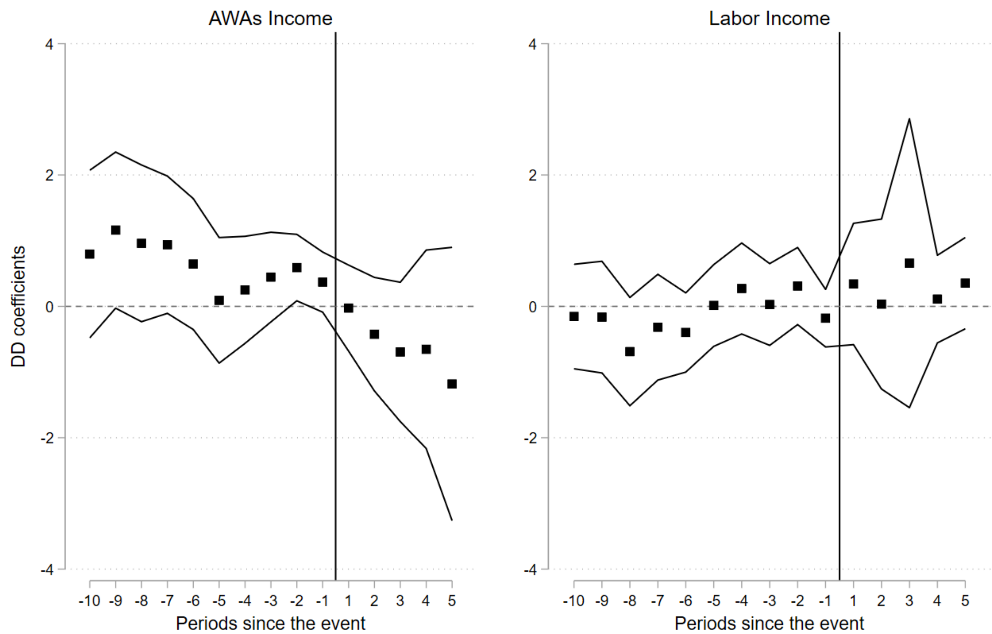

Figure B15: **Placebo estimates of the event study between 2008 and 2011.**

Notes: Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2008 and 2011 with vouchers. The squares represent point estimates, while the two solid lines are the 95^{th} confidence intervals. The DD coefficients should be interpreted as semi-elasticities, and are obtained by interacting the year dummies with the treatment dummy. Observations are approximately 3,000 workers-month tuples associated with approximately 1,000 workers. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings

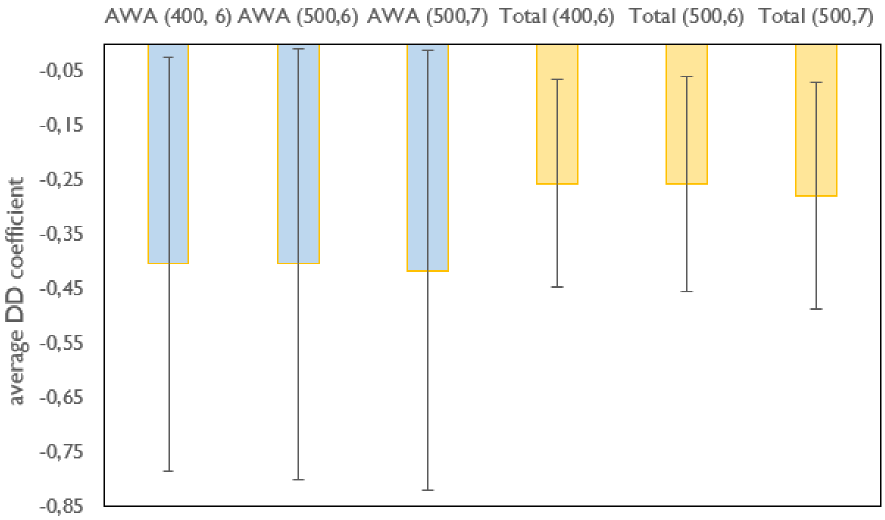

Figure B16: **Chaisemartin and D'Haultfoeuille 2020 average estimates of Equation 5.**

Notes: The first three bars (light blue) indicate AWAs income, while the last three (lighy yellow) indicate Total income. The empirical equation is displayed in Equation 5, with the difference that we calculate the average of the dynamics effects. In parenthesis, there are the number of bootstrap replications and the periods used to compute the dynamics effects. Population is made of those individuals whose earnings are between 5,000 and 8,000 euros in a year between 2012 and 2014 with vouchers. The columns represent the DD coefficients, while the bars represent the 90% confidence intervals. The DD coefficients should be interpreted as semi-elasticities, and are obtained as an average of all the dynamics effects. The dynamics effects are 5,6, and 7 months after the event. Observations are 13,085 workers-month tuples associated to 1,151 workers. The model is estimated with 400 and 500 bootstrap replications. Data have been previously cleaned to delete all those workers above the 99^{th} percentiles in total, voucher and labor earnings.