Working Paper no. **192**

Monopsony and rent sharing: evidence from Italian hiring subsidies

Lia Pacelli
University of Torino, CIRET and LABORatorio R. Revelli

Filippo Passerini
University of Milan and LABORatorio R. Revelli

March, 2024

LABORatorio R. Revelli, Collegio Carlo Alberto
Piazza Vincenzo Arbarello, 8 – 10122 Torino, Italy

Tel. +39 011 670.50.60
www.laboratoriorevelli.it – labor@ laboratoriorevelli.it

Monopsony and Rent Sharing: Evidence from Italian Hiring Subsidies*

Lia Pacelli[1] and Filippo Passerini[2]

[1]University of Turin; LABOR
[2]University of Milan; LABOR

March 20, 2024

Abstract

We estimate that subsidies reduce concentration in labor markets where small and large firms coexist. Wages mildly increase only when the HS are in place. Furthermore, workers retain only 4% of the 2015 subsidy amount, while firms 96%, indicating that even small ones have relevant wage-setting power.

Keywords: Monopsony; Labor Market Concentration; Hiring Subsidies; Wages.
JEL Classification: J42; J31; J38; L13; J48.

*We acknowledge *INPS* support for providing us with the *LoSaI* data. Thanks to the participants of the 15^{th} IAB "PERSPECTIVES ON (UN-)EMPLOYMENT" workshop, those of the LABOR seminar series 2023-24, and Salvatore Lattanzio, Claudio Lucifora, and Massimiliano Bratti. The usual disclaimers apply.

1 Introduction

A recent stream of literature uses market concentration to estimate monopsonistic dynamics[1]. As concentration is intrinsically endogenous to any labor market outcome, identification is challenging. We exploit the temporary availability of hiring subsidies (HS) and their variability in generosity across firms' sizes to overcome endogeneity and identify their impact on labor market concentration (HHI) and wages, providing a novel contribution to the literature.

2 Empirical Strategy

2.1 Hiring Subsidies and the treatment

In Italy, firms enjoyed a social security contribution rebate for all their new open-ended contract (OEC) workers (hired or incumbents whose contract was transformed) who had no OEC in the previous 6 months: a subsidy up to a cap of 8,060 euros per year for 3 years in 2015; and up to a cap of 3,250 euros per year for 2 years in 2016. HS were designed to target workers and therefore could be exploited by all firms. However, the cap makes HS generosity a function of firms' wage level, which is increasing with firm size. In particular, Ardito et al. 2023 show that there is a significant discrepancy in the hiring margin based on the 15-employee threshold: small firms (below 15 employees) used HS massively, while larger firms (above 15) reacted weakly to HS to hire new workers, and mostly used them to transform incumbents' contracts instead 2. Hence, our strategy leverages this discrepancy by comparing small (intensively treated) and large firms (weakly or not treated).

2.2 Data and Concentration

We exploit *LoSaI*, a dataset covering 10% of Italian private sector workers' careers. For each spell it records the start, eventual transformation and end date, gross daily wage, region of residence, occupation in five brackets, contract type, 2-digit industry, and size class in 14 brackets.

We select all new hires in 2010-2018 to compute a flow-based Herfindhal-Hirschman Index (HHI), more capable of estimating the evolution over time of monopsonistic dynamics (Marinescu et al. 2021) [2]. We define a labor market m as an interaction of industry s, occupation o and region r. We estimate concentration as follows:

[1] *e.g.*, Marinescu et al. 2021.

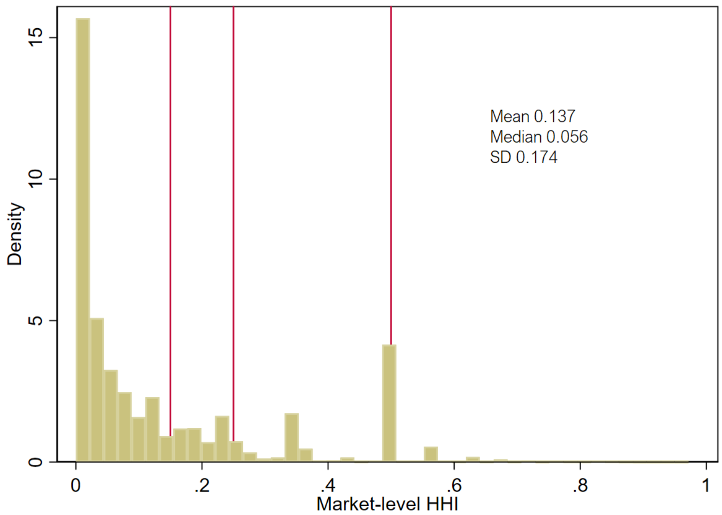
[2] We consider all hires and not OEC only to take into account potential substitution effects.

$$\text{HHI}_{m,y} = \sum_{N_{dm}} K^2_{d,y} \tag{1}$$

where N_{dm} represents the number of class sizes in each market m; K is the ratio of the number of new hires for the representative firm in class d in market m in year y to the total number of hires in m and y. 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 intuition behind the class size-based index is that monopsonistic dynamics can arise even in the presence of a large number of (small) firms, which would not be captured by an HHI based on shares. Monopsonistic dynamics arise when there are a few (large) firms, but also when, as is typical of the Italian labor market, there is a multitude of small firms[3] that on average pay low wages because not highly productive. This is particularly relevant in our framework since our identification exploits markets in which many small firms compete for hirings.

Figure 1 plots the index distribution: Most markets are weakly concentrated, while some are highly concentrated, due to industry heterogeneity. Figure A.1 instead plots the correlation between HHI and the share of hires of large firms: as expected the higher the share, the higher concentration; crucially, after the reform this relationship softens.

Figure 1: HHI distribution.



Notes. Observations are 30,365 market-year tuples: 4,716 markets observed from 2010 to 2018. Industries are 76 two-digit NACE Rev.2 cells, regions are the 20 Italians, and occupation is in 5 brackets. The red lines represent the standard US antitrust threshold for low (.15), medium (.25) and high concentrated (.5) markets. HHI is calculated in Equation 1.

[3] A firm-share HHI would yield a low value in this case, while a class size index would not.

2.3 The impact of HS on HHI

Following the argument in 2.1, we define a continuous treatment T_m indicating the share of hires in firms above 15 employees over all hires in each market-year tuple[4]. To rule out the fear of manipulation on the threshold, we measure it in the pre-policy period. Hence, the higher T_m the lower the share of small firms.

We estimate a difference-in-difference (DD) equation at the market level to single out the impact of HS (through T_m) on HHI:

$$HHI_{m,y} = \Theta_{o,y} + \delta_{s,y} + \Delta_{r,y} + \sigma Z_{m,y} + \sum_{y \neq 2014} \gamma_y \cdot T_m \cdot 1[y] + \varepsilon_{m,y} \tag{2}$$

where $1[y]$ is a dummy equal to 1 in year y, 0 otherwise; the baseline year is 2014. $Z_{m,y}$ contains market-specific variables: $HHI_{m,y<2015} \cdot 1[y]$ *i.e.*, pre-reform HHI mean interacted with each year, to estimate γ_y as deviations from the pre-reform HHI average; market-year average share of women and average workers' age, as they evolve and influence systematically the hiring patterns.

We further split the sample according to markets where T_m is above vs below its median value of 0.5 exactly[5], to single out relevant non-linearities in the effects, as we discuss below.

Figure 2 plots γ_y of equation 2, separately for T_m above and below its median. While γ_y is never significant when estimated with the whole sample, it is in markets with T_m below the median and equal to 6 p.p., which translates into -46% in HHI. Intuition is as follows. In markets where large firms are predominant and drive the hiring process, HS are not effective in modifying concentration of hiring, as HS are predominantly exploited by small firms and small firms are not so present in these markets.

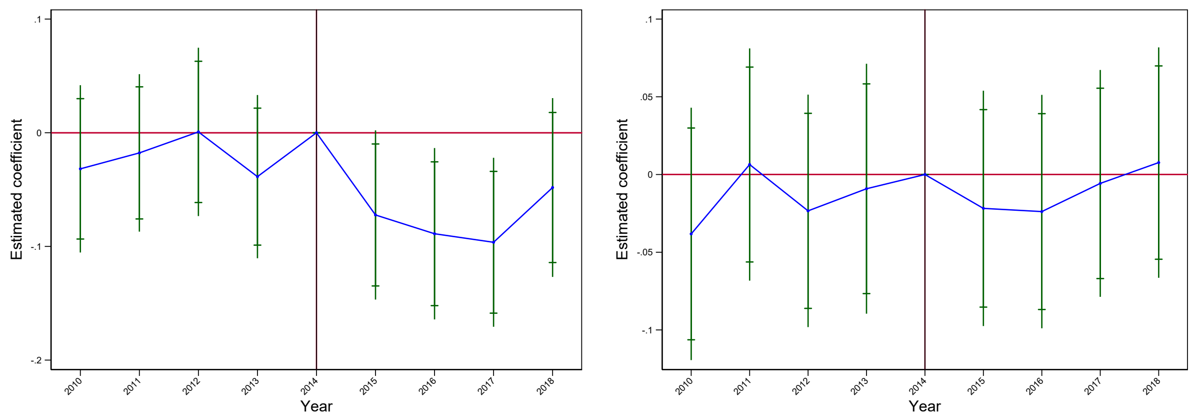
On the contrary, in markets where small and large firms compete to hire workers, HS help smaller firms gain hiring market shares and hence decrease concentration. In this more balanced context, the more the market is balanced (*i.e.*, the higher T_m, although below its median value) the larger the reduction in concentration ($\widehat{\gamma}_y$ is negative), while when small firms are predominant in the hiring market ($T_m \approx 0$) concentration is less affected. Notice that the significant effect on HHI lasts beyond 2016. We might suppose that firms employ the 2 or 3-year savings generated by HS to support unsubsidized hirings in 2017 as well.[6].

[4]We dropped the values equal to 0 and 1, i.e. those markets in which only small or large firms compete.

[5]That is the median value of T_m before 2015.

[6]Table A.1 reports full-blown results.

Figure 2: Event study of HI effect on concentration, by treatment distribution.



(a) Below median (b) Above median

Notes: The figure plots γ_y of Equation 2. The panels refer to different samples: in (a) and (b), respectively, below and above the threshold of 0.5 in the treatment. 95 and 90% CI.

A possible concern regarding identification is that we might capture also the FC reduction effect. Therefore, we compute a different treatment. We use only open-ended hires of firms to which the FC reduction applies; for each firm, we compute full-time equivalent average daily wages and their share of social security contributions covered by the 2015 HS of 8,060 euros. Then we compute the mean share in each market in the pre-reform period. This is now the continuous treatment whose estimates are shown in Figure A.2. Reassuringly, results are consistent with those of the main model, since there is a negative effect on HHI increasing in HS coverage below the median and null above.

3 Wages

We now estimate a wage equation at the worker level. It is aimed at capturing two possible effects: (1) a direct rent sharing effect of HS on entrants' wages, as HS increase the rent to be shared; (2) an indirect effect through the decreased HHI.

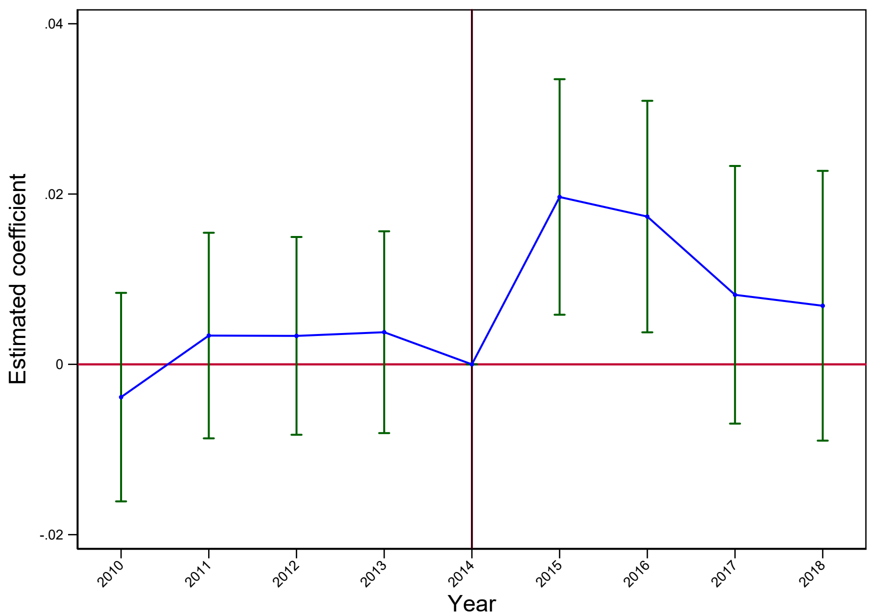
Treatment DT_m becomes a dummy at the market level obtained by dichotomizing the continuous treatment T_m of equation 2 at the median (=1 below the median). In summary, we compare wages between markets in which HS were massively exploited ($DT_m = 1$) and markets in which they were not, based on Figure 2, as discussed above. We estimate this equation at the worker level:

$$Y_{i,j,y} = \alpha_i + \Theta_{o,y} + \delta_{s,y} + \Delta_{r,y} + \sigma X_{m,y} + \sum_{y \neq 2014} \gamma_y \cdot DT_m \cdot 1[y] + \xi_{i,j,y} \tag{3}$$

where Y is the log daily wage of entrant i, in firm j, and year y. $1[y]$ is a dummy equal to 1 in year y, 0 otherwise; the baseline year is 2014. As in Equation 2, $X_{m,y}$ includes the market-specific pre-reform HHI means; it also includes the pre-reform mode of firm j size and a cubic polynomial in age [7]. We do not control for FC reduction because Leonardi and Pica 2013 prove that EPL variations do not affect wages directly.

The estimates are shown in Figure 3 and indicate that there is an increase in wages in markets where HS has been effective in reducing HHI, compared to markets where it has not. The dynamics estimates are significant only when HS exist at the date of hiring. However, HS are received until 2018 for workers hired in 2015 or 2016, generating a rent that could be shared up to that year. Therefore, we interpret this as a sign of a direct effect of HS rather than a change in concentration, which we disentangle by looking at the timing of the dynamic effects.

Figure 3: Event study of HI effect on wages.



Notes: The dependent variable is the log of entrants' daily wage. The figure plots the semi-elasticities γ_y's of Equation 3. 2014 is the baseline. 95% CI.

4 Conclusion

We find that HS lowered concentration in markets where small and large firms compete to hire, thus reducing the degree of monopsonistic power. In the same markets, we estimate an increase in wages, but it shows a different timing than the decrease in HHI, so being more likely compatible with a rent sharing explanation

[7]Details in Table A.3.

than with a decrease in concentration. However, we find that workers obtain through their wages at most 4% of the entire subsidy amount in 2015[8]. We might suppose that firms employ the 2- or 3-year savings generated by HS to support unsubsidized hirings in 2017. Contributions are (i) implementing a novel and exogenous identification strategy, identifying an additional driver of employers' power, and (ii) highlighting that also small firms have large wage-setting power.

References

Leonardi, Marco and Giovanni Pica (2013). "Who Pays for it? The Heterogeneous Wage Effects of Employment Protection Legislation". *The Economic Journal* 123.573, pp. 1236–1278.

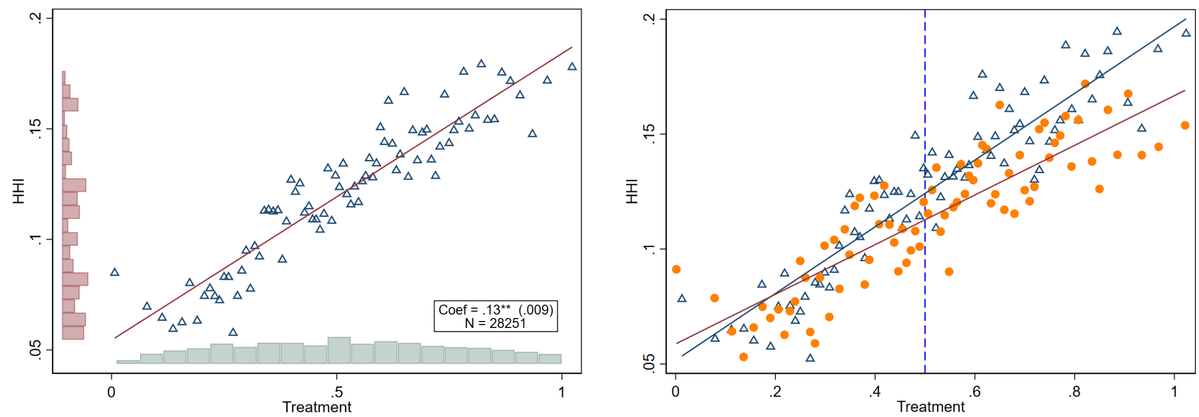
Marinescu, Ioana, Ivan Ouss, and Louis-Daniel Pape (2021). "Wages, hires, and labor market concentration". *Journal of Economic Behavior & Organization* 184, pp. 506–605. ISSN: 0167-2681.

[8] Details in Table A.4-footnote.

A Appendix

A.1 Figures

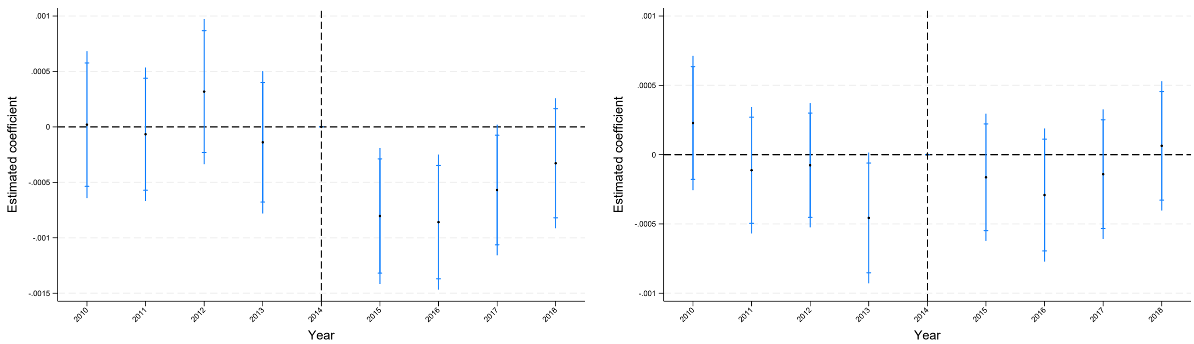
Figure A.1: Binscatters between HHI and the treatment.



(a) Whole sample (b) 2010-14 (triangle) and 2015-18 (dot)

Notes: In Panel (a), we fit the data with a linear polynomial, while in Panel (b) we use a quadratic polynomial. We let the number of bins be decided by the algorithm based on variance optimization (more details here). We collapse the market-year dataset into a market-level one, in which we compute the average concentration levels pre- and post-reform. Concentration is computed in Equation 1. Treatment is the pre-reform market-specific mean of the share of all hires of firms above 15 employees over the total number of hires in each market-year tuple. We control for the average age and women's share in each market in the pre-period. An observation is a tuple market-period.

Figure A.2: Event study of HI effect on concentration, by treatment distribution, with the alternative treatment.



(a) Below median (b) Above median

Notes: The figure plots γ_y of Equation 2 where the treatment is the market-specific share of full-time equivalent gross annual income coverage by the HS, computed in the pre-reform period (2010-2014). The panels refer to different samples: in (a) and (b), respectively, below and above the threshold of 0.5 in the treatment. 95 and 90% CI.

A.2 Tables

Table A.1: DD estimates of HI effect on concentration.

Dependent variable: HHI	(1) Overall sample	(2) Below median	(3) Above median
Treat x Post	-0.0217**	-0.0576***	0.00264
	(0.0102)	(0.0196)	(0.0185)
Observations	28,135	13,009	15,085
R-squared	0.738	0.726	0.749
Occupation-year FE	✓	✓	✓
Industry-year FE	✓	✓	✓
Region-year FE	✓	✓	✓
Market-level controls	✓	✓	✓
HHI pre-2015-year FE	✓	✓	✓
Pre-2015 HHI mean	0.125	0.0885	0.155

Notes: *** p<0.01, ** p<0.05, * p<0.1. The coefficient is an ITT. Pre-period is 2010-14, post-period 2015-18. The table displays the aggregated γ_y from Equation 2, that is, the coefficient associated with the interaction between T_m and the post-period dummy. Standard errors clustered at the market level in parentheses. Obs. are 28,135 market-year tuples associated with 4,202 markets. The dependent variable is in level. The columns refer to different samples: in (1) all markets, while in (2) and (3) respectively, only those markets below and above the threshold of .5 in the treatment. Treatment is computed as the share of all hires in firms above the threshold of 15 employees over the total number of hires in each market year cel.

Table A.2: Event study estimates for HHI above and below the treatment median, by year.

Dependent variable: HHI	(1) Below median	(2) Above median
2010	-0.0317	-0.0382
	(0.0375)	(0.0414)
2011	-0.0177	0.00643
	(0.0353)	(0.0381)
2012	0.000754	-0.0234
	(0.0377)	(0.0381)
2013	-0.0386	-0.00914
	(0.0366)	(0.0410)
2015	-0.0723*	-0.0218
	(0.0379)	(0.0386)
2016	-0.0888**	-0.0238
	(0.0384)	(0.0383)
2017	-0.0963**	-0.00571
	(0.0379)	(0.0372)
2018	-0.0482	0.00768
	(0.0401)	(0.0378)
Observations	13,009	15,085
R-squared	0.726	0.749
Occupation-year FE	✓	✓
Industry-year FE	✓	✓
Region-year FE	✓	✓
Market-level controls	✓	✓
HHI pre-2015-year FE	✓	✓
Pre-2015 HHI mean	0.0885	0.155

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each coefficient is an ITT. The baseline in 2014. The table displays the γ_y from Equation 2, that is, the coefficient associated with the interaction between T_m and the yearly dummies. Standard errors clustered at the market level in parentheses. The dependent variable is in level. The columns refer to different samples: in (1) those markets below the threshold of 0.5 in the treatment, in (2) above and equal. Treatment is computed as the share of all hires in firms above the threshold of 15 employees over the total number of hires in each market year cel.

Table A.3: DD estimates on entrants' wages.

Daily wages	(1) log	(2) log	(3) log	(4) log	(5) log	(6) level	(7) level	(8) level	(9) level	(10) level
Treat x Post	0.00283	-0.00380	0.0122***	0.0129***	0.0123***	-0.653	-0.600***	0.464*	0.580**	0.501**
	(0.00729)	(0.00364)	(0.00407)	(0.00369)	(0.00371)	(0.418)	(0.213)	(0.276)	(0.246)	(0.247)
Observations	1,685,145	1,684,985	1,684,985	1,160,980	1,160,794	1,706,046	1,705,889	1,705,889	1,180,619	1,180,425
R-squared	0.320	0.374	0.378	0.737	0.737	0.374	0.413	0.417	0.745	0.745
Age cubic	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region FE	✓	✓				✓	✓			
Occupation FE	✓	✓				✓	✓			
Industry FE	✓	✓				✓	✓			
Size FE	✓	✓				✓	✓			
Market FE		✓	✓	✓	-		✓	✓	✓	-
Region-year FE			✓	✓	✓			✓	✓	✓
Occupation-year FE			✓	✓	✓			✓	✓	✓
Industry-year FE			✓	✓	✓			✓	✓	✓
Size-year FE			✓	✓	✓			✓	✓	✓
Worker FE				✓	✓				✓	✓
HHI pre-year FE					✓					✓
Pre-2015 daily wages mean	62					62				
Pre-2015 daily wages median	58					58				

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is entrants' daily wage. Observations are worker-year tuples, as we kept for each worker the longest job spell per year. The table displays the aggregated γ_y from Equation 3, namely the coefficients of the interaction between T_m and the post-period dummy. Standard errors clustered at the market-year level in parentheses. Columns refer to different sets of controls, from left to right, increasing. In columns (1)-(5) the dependent variable is the log of daily wages, while in columns (6)-(10) is in level. If below 45-year-old, age is replaced with 0 to deal with its multi-collinearity with worker and time fixed effects (Chan, 2018). Columns have different numbers of observations because, when adding iteratively fixed effects, some observations are dropped. Records with wages equal to 0 are dropped. Treatment is defined as the share of all hires of firms above 15 employees over the total number of hires in each market-year tuple.

Table A.4: Event study estimates for wages above and below the treatment median, by year.

	(1) Log(Daily wages)
2010	-0.00385
	(0.00625)
2011	0.00338
	(0.00616)
2012	0.00334
	(0.00593)
2013	0.00377
	(0.00605)
2015	0.0197***
	(0.00706)
2016	0.0174**
	(0.00694)
2017	0.00817
	(0.00772)
2018	0.00688
	(0.00808)
Observations	1,160,794
R-squared	0.737
Year FE	✓
Age cubic	✓
Region-year FE	✓
Occupation-year FE	✓
Industry-year FE	✓
Size-year FE	✓
Worker FE	✓
HHI pre-2015-year FE	✓
Pre-2015 daily wages mean	62
Pre-2015 daily wages median	58

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the log of entrants' daily wage. Observations are worker-year tuples, as we kept for each worker the longest job spell per year. The table displays the γ_y from Equation 3, namely the coefficient of the interaction between T_m and the yearly dummies. The baseline is 2014. Standard errors clustered at the market-year level in parentheses. If below 45-year-old, age is replaced with 0 to deal with its multi-collinearity with worker and time fixed effects (Chan, 2018). The estimates in 2015 and 2016 translate into an increase of approximately 1.24 and 1.16 euros at the mean and median, respectively. Wages equal to 0 are dropped. Treatment is defined as the share of all hires of firms above 15 employees over the total number of hires in each market-year tuple. 2 p.p. increase in 2015 translates into an increase for a daily wage at the mean (62 euros) of 1.24 euros. This means that for full-time workers, wages increased by approximately 317 euros in 2015, which corresponds to approximately 4% of the subsidy amount in 2015 (8,060 euros).

A.3 Further references

Chan, David C. "The efficiency of slacking off: Evidence from the emergency department." Econometrica 86.3 (2018): 997-1030.