The perverse effects of hiring credits as a place-based policy: Evidence from Southern Italy

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Abstract

This paper evaluates the wage effects of a tax credit policy on new hirings in Southern Italy. We use high-quality administrative data and propose a latent class inverse probability weighting method as a strategy to account for workers' unobserved heterogeneity. We find an unexpected negative effect of the tax cut on the wages of treated workers, which is more marked for women. Our results also provide new insights on the job-segregation dimension of the gender gap. We provide a theoretical model with worker and firm fixed effects to analyse the impact of employer tax cuts as a place-based policy in lagging regions.

Keywords: Regional disparities, place-based policies, hiring credits, wage differentials, gender segregation
JEL Classification: J16, J31, J38, J42, R58

1 Introduction

Hiring credits to favour job creation have been increasingly popular in the US and Europe to counteract the high unemployment induced by the Great Recession. Expansionary tax credits also came into use under the framework of the EU cohesion policy to outweigh the competitive disadvantages in structurally lagging regions. Empirical evidence of the effect of hiring credits shows little employment impacts and no significant effect on the wages of subsidised jobs (see Egebark and Kaunitz 2018; Saez et al. 2019, among others). Although this questions the effectiveness of such a costly fiscal stimulus, hiring credits still belong to the standard toolkit of place-based policies (Kline and Moretti, 2014).

In this paper, we analyse the distributive and side effects of hiring credits in lagging regions. The rationale is the downplayed though fundamental aspect of such kinds of policies: they are basically targeted at a segment of the labour market that can be defined as *marginal.* Tax credits are ultimately aimed at allowing job matches that are not profitable at market conditions. Nevertheless, labour costs can obviously be one (though not the only) determinant of the profitability of a job match. Furthermore, whenever relevant obstacles to regional development rely on structural factors other than gross wages, including rationing in final or credit markets or the lack of infrastructure and good institutions, tax credits become less effective. In this case, the firms' marginal productivity curve is steep, and the transfers involve the activation of hardly productive matches. If both the fall in the marginal productivity of the subsidised matches offsets the value of the tax credit and the labour supply curve has a negative slope for this marginal segment, then we would expect a negative effect of the policy on the wage of the subsidised hirings. Although this result is in contrast with the standard labour market model, it is consistent with the hypothesis that the activated matches can actually involve only low-wage and low-productivity jobs.

Focusing on this 'bad jobs effect', we estimate the impact of hiring credits on wages in the lagging regions of Southern Italy and show as core evidence of our case study that the tax credit negatively affected the wage of the workers hired with the policy scheme. Indeed, the two features, very low productivity and low outside option of workers, characterise the marginal segment of the labour market.

The sign of the effect on wages seems also at odds with the rent-sharing model of wage setting[1], according to which wages should catch part of the subsidy in the bargaining process. We provide a discussion arguing that our evidence is consistent with a model of monopsony, rent sharing and sorting in the labour market (Card et al., 2018) and reconsider it to interpret the other main result of the empirical analysis: the substantial gender dimension of the wage effect. This result is determined by the high concentration of bad jobs among women and can be explained by job segregation, which is indeed compatible with the sorting channel in the labour market model proposed by Card et al. (2016). Applied to the case of marginal jobs, the sorting channel working through search costs and women's preferences on non-pecuniary aspects of jobs explains the gender-biased impact of hiring credits and predicts that hiring credits may involve a further segregation of woman in bad jobs. This offers us a novel perspective from which to interpret the overall effect of hiring credits in lagging regions with a gender-segmented labour market.

Southern Italy is an interesting case for the empirical analysis because, in light of the history of the Italian persistent economic dualism, hiring subsidies reducing firms' labour costs to favour job creation have been a key component of place-based policies for a long time (de Castris and Pellegrini, 2012). In addition, a new interest for hiring credits after the Great Recession was justified by the concern that the persistent economic crisis might exacerbate the already high-income dispersion among regional partitions (Ciani and Torrini, 2019; Ciani et al., 2019; Mussida and Parisi, 2020).

Despite the wide use of hiring credits to spur job creation in Southern Italy, no evidence at all has been provided regarding the specific effects in the targeted regions. This study fills this gap, estimating the effect of a large and long-lasting programme of hiring credits on the wage of workers hired with the tax cut provided over the period 2008-2014. Since gender is one of the most important sources of labour market segmentation in Italy, particularly in the South (Deidda et al., 2015; Del Bono and Vuri, 2011), we investigate this issue from a perspective that emphasises labour market segmentation between the genders, a dimension that has been neglected in the limited evidence on the Italian case provided so far[2]. From a practical standpoint, this analysis helps unveil the coherence of tax credits with the goals of the place-based policy implemented in Southern Italy and, more generally, questions the effectiveness of such programmes of cuts to the employer portion of payroll taxes, which currently represent the most common type of active labour market policies (ALMPs) in Italy and are reauthorised on a regular basis at the national and regional level.

[1]Saez et al. (2019) show an implicit 'insider effect' of rent sharing in the case of hiring credits implemented in Sweden, implying that the tax credit positively affected the wages of other workers in the treated firms. This result makes the rent-sharing hypothesis compatible with a non-positive effect of the policy on the wages of subsidised workers.

[2]See Cipollone and Guelfi 2006; Pasquini et al. 2018; Sestito and Viviano 2016 for some recent evidence on the impact of hiring incentives on employment at the national level.

Another contribution of the paper is motivated by the lack of some relevant information on workers' characteristics in our administrative dataset. In this regard, we propose a latent class inverse probability weighting (LC-IPW) method as a strategy to identify the impact of the tax cut on wages while providing a refinement in controls for unobserved labour market qualifications, such as the level of education of the workforce and family background. In this way, we obtain results that are controlled for gender composition effects related to unobserved characteristics.

Finally, we provide a generalisation of our findings to coherently locate the analysis in the existing theoretical literature. By proposing a model with rent-sharing and sorting mechanisms, we show that hiring credits may produce wage differentials and a gender wage gap in a labour market characterised by monopsonistic competition and firm and worker heterogeneity.

Our study is related to the literature investigating the counter-recessionary effects of hiring credits in the US and European countries[3]. Cahuc et al. (2019) show that the effectiveness of hiring credits in France is contingent on particular circumstances: temporary hiring credits have little congestion effects on wages and thus induce significant effects on employment, whereas permanent economy-wide hiring credits induce permanent increases in labour market tightness, and thus have a stronger impact on the expected gains of unemployed workers and, then, on wages. This analysis casts doubt about the effectiveness of permanent hiring credits on job creation, as documented in a number of available studies (Dickert-Conlin and Holtz-Eakin, 1999; Neumark, 2016; Neumark and Grijalva, 2017; OECD et al., 2010). However, Saez et al. (2019) analyse a large and permanent payroll tax cut applied to both new and ongoing jobs for young workers in Sweden and show that this policy generates firm-specific profit windfalls and rent-sharing responses within firms with a high incidence of treated workers. They show that the policy, when targeted to groups with weak attachment to the labour market (such as the young, workers in depressed geographical areas, or lower-paid workers), can be a useful tool to fight inefficiently high unemployment and to sustain wages of the targeted workers. Similar results are provided by Egebark and Kaunitz (2018), but they conclude that the Swedish payroll tax reduction was not a cost-effective way for increasing employment for young individuals. Overall, these findings show that the effectiveness of hiring credits with respect to a wide range of outcomes is strongly context-specific and contingent on the design of the measure.

Furthermore, this research draws on the literature on rent sharing and sorting that explains potential heterogeneous effects of the same institution or policy in different labour market segments (Card et al., 2013, 2016, 2018). These studies show that trends in wage dispersion closely track trends in the dispersion of productivity across workplaces and differential sorting of higher-ability workers to more productive firms. In addition, workers have heterogeneous preferences over potential employers depending on workplace differentiation in firm location and job characteristics, such as starting times for work. Such heterogeneity makes employers an imperfect substitute for workers, which gives firms some wage-setting power along the line of the Robinson's monopsony model (Robinson, 1933).

Furthermore, since workers' allocation across jobs and establishments might imply gender-based differences in a number of factors (e.g., the elasticity of labour supply, search costs, outside options) that produce incentives for monopsonistic discrimination, we refer to the literature on gender discrimination and job segregation (Barth and Dale-Olsen, 2009; Flabbi, 2010; Petrongolo, 2004).

The article is organised as follows. Section 2 describes the institutional background of the hiring credit schemes implemented in Southern Italy since 2008 and highlights the relevance of the discussion for the current political debate on hiring subsidies in Italy. Section 3 presents the data. Section 4 discusses the empirical strategy. The results of the estimation are presented in

[3]See Neumark (2016) for a review of the literature with a focus on US evidence. See also Brown (2015) for a review on pros and cons of hiring subsidies, in general.

Section 5, whereas Section 6 provides the theoretical model to interpret our findings. Section 7 concludes.

2 Background

Hiring credits should spur labour demand by lowering labour costs for eligible employers. This policy is based on tax credits targeting specific groups - often the disadvantaged - or counter-cyclical tax credits targeting the unemployed.

In Italy, there is a long lineage of hiring subsidies that provide financial incentives to private employers by temporarily reducing their labour costs (salary, tax or social contribution costs). According to Law 407/1990, any firm had access to a benefit corresponding to 50 percent of the taxes the firm had to pay to the social security service (INPS) and insurance agency (INAIL) for each individual, for a period of 36 months, on the condition of hiring the long-term unemployed and providing them a permanent contract [4]. The law ended on December 31st 2014 according to the 2015 Finance Law, as we will explain next. Considering the last twenty years, it is worth recalling that the 2001 Finance Law introduced a new hiring incentive in the form of a general, automatic and quite generous tax credit to all firms hiring workers with open-ended contracts, conditional on the firm's net job creation (Cipollone and Guelfi, 2006). This measure was intended to be temporary, but it was renewed and lasted up to 2006.

Even more relevant has been the implementation of geographically targeted hiring incentives that has been justified by large and long-standing regional inequalities (Colombo and Regini, 2016) [5]. For example, the incentive for hiring the long-term unemployed provided by Law 407/1990 was increased to 100 percent of the amount of social security contributions for artisans and other firms located in Southern Italy (Pasquini et al., 2018).

In the post-crisis period, the Southern Italian regions faced a severe economic downturn and an enormous job creation challenge. The downturn has had adverse impacts on almost every group of workers and all regions of the country in terms of both substantial unemployment and stagnant wages. However, job losses in the wake of the Great Recession have disproportionately affected workers in the South, where the unemployment rate had historically tended to be higher. In parallel, earnings inequality in the South has widened sharply over the last decade.

The continued fragility of the economy, the possibility of a sustained jobless recovery (Cellini and Torrisi, 2014), and the increasing inequality within the South macro-area (Mussida and Parisi, 2020) represented calls to action for immediate policy steps to expand employment and incentivise job creation. Hence, the 2008 Finance Law enacted by the Prodi Government introduced a job creation package for the Southern regions (Law 244/2007) that included tax relief measures for investments and new jobs, and other measures to deal with issues ranging from competitiveness and development to business finance and investment in scientific and technological research. The main components of the package were the enhanced short-run tax incentives for private sector employment expansion (Newell, 2009). The design of the measure was based on an automatic tax credit for new permanent jobs in the Mezzogiorno to reward net positive changes in employment that would not have occurred otherwise, and the credit was categorically targeted to the unemployed and people who had never worked[6].

[4]Long-term unemployed are individuals who had been either in unemployment status, or in temporary layoff with the wage supplementation scheme (*the so-called Cassa integrazione guadagni, CIG*), for at least 24 months.

[5]Italy includes 20 administrative regions: Piedmont,Valle d 'Aosta, Lombardy, Liguria, Trentino–Alto Adige, Veneto, Friuli–Venezia Giulia, Emilia–Romagna (North), Tuscany, Marche, Umbria, Lazio (Centre), Abruzzo, Molise, Puglia, Campania, Basilicata, Calabria, Sicily and Sardinia (South).

[6]For the subgroup of women, the eligibility was extended to disadvantaged workers according to the European Commission's definition, namely, workers with difficulties entering the labour market, including disabled workers, young workers, migrants in the labour market and the long-term unemployed. The tax credit was fixed to 330

The hiring measure was announced to last for three years (from 2008 to 2010). However, the extremely high unemployment rate and continued rise in long-term unemployment made a strong humanitarian and economic case for a longer-than-normal duration of benefits through the continuation of the tax credit compensation. Hence, in 2011, the Monti Government issued the so-called *Bonus Sud* (Law 106/2011) that confirmed the measure with the explicit aim of increasing the productivity of the depressed areas. More in detail, the *Bonus Sud* provided a generous tax credit (50 percent of the wage cost, including gross earnings and social security contributions) for new employment on a permanent basis for the disadvantaged workers in Southern Italy[7]. Again, the announced duration of the hiring credit was up to 2013, but the measure was extended up to 2015 by the Letta Government (Law 76/2013, known as *Labour Decree*).

Since the job losses over the 2008-2014 period were concentrated among young people and people holding temporary contracts, the political debate on employment creation policies at the time was focused on the objective of reducing labour market dualism. Thus, two major labour market reforms (the *Fornero reform* introduced with Law 92/2012, and the *Jobs Act*, enacted by Law 183/2014) were introduced with the aim of reducing the firing costs for fair dismissal in firms with more than 15 employees. To offset the increasing flexibility in dismissal regulation, hiring subsidies became a constant feature of the national policy to spur employment. The 2015 Finance Law issued by the Renzi Government introduced non-conditional hiring incentives for the first time in Italy (OECD, 2017; Centra and Gualtieri, 2017; Pasquini et al., 2018; Sestito and Viviano, 2016), providing a three-year exemption from social security contributions for new hires in 2015 with the new contract on a permanent basis (the so-called *Contratto a tutele crescenti*) and for conversions from temporary contracts to open-ended positions.

In 2016, this measure was confirmed (with a relief reduced to 40 percent of social security contributions) and strengthened for the Southern regions, extending the eligibility to additional employment contracts. Indeed, the so-called *Incentivo Occupazione Sud* provided a 100 percent cut in the non-wage labour costs for hirings in the Southern regions of young people (with less than 24 years) on a permanent basis or hirings with the apprenticeship contract.

Similarly, the 2018 Finance Law issued by the Gentiloni Government introduced a structural incentive for the employment of young people (with less than 35 years), providing a relief of 50 percent of the non-wage labour cost for new hirings or conversion of fixed-term contracts into permanent contracts. This approach has also been followed by the next governments, which extended up to 2020 the generous reliefs in the non-wage labour cost for the employment of young people on a permanent basis, at both the national and regional level, with specific measures for the less-developed regions.

Given the relevance of hiring credits in the current policy for job creation in the Southern regions, and, more generally, in Italy, it is relevant to run an evaluation of their impact in the distressed areas to provide some policy implications and suggestions for the near future.

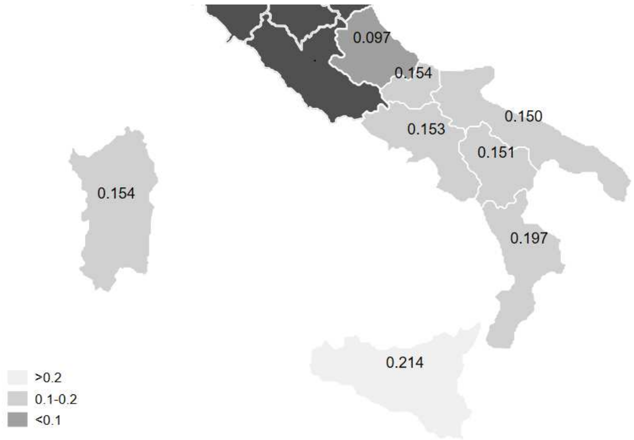
Our analysis regards the measures implemented in Southern Italy by Law 244/2007 (over the 2008-2010 time span) and Law 106/2011 (that lasted from 2011 up to 2015). To assure an accurate identification of the tax credit policy, we have to take in account the overlap of multiple policies, and thus our empirical analysis excludes year 2015, during which new tax cuts at the national level were introduced. Figure 1 displays the share of the incentivised hires in the total new hires of dependent workers on a permanent basis in the private sector, by region, in the two subperiods covered by our analysis. We observe that the incidence of the subsidised hires is large, ranging from almost 9% in Abruzzo to 21% in Sicily as an average over the two

euros per month for each new hire; this amount was increased to 416 euros in the case of women who were disadvantaged workers.

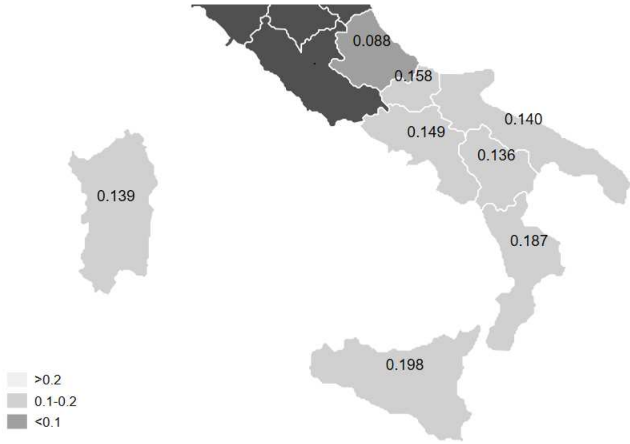
[7]The measure was also targeted to the very disadvantaged workers, i.e., the individuals who are unemployed for at least 24 months.

subperiods.

Figure 1: Share of workers hired with the tax credit on total new hires, by region



(a) 2008-2010



(b) 2011-2014

Source: Authors' calculations on LoSaI dataset.
Note: The figure reports the distribution of workers employed with the hiring credit in Southern Italy as a share of total new hires of dependent workers on a permanent basis in the private sector. Panel a) reports the shares of the workers hired with the incentives provided by Law 244/2007 over the period 2008-2010; panel b) reports the shares of the workers hired with the incentives provided by Law 106/2011 over the period 2011-2014.

3 Data

We exploit high-quality micro-level data with a longitudinal structure drawn from the administrative records maintained by the Italian Social Security Institute (Longitudinal Sample INPS – LoSaI). The dataset covers 1/15 of the population working in the private sector from 1985 up to 2015 and provides information on the individual yearly employment history, along with information on the job attributes (annual earnings, weekly wages and annual weeks worked) and basic demographic information on workers' characteristics (such as gender, year of birth, place of residence and position) and some information on the employers' characteristics (such as firm size and industry).

To select the sample, we excluded self-employed and semi-subordinate workers since the dataset reports only partial information for these workers. We also excluded those workers who were already employed with a permanent contract and remained in the contract for the whole period of the analysis, and then, we collected information on the history of each individual who was newly hired on a permanent basis in the period starting from 2008 up to 2014. When the individual had contemporary jobs in different firms during the same year, we used the information on the weeks of the social security contribution and wages to isolate the main activity from a secondary job.

Then, from the dataset, we extracted relevant information on the ALMPs implemented during the analysed time span. LoSaI distinguishes among several active labour market interventions[8]. We identified the workers with a permanent position acquired through the hiring

[8]LoSaI archives classify ALMPs in five categories: i) human capital-enhancing policies for the youth working age population (i.e., apprenticeship and training contracts), ii) other measures to facilitate the occupations of first-job seekers (i.e., access-to-work contracts), iii) hiring credits for permanent contracts, iv) incentives to firms to hire workers with permanent or temporary contracts, and v) policies to increase the matching between workers and firms (i.e., temporary agency works and on-call contracts).

credit policy in the considered time span and dropped the employees who benefited from a different active policy or a mix of them. Note also that our dataset does not distinguish between the population of workers employed with the hiring credit policy and the population of long-term unemployed that were hired with the tax relief provided by Law 407/1990. Hence, we restricted the sample, excluding all the individuals who were unemployed for at least 24 months in order to remove the overlapping in the targeted population of the two measures [9]. Then, we constructed a dummy variable (T_policy), recorded as 1 when the new hiring on a permanent basis is attributable to the tax credit policy. We ended up with a sample size of 164,785 observations, including 14,137 treated and 28,242 untreated individuals.

We consider the log of daily wages [10] as a dependent variable in our analysis. Wages are expressed in real terms (i.e., deflated with the consumer price index 2010) and represent a proxy for the price of labour[11], reflecting a worker's productivity or the rent sharing due to bargaining effects at the firm level.

We also rely on the log of annual weeks worked and the log of annual earnings as dependent variables to provide complementary evidence supporting our main analysis. These variables summarise information on different job attributes. 'Annual weeks worked' is an indicator of the intensive margin effect of the policy and provides information on workers' employability and experience acquired on-the-job. 'Annual earnings' is a variable that provides a better idea of how much workers 'take home'.

Regarding other characteristics of the workers, we collected demographic information on gender, age and cohort of entrance in the labour market, and we accounted for the type of contract (full and part time) and position (executive, white collar and blue collar) held at the firm.

We also collected information on the previous labour market status of the worker before the subsidised hire. We distinguished when the individual was predominantly unemployed or employed with a fixed-term contract, calculated on the basis of the number of years spent in each status[12]. We added a measure of the work experience of the individuals, obtained as the cumulative distribution of the worked weeks since the first entrance in the labour market. To account for unemployment spells, we included the average days compensated under the ordinary wage supplementation scheme (Cassa Integrazione Guadagni, CIG) and the average days spent in unemployment receiving social benefits. We introduced a dummy variable to account for the previous experience of the employee within a firm, which indicates whether the employer had the opportunity to screen the worker before hiring her/him on a permanent basis. The variable is recorded as 1 when the individual is hired with the new permanent contract after experiencing a fixed-term contract in the same firm in the previous year[13]. In this way, we isolate the cases in which hiring credits actually are used to retain workers that have already signalled their productivity and work-specific human capital that is valuable for the hiring firm.

We then calculated the incidence of atypical jobs (part-time work and fixed-term contracts) among workers at the firm level, considering the whole LoSaI-dataset population. This variable, that we name 'bad job' (Cardoso et al., 2016), proxies the extent to which in each firm, workers

[9] We acknowledge another source of possible bias in our estimates because temporary hiring subsidy measures might have been implemented at local level by regional governments over the period of the analysis. We cannot control for this contemporaneous effect, but we assume that it homogeneously affects our estimates because of the strong similarity of the regions in Southern Italy along both the economic and institutional dimensions.

[10] Daily wages are calculated using the taxable amount of weekly wages, i.e., gross weekly wages net of social and insurance contributions; correspondingly, annual earnings are based on taxable wages.

[11] Hourly wages are not available in the LoSaI dataset.

[12] To identify the unemployment spells, we recorded the worker status as 'unemployed' when, in a selected year, the individual received less than 12 weeks of social security contributions.

[13] In explorative estimations, we used this variable constructed also on the previous two or more years. Because these alternative definitions yielded similar estimation results, the final analysis is based on this version only.

Table 1: Descriptive statistics

	Male sample		Female sample	
	Untreated workers	Treated workers	Untreated workers	Treated workers
Outcome variables				
Wages (Log)	4.21	4.04	3.96	3.65
Weeks worked (Log)	46.01	44.12	46.78	43.86
Earnings (Log)	9.70	9.45	9.47	9.06
Covariates				
Age Class				
15-20	0.69	0.71	0.49	0.39
20-25	5.09	11.36	5.35	10.49
25-30	10.79	16.56	11.89	18.20
30-35	16.75	19.16	18.29	19.68
35-40	17.57	17.81	18.64	17.91
40-45	14.40	12.51	15.49	14.39
45-50	12.15	9.11	12.41	10.10
50-55	11.07	6.50	10.14	5.53
55-60	8.49	4.15	6.13	2.60
60-64	3.01	2.14	1.19	0.72
Cohort of entrance				
85-89	24.40	19.76	12.35	9.47
90-94	9.49	9.95	8.53	6.52
95-99	10.89	11.48	10.90	8.95
00-04	21.35	23.94	28.89	25.39
05-09	28.44	28.29	32.97	39.13
10-14	5.44	6.58	6.37	10.53
Type of contract				
Full time	83.80	72.76	57.44	36.81
Part time	16.20	27.24	42.56	63.19
Position				
Executive	2.74	0.48	2.10	0.28
White Collar	62.87	76.37	38.07	54.70
Blue Collar	34.19	23.03	59.44	44.93
Others	0.20	0.12	0.39	0.09
Previous status				
Unemployment	58.00	67.79	55.43	68.82
Fixed-term job	42.00	32.21	44.57	31.18
Experience	6.02	4.97	5.75	4.34
Ordinary redundancy fund (days)	10.59	8.86	5.47	6.46
Paid Unemployment (days)	10.64	18.26	7.85	14.85
Previous screening in the firm				
no	67.28	82.79	61.63	76.12
yes	32.72	17.21	38.37	23.88
Bad job				
no	84.89	73.32	59.84	37.89
yes	15.11	26.68	40.16	62.11
Firm Size				
≤ 5	20.55	37.95	21.33	40.40
6-10	9.94	13.22	8.59	11.79
11-50	21.33	21.25	17.50	19.75
> 50	48.18	27.58	52.58	28.05
Firm structure				
Subsidiary	7.98	5.60	7.50	4.81
Parent company	31.86	15.37	33.17	16.18
Single firm	60.16	79.03	59.33	79.01
Economic sectors				
Agriculture and mining	3.77	1.76	0.67	0.35
Manufacturing	18.93	16.28	9.89	10.60
Private services	29.32	42.17	44.65	60.48
Government services	7.29	6.01	21.39	18.33
Construction	12.99	18.70	1.19	1.91
Utilities	25.35	11.28	18.20	3.09
Others	2.34	3.80	4.00	5.24

are over- or underrepresented in less secure jobs (fixed-term contract) or in jobs that may be considered as a form of underemployment (part-time work). The variable 'bad job' is then obtained setting a dummy that takes the value of 1 for observations above the median and 0 below the median.

Last, we extracted firm-level information from the dataset and selected four groups of employers, namely, micro-enterprises with ≤ 5 employees and micro-enterprises with $6-10$ employees, small enterprises ($11-50$ employees) and medium and large enterprises (> 50 employees)[14]. We also distinguished when the employer is a single firm, a parent company or a subsidiary of a corporate group. We completed the sample by introducing the economic sectors of the firms where workers are hired (agriculture and mining, manufacturing, private services, government services, construction, utilities, others), obtained by aggregating information defined according to the NACE sectoral codes.

The upper part of Table 1 reports the descriptive statistics for the chosen outcome variables, whereas the bottom part of the table reports the main characteristics of the sample, distinguishing between workers hired with the tax credit and without it, and male and female subsamples. The table reveals some important differences between the samples. Generally, the individuals hired with the tax credit are more likely to be employed in micro-enterprises (≤ 5 employees) and predominantly in single firms, and in the private services sector, compared with the workers hired without the tax cut. As expected, they also show a higher probability of transiting from the unemployment status and a lower probability to have worked for the same firm before the new hiring compared with their untreated counterparts. Some interesting differences also emerge comparing men and women. For example, the probability of working in a part-time job for women, relative to men, is significantly higher, and this gap increases when looking at the individuals hired with the tax credit compared to the untreated groups. Women hired with the tax credit are also overwhelmingly represented in firms with a high concentration of bad jobs and are overrepresented in the private services sector compared to men hired with the same policy.

4 Empirical model

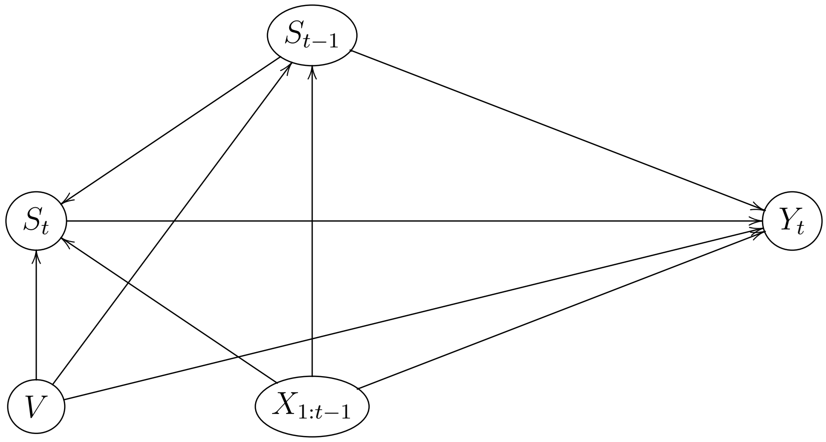
In the present analysis, both the timing and the length of the policy vary across the population and this produces, in turn, sequential treatment effects and dynamic causal effects. Let us consider a random sample where we observe an individual i for T consecutive labour market episodes after the introduction of the policy. We define t as a single episode where we observe the subject, such that $t = 1, ..., T$ and define the binary indicator of the treatment at episode t as S_t. We introduce two vectors of confounding factors, $\boldsymbol{V}$ and $\boldsymbol{X}_t$, to account for pretreatment covariates (i.e., measured before the first episode) and for time-varying covariates, respectively. Then, let us consider that the covariates simultaneously affect the outcome variable and the treatment indicator. To identify the effect of each treatment occasion (i.e., S_t) on the outcome, we have to account for the previous treatment occasion (i.e., S_{t-1}) along with the effect of the confounders included in $\boldsymbol{V}$ and $\boldsymbol{X}_{1:t-1}$[15]. This causal path is summarised by the direct analytic graph presented in Figure 2. Our goal is to estimate the causal link between S_t and Y_t by conditioning on a set of confounding factors that includeg the previous treatment occasion S_{t-1} and the vectors $\boldsymbol{V}$ and $\boldsymbol{X}_{1:t-1}$. Since all these confounding factors are non-colliders, to estimate the path between S_t and Y_t and interpret it as causal, we need to control for all these

[14] We could not use the precise classification of firm size of the European Commission neither isolate the medium-size firms because LoSaI dataset does not provide the same disaggregation of the relevant information.

[15] Following Lechner and Miquel (2010), we solve the simultaneous issue by postulating that S_t is determined before X_t.

non-colliders. As shown by the graph, to correctly identify the structural model, we need to cancel out all the possible feedbacks between the S_{t-1} and $\boldsymbol{X}_{1:t-1}$.

Figure 2: Direct analytic graph



Let us suppose, for example, that we want to estimate the path linking the tax credit at year t and wages, accounting for employment as a major time-varying confounding factor. Employment, by construction, is a post-treatment variable when we analyse the effect of S_{t-1} on wages, but it may be considered as a pretreatment variable when we look at the subsequent occasion S_t. To obtain an unbiased estimate of the effect of the tax credit on wages, we should condition on employment since it is a confounder but, at the same time, we need to exclude it since is also a post-treatment variable.

Following Robins (2000), when the treatment varies over time, the standard approaches for adjustment of confounding are biased. To analyse this dynamic model, Robins (2000) introduce the consistency assumption defining, for any subject, the observed outcome as a potential outcome (PO) corresponding to the observed treatment sequence. In the present case, this means to define the potential wage of a subject as she/he was treated by any possible sequence of treatment occasions. Using this structure, we define $Y^{\boldsymbol{s}_{1:T}}$ as the potential outcome (PO) for each treated sequence and specify the marginal structural model (MSM) that accounts for the relationship between the potential outcome and the treatment indicator as:

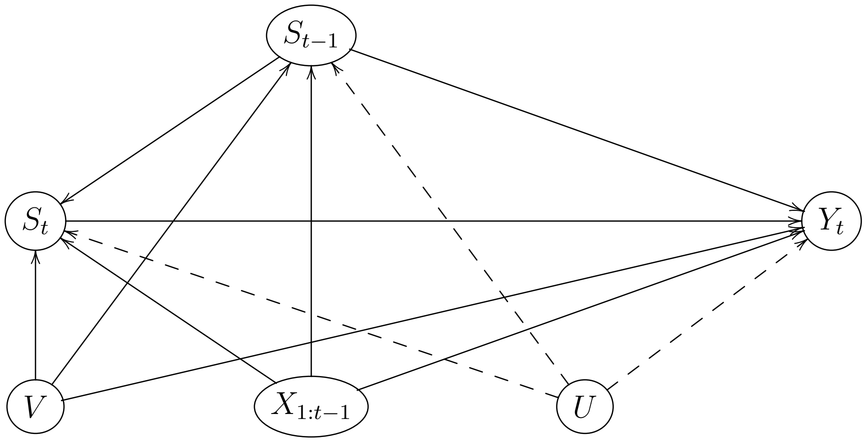
$$E(Y^{\boldsymbol{s}_{1:T}}) = \alpha + \boldsymbol{g}(\boldsymbol{S}_{1:T})'\beta \tag{1}$$

where $g(\boldsymbol{S}_{1:T})$ is a function summarising the treatment sequence, and β is the causal parameter that accounts for the effect of the tax credit on selected outcomes. Under the stable unit treatment values assumption (SUTVA), the common support requirement (CSR) and, in the dynamic setting, the sequential ignorability assumption (SIA), β is unbiasedly estimated using the inverse probability weighting (IPW) method (Robins, 2000). According to the IPW method, each subject is weighted by the inverse of the probability of its observed treatment sequence, with a weight defined as $1/\prod_{t=1}^{T} Pr(S_{it} = s_{it}|\boldsymbol{s}_{i,1:t-1}, \boldsymbol{x}_{i,s_1:t-1}\boldsymbol{v}_i)$. A stabilised version of the IPW weight is preferred to increase the efficiency of the IPW estimator. The stabilised weight reads:

$$w_i^{IPW} = \frac{\prod_{t=1}^{T} Pr(S_{it} = s_{it}|\boldsymbol{s}_{i,1:t-1})}{\prod_{t=1}^{T} Pr(S_{it} = s_{it}|\boldsymbol{s}_{i,1:t-1}, \boldsymbol{x}_{i,s_1:t-1}\boldsymbol{v}_i)} \tag{2}$$

where the denominator is estimated by a logit model, regressing the treatment variable on its past values for each individual and on the confounders included in the vectors $\boldsymbol{V}$ and $\boldsymbol{X}_t$. Similarly, the numerator is estimated using a logit model where we regress the treatment status on its lagged values.

Figure 3: Direct analytic graph



Now, suppose that the SIA assumption does not hold because there are some omitted confounding factors that have to be controlled for to obtain unbiased estimates of the relationship between the tax credit policy and wages. In the present case, the main concern with the SIA assumption is that LoSaI dataset does not provide some relevant information such as workers' degree of education, family background, marital status and country of birth. As all the relevant variables that should be included in the analysis are not available, the potential outcomes of the compared groups are not orthogonal to the treatment variable.

Figure 3 displays the causal path linking the treatment variable S_t and the outcome variable Y_t when an unobservable vector of non-colliders U is introduced into the marginal structural model. At this scope, we propose the latent class inverse probability weighting (LC-IPW) method that allows us to collect additional information on individual characteristics that were missing in our administrative dataset. In this respect, we modify the SIA assumption and propose a latent class sequential ignorability assumption (LC-SIA), defined as:

$$S_t \perp \boldsymbol{Y}^{(all)} | \boldsymbol{S}_{1t-1}, \boldsymbol{X}_{1t-1}, \boldsymbol{V}, U, \quad t = 1, \dots, T \tag{3}$$

where U is a categorical latent variable with categories $c = 1, ..., k$ and where $\boldsymbol{Y}^{(all)}$ is the vector containing all the potential outcome sequences $Y^{(\boldsymbol{s}_{1:T})}$. This assumption is weaker than the SIA and requires that the treatment indicator is independent from the potential outcomes, given the vectors of confounding variables and the unobservable confounding latent factor U. Hence, the weights are computed using probabilities conditioned on U, as:

$$\Pr\left(S_{it} = s_{it} | s_{i,1:t-1}, \boldsymbol{x}_{i,1:t-1}, \boldsymbol{v}_i, U_i = c_i\right) \tag{4}$$

where c_i describes the specific latent class the individuals belong to.

By using this framework, a two-step estimation procedure is applied, consisting in: i) fitting an auxiliary latent class model to assign subjects to latent classes and ii) fitting the MSM using weights that are estimated through Equation (4). The joint distribution of the observed variables can be written as:

$$f\left(\boldsymbol{v}, \boldsymbol{x}_{1:T}, \boldsymbol{s}_{1:T}\right) = \sum_{c=1}^{K} f\left(\boldsymbol{v}, \boldsymbol{x}_{1:T}, \boldsymbol{s}_{1:T} | c\right) \pi_c \tag{5}$$

where π_c is the probability for each individual to be included in this specific class such that $\pi_c = Pr(U = c)$.

The parameters of Equation (5) are estimated using the log likelihood, and the number of latent classes are chosen according to the Akaike information criterion (AIC) or the Bayesian information criterion (BIC). As reported in Appendix A, both the AIC and BIC criteria suggest a latent class model with three latent classes.

When the number of classes is determined, the subject is assigned to a latent class on the basis of the estimated posterior probabilities $\hat{q}_{ic}$, where

$$\hat{q}_{ic} = \frac{\hat{f}\left(\boldsymbol{\mu}_i, \boldsymbol{z}_{i,1:T}, \boldsymbol{s}_{i,1:T}|c\right)\hat{\pi}_c}{\sum_{c=1}^{k} \hat{f}\left(\boldsymbol{\mu}_i, \boldsymbol{z}_{i,1:T}, \boldsymbol{s}_{i,1:T}|c\right)\hat{\pi}_c} \tag{6}$$

Finally, in the second step, we fit the MSM (1) with the modified IPW procedure, where the weight of each subject is computed conditionally on the assigned latent class as follows:

$$w_{i,c_i}^{IPW} = \frac{\prod_{t=1}^{T} \Pr\left(S_{it} = s_{it}|s_{i,1:t-1}, U_i = \hat{c}_i\right)}{\prod_{t=1}^{T} \Pr\left(S_{it} = s_{it}|s_{i,1:t-1}, \boldsymbol{x}_{i,1:t-1}, \boldsymbol{v}_i, U_i = \hat{c}_i\right)}. \tag{7}$$

The standard errors and the corresponding confidence intervals are computed through non-parametric bootstrap.

5 Results

5.1 Wage effects

Table 2 reports the main results of the LC-IPW model for our main outcome variable, daily wages. The table reports the average treatment effect (ATE_{t_policy}) of the hiring credit, along with the potential outcome mean (POM). Further, the LC-IPW includes, according to the AIC and BIC criteria, three latent classes.

Table 2: Estimation results: wages

	Full sample	Males	Females
ATE_{t_policy}	-0.026	-0.019	-0.047
	(0.003)	(0.004)	(0.004)
	[0.000]	[0.000]	[0.000]
Potential outcome mean	4.043	4.166	3.847
	(0.002)	(0.002)	(0.003)
	[0.000]	[0.000]	[0.000]
Observations	164,518	100,447	64,071

Notes: ATE_{t_policy} is the average treatment effect of the hiring credit. The significance levels of p-values are shown in square brackets.

As the table clearly shows, the introduction of the tax credit overall produced a loss in the daily wages of workers exposed to the policy compared to the untreated workers (2.6%). This result is in stark contrast with the prevailing literature, showing that hiring credits have insignificant or small positive effects on the wages of treated workers (Saez et al., 2019; Egebark and Kaunitz, 2018).

In a similar spirit to the analysis of Saez et al. (2019), we are interested in investigating whether the aggregate finding masks sorting mechanisms and differential wage settings by treated firms generating wage dispersion among different groups of workers. However, the LoSaI dataset is not a representative firm-level database; hence, we cannot directly analyse firm fixed effects to detect sorting and potential rent-sharing responses that the aggregate focus on wages might have concealed. To capture this heterogeneity, we follow the more general literature claiming that the same labour market institution can have large heterogeneous effects in different labour market segments, even within the same country (Card et al., 2013, 2016, 2018). In this perspective, we replicate the previous estimate for the male and female subsamples. We find that for both groups, the tax credit had a negative effect on wages. More interestingly, this loss is substantially larger for women (4.7%) than for men (1.9%).

It could appear counterintuitive, at first glance, that the tax credit negatively affected the wages of workers hired with this policy and it must be explained why the wage gap shows a differential pattern by gender. Section 6 provides a theoretical framework for the analysis, whereas in what follows, we propose several arguments considering the different theories and channels provided by the existing literature.

We first discuss the negative impact of the tax cut on wages. Differently from the policy analysed in Saez et al. (2019), in our case, hiring credits are targeted to new hires of disadvantaged workers, whose outside option and reservation wage are expected to be low and whose bargaining power *vis-a'-vis* the hiring firms is supposed to be very weak. Indeed, we may argue that the policy activated matches that would have not been profitable in the prevailing tax regime. Hence, we expect that the treated workers could not effectively bargain to capture their share of the higher surplus produced by the reduction of firms' labour cost. In addition, we conjecture that the new hirings involve very low-productivity workers and that firms employ the marginal workers in bad jobs, i.e., jobs paid with a wage lower that the average market wage. In this case, the marginal labour productivity falls beyond the reservation wage of the marginal worker, but firms are willing to activate additional matches because they receive a subsidy that balances the lower marginal productivity.

Regarding the gender wage gap, we consider two competing explanations, which are observationally equivalent. The first explanation is related to the sorting mechanisms (Card et al., 2013, 2016, 2018) according to which productivity differentials and worker preferences determine an unequal allocation of genders across firms and jobs, and this, in turn, produces the gender wage gap. We may argue that women are more willing to accept the lowest-paid jobs because they are endowed with less human capital than men. However, our empirical analysis controls for idiosyncratic characteristics that influence the worker productivity; hence, the results do not reflect compositional effects due to ex ante differences by gender. The hypothesis that is consistent with our empirical strategy is instead that women's preferences are affected by time and family-related constraints on search and mobility or they may differently evaluate non-pecuniary versus pecuniary aspects of a job (Del Bono and Vuri, 2011). In particular, as Goldin (2014) shows, women place a higher value than men on temporal flexibility, and firms or sectors face different costs of providing it. Thus, workers sort across workplace accordingly (Blau and Kahn, 2017).

The role of different preferences and the wage penalty for flexibility and shorter hours of work may be particularly important for Italian women in the South for two related reasons: the lack of adequate formal care services provided by the local welfare system and the traditional role of women as caregivers who have to combine paid work and home production (Baussola and Mussida, 2014; Deidda et al., 2015; Sulis, 2012). Moreover, the option of the targeted women is to be unemployed or to be employed in the informal sector, whereas the tax credit could have helped regularising female workers (Baussola and Mussida, 2014)[16].

The alternative explanation relies on employers' discrimination and gender identity considerations affecting salary bargaining that determine a persistent wage gap beyond gender differences in productivity and women's preferences (Barth and Dale-Olsen, 2009; Flabbi, 2010;

[16] Deidda et al. (2015) found this result in a policy evaluation of the impact on female employment of a regional temporary programme of hiring subsidies targeted to the unemployed in Sardinia.

Olivetti and Petrongolo, 2016). According to this view, taste discrimination might reduce the outside options of women and their wage schedule, producing new hires at a lower wage compared to men (Becker, 1971). Following this literature, monopsonistic discrimination in wage policy is more applicable in specific occupations and sectors, such as the one with a high feminisation rate. Hence, women's segregation by industry and position is also consistent with gender discrimination (Petrongolo, 2004). These issues are further investigated in the next section.

5.2 Sorting and segregation effects

To empirically address the possible drivers of the gender wage gap for treated workers, we assess various heterogeneous effects linked to a set of selected covariates. Figure 4 reports the marginal effects obtained by the joint use of the LC-IPW and the regression adjustment methods. This analysis is aimed at testing whether the sorting effect of different treated workers across different firms, jobs and economic sectors contributes to explaining the gender wage gap produced by the tax credit and whether some residual effect might be consistent with the hypothesis of discrimination through segregation[17].

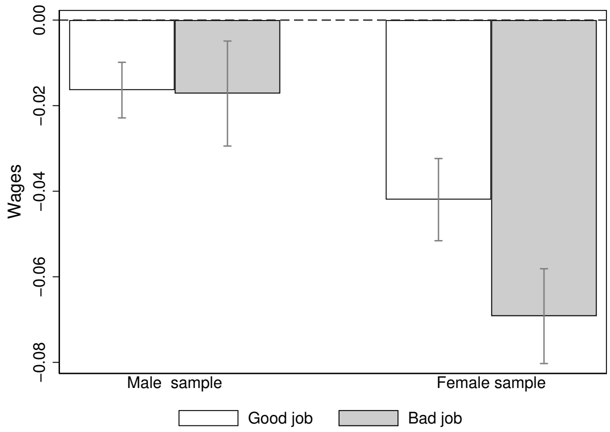
According to the sorting hypothesis, the lowest-ability workers are matched with the lowest-pay jobs or different jobs are matched on the basis of idiosyncratic preferences of employers and employees. For example, women prefer an hourly wage penalty for shorter hours, as stressed by Goldin (2014). Indeed, a body of literature shows that women tend to be employed in firms that pay a lower wage to everyone (Blau and Kahn, 2017; Petrongolo, 2004) and are overrepresented in firms providing a higher incidence of atypical jobs (Petrongolo, 2004).

The first panel of the figure (panel a, Figure 4) shows the marginal effect with respect to the variable indicating when a firm has a high concentration of atypical jobs (namely, bad jobs). The picture sketches a remarkable gender wage gap that is amplified when looking at treated workers in firms offering bad jobs and indicates that in the male sample, there are no significant differences between treated and untreated workers hired in different types of firms. This finding clearly indicates that women are more likely to be sorted in firms that allow for more flexible working arrangements and that treated women bear the highest penalty for flexibility. Hence, treated women are likely to be particularly time constrained, or not inclined to compete, irrespectively of their ability and education level, and they are willing to accept a marked loss of their earnings if they are compensated by a better balance between their family and work. This finding could also mask a discrimination through segregation component since males and females working in firms with a high concentration of bad jobs show such a relevant differential in pay standard. The employer's discriminatory behaviour may be due to pure taste-based discrimination or to cost reasons (i.e., the cost of granting maternity leave to a full-time female worker).

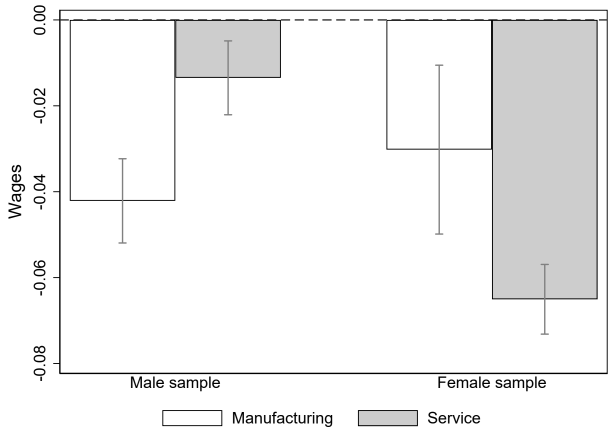
The second panel of the figure (panel b, Figure 4) presents the marginal effect by economic sector. We find that the wage gap significantly increases for workers in the service sector and decreases in the manufacturing sector. The heterogeneous effect in this case runs in the opposite direction for male and female workers, with men hired in the manufacturing sector experiencing a significant loss in wages (approximately 4%) with respect to their untreated counterparts, and women hired in the service sector suffering a loss of approximately 6.3% compared to the untreated female workers. Again, we find that the more negative gender wage gap is generated in the economic sector with the higher proportion of women. The explanation could be the self-selection of women in non-manual low-skill jobs in the service sector that comply with social norms and stereotypes (e.g., salespersons, cleaners, health care and social care practitioners), but this result could also be explained with some form of gender discrimination through the

[17] Segregation is defined as the concentration of a group of workers in a relatively small number of occupations or economic sectors (Flabbi, 2010).

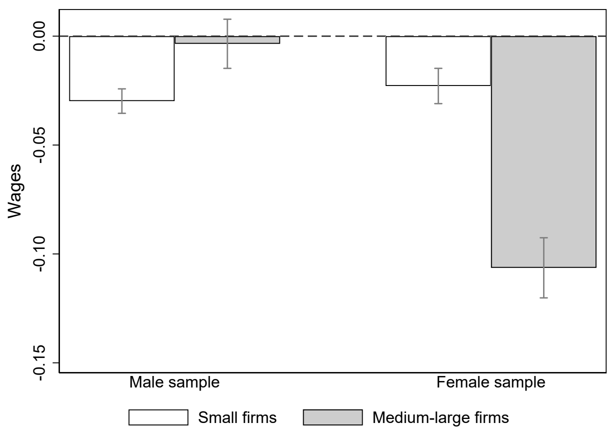
Figure 4: Marginal effects



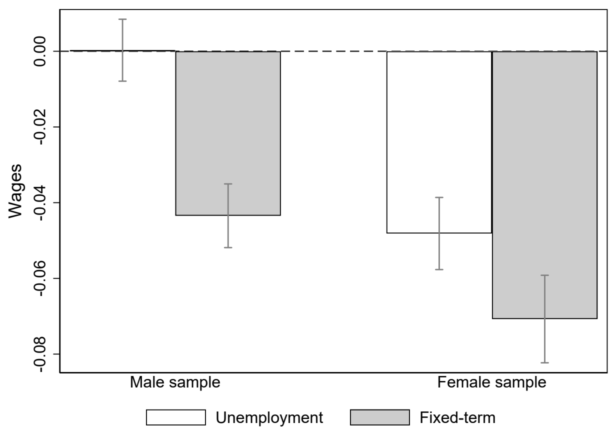
(a) Job quality



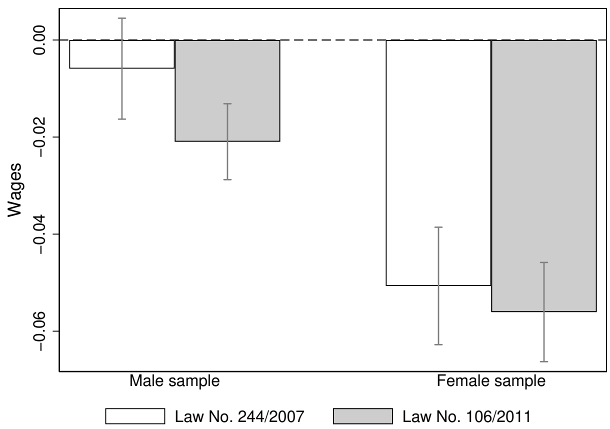
(b) Economic sector



(c) Firm size



(d) Previous status



(e) Policy design

Notes: The marginal effects reported here are obtained by the joint use of the LC-IPW and the regression adjustment methods. Tables of the estimated parameters are available from the authors.

segregation of women in some occupations that pay less or with a shortfall of women bargaining power in occupations with a high feminisation rate (Cardoso et al., 2016).

Other interesting results emerge when we consider the heterogeneity by firm size. From panel c, Figure 4, we find a significant wage loss for treated men (approximately 3%) and women (approximately 2%) hired in small firms and a sharp wage loss (approximately 10%) for female workers hired in medium-large firms. This may reflect a discrimination channel for two reasons. First, large firms are more likely to apply monopsonistic discrimination (Barth and Dale-Olsen, 2009). Second, large firms are associated with higher rates of vertical segregation due to their hierarchical structure and this, in turn, increases the gender wage penalty (Cardoso et al., 2016).

Panel d, Figure 4 displays a heterogeneous effect by the previous status of treated workers. Interestingly, the loss in wages for treated men occurs only when they had been previously employed in a fixed-term contract (approximately 4%). As firms could screen these workers, the wage penalty compared to the untreated workers suggests that they are perceived less productive than average and confirms that these matches would not have been activated without the hiring subsidy. Looking at the heterogeneity effect for the female sample, we find that treated women experience a wage loss of approximately 4% when they had been previously unemployed and a wage loss of approximately 6.3% when they had been previously employed in a fixed-term contract. Hence, the negative screening effect is even stronger for the female workers hired with the tax credit compared to men. This may reflect, again, the lower bargaining power of treated women or firms' discrimination against women.

The last aspect that it is worth analysing is the effect of the change in the policy design. We remind that we are considering two tax credit policies that have been implemented by Law 244/2007 (covering the time span 2008-2010) and Law 106/2011 (analysed for the time span 2011-2014). The two measures, as described in Section 2, differ somewhat and, in particular: (i) the first policy was more targeted to women because it included disadvantaged female workers, beyond unemployed and female individuals who had never worked, and it also provided a more generous benefit in the case of new hires of women; (ii) the second policy extended the focus on disadvantaged workers to the male population and, generally speaking, was more generous than the previous one. Hence, it is interesting to analyse whether the change in the target and dose of the measure have determined significant differences in the impact of the tax cut on the wages of male and female individuals. As shown in panel e, Figure 4, the negative wage gap between the treated and control male workers is significant only for the second policy intervention targeted to disadvantaged workers, whereas no significant difference is found in the female sample. This finding corroborates that sorting of disadvantaged workers to low-pay jobs is an important driver of the negative wage effect of the policy, and it also confirms that discrimination through segregation might have contributed to the gender wage differential.

Overall, these findings suggest that the policy produced a gender wage gap through the sorting channel, and we speculate that it is mainly based on potential gender differences in time constraints and in the evaluation of non-pecuniary versus pecuniary aspects of a job (Barth and Dale-Olsen, 2009). Although we cannot provide a direct test on gender discrimination, the estimated wage differentials by economic sectors and job allocation are consistent with some degree of discrimination through gender segregation across jobs, sectors and establishments because the evidence shows that the policy amplified the gender wage gap in large firms and in female-dominated jobs that pay less.

5.3 Earnings and weeks worked effects

We present further evidence related to other two outcome variables, i.e., annual earnings and annual weeks worked, to support our main analysis and give a more comprehensive picture of the distributional implications of hiring credits.

Table 3: Estimation results: earnings and weeks worked

	Full sample		Males		Females	
	Earnings	Weeks worked	Earnings	Weeks worked	Earnings	Weeks worked
ATE_{t_policy}	0.006	0.018	0.017	0.022	-0.024	0.009
	(0.003)	(0.002)	(0.004)	(0.003)	(0.006)	(0.004)
	[0.080]	[0.000]	[0.000]	[0.000]	[0.000]	[0.011]
Potential outcome mean	9.491	3.759	9.614	3.756	9.294	3.761
	(0.002)	(0.001)	(0.002)	(0.001)	(0.004)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
N	164,518	164,579	100,447	100,486	64,071	64,093

Notes: ATE_{t_policy} is the average treatment effect of the hiring credit. p-value significance levels are shown in square brackets.

Table 3 shows that the tax credit had a positive effect on the annual weeks worked of the male sample (2.2%), and overall produced a gain of 1.7 in their annual earnings. This result suggests that the policy had a countercyclical impact on the income of male workers, and that, in this case, hiring credits in the lagging Southern regions have complemented other automatic stabilisers that seek to boost workers' incomes when a recession occurs.

In contrast, we do not find any statistically significant variation in the weeks worked of women hired with the tax credit. Hence, we find a loss of 2.4% in the annual earnings of women exposed to the policy. This result supports the argument that the tax credit generated lower-quality matches for treated women compared to treated men. Looking jointly at our findings, the additional evidence confirms a strong asymmetry in the effects of the tax credit policy across women and men.

5.4 Robustness analysis

In this subsection, we analyse the effect of the firms' tax credit on wages over the period 2008-2014 by exploiting the discontinuity generated by the policy requirement, namely, that small firms benefiting from the tax credit have to retain the new subsidised hirings for at least two years and large firms for at least three years with respect to firms hiring workers without the tax credit. We apply a fuzzy RD design and use this discontinuity to identify the appropriate running variable. We restrict the sample and consider only workers that stay in the same firm after the hiring for a period between one and four years.

Under random assignment, the RDD provides an unbiased estimate of a weighted version of the local average treatment effect (LATE), evaluating the impact of the programme on individuals who were assigned and actually participated in the tax credit policy compared with those who were assigned but did not participate (non-compliers) (Hahn et al., 2001). The RD design that identifies a causal effect only locally or in the neighbourhood of the policy change (i.e., two or three years of employment after hiring, according to the policy requirement) is less generalisable with respect to the LC-IPW method, which also provides an unbiased estimate of the ATE when we move from the neighbourhood of the cut-off.

Table 4 lists the results of the RD design when we account for daily wages and consider the aggregate sample along with the disaggregation by gender. The correlation measures

Table 4: Estimation results: wages

	Full sample	Males	Females
$LATE_{t\_policy}$	-0.055	-0.038	-0.078
	(0.023)	(0.020)	(0.023)
	[0.018]	[0.095]	[0.001]
$ath\rho_{control}$	0.716	0.844	0.802
	(0.071)	(0.120)	(0.106)
	[0.000]	[0.000]	[0.000]
$ath\rho_{treatment}$	-0.286	-0.066	-0 .189
	(0.051)	(0.079)	(0.078)
	[0.000]	[0.404]	[0.016]
Wald test $\chi^2(2)$	135.25	50.81	63.25
	[0.000]	[0.000]	[0.000]
Observations	125,673	76,476	49,197

Notes: At the bottom of the table, we report the estimated correlation between the treatment-assignment errors and the outcome errors for the treated ($ath\rho_{treatment}$) and control groups ($ath\rho_{control}$), along with the Wald test that accounts for the presence of correlation between the treatment-assignment errors and the outcome errors for the treated and control groups.

($ath\rho_{treatment}$ and $ath\rho_{control}$) reveal the bias in the estimated coefficient due to unobservable confounders that differently affect the treated and control groups, whereas the LATE is the estimated impact. A positive selection bias in the control group suggests that the daily wage of the treated workers is downward biased and that the composition of the two groups is heterogeneous. That is, some treated workers are sorted in low-pay jobs not because they are treated but depending on their idiosyncratic characteristics. The Wald test rejects the null hypothesis of no correlation between the treatment-assignment errors and the outcome errors for the control and treatment groups, confirming the presence of unobservable confounding factors.

Moving to the estimated LATE parameters, we find that the effect of the introduction of the tax credit policy is not statistically significant with respect to the ATE estimates obtained by the LC-IPW. Indeed, comparing Table 2 to Table 4, in the aggregate sample, the estimated ATE (-0.055) relies on the confidence interval of the LATE. A similar result is also found when we use the disaggregation by gender. This result implies that the estimated effect of the policy is also stable when we analyse the effect in the neighbourhood of the policy cut-off.

6 An explanatory theoretical model

In this section, we analyse the effects of the tax credit on the labour market equilibrium from a theoretical perspective. The aim is to obtain an insight on the possible interpretation of a negative sign of the wage effect and on possible group differences in the wage effect, in particular taking into account the gender dimension.

The literature evaluating the impact of a tax credit policy (Egebark and Kaunitz, 2013, 2018; Saez et al., 2019), among others, and Cipollone and Guelfi (2006) for the Italian case) has usually found little employment effect, pointing at a rigidity of at least one of the two sides of the market.

In such a framework of modest quantity effect, the extent and the sign of the wage effect depends on which of the two sides of the market is the rigid one. Since the tax credit involves an upward shift of the demand curve, a rigid supply would imply, together with a small quantity effect, a strong positive effect on the wage, thereby shifting the large part of the subsidy to workers. In contrast, when the demand is the rigid side, the tax cut would continue to be incorporated into profits and the wage would be weakly affected. However, in the peculiar case of a negatively sloped supply curve, the upward shift of the demand curve has a negative impact on wages. In this case, a relatively small employment effect would be compatible only with a very rigid demand side. Hence, a negative wage effect, when not coupled with a significant employment expansion, would suggest that the tax credit is not an effective policy to support employment. Said differently, in this case, the main issue is not to shift the labour demand curve but rather to change its elasticity by preventing the marginal labour productivity from falling; otherwise, the main effect is no more than a transfer to profit earners.

As an aggregate downward sloping supply curve does not necessarily require individual downward sloping curves nor the absence of workers' bargaining power, we will consider a non-competitive labour market framework with rent sharing. In this model, also in the case of individual upward sloping supply curves and wages capturing part of the subsidy through a rent-sharing component, the wage effect on subsidised hirings can be negative, that is, workers hired with the tax credit may have, on average, lower wages than equally productive workers who have been hired without incentives. At the same time, as standard, the model assumes workers' labour supply to be increasing in wages and workers with the same productivity to have the same reservation wage.

Our model is an extension of the wage-setting framework proposed by Card et al. (2018) that is built on the Robinson' monopsonistic model of the labour market. In this framework, firms exhibit differentiated work environments over which workers have heterogeneous preferences. Workers derive utility from working by earning a wage and through some non-monetary aspects of the specific job and workplace, such as distance to work, starting times for work, and flexibility of working hours. Hence, for worker i, the indirect utility of working at firm j is:

$$u_{i,s,j} = \beta_s \ln(w_{sj} - b_s) + a_{sj} + \epsilon_{ij} \tag{8}$$

where the individual i of productivity type s chooses according to the wage posted by the firm j, the reservation wage or outside option in value b_s, the rent-sharing parameter β_s for workers of the same productivity type[18], the firm-specific characteristics a_{sj} common to all workers of the same productivity type and her/his individual preference for the firm-specific non-monetary aspects ϵ_{ij}.

The elasticity of the labour supply function $L_{sj}(w_{sj})$ corresponding to such a utility function is:

$$e_{sj} = \frac{\beta w_{sj}}{w_{sj} - b_s} \tag{9}$$

First-order conditions from the demand side are:

$$w_{sj}[\frac{1 + e_{sj}}{e_{sj}}] = T_j \mu_j f_s \tag{10}$$

where the right-hand side is the marginal product of type s at firm j, Tj and μ_j represent the firm-specific productivity and relative cost, and f_s is the marginal productivity of workers of type s. Taking together equations 9 and 10, the wage defined by the equilibrium solution is:

$$w_{sj} = \frac{1}{1 + \beta_s} b_s + \frac{\beta_s}{1 + \beta_s} T_j \mu_j f_s \tag{11}$$

[18]More in detail, β_s is the labour supply parameter that determines the firm-specific pay premium for workers of the productivity type s.

To simplify the exposition, the model in Card et al. (2018) considers two types of workers (high- and low-skilled workers), with the same parameter β and reservation wages b_s proportional to their type's marginal productivity, $b_s = bf_s$. This involves wages to be proportional to workers' productivity and forces at implicitly assuming that $T_j\mu_j > b$ to make the job activation profitable for a firm.

6.1 Marginal Jobs

Suppose now that instead of two types of workers, as in the original model, we have a continuum of productivity types $s \in (\underline{s},\ \overline{s})$ of workers. As in the baseline case of the original model, we assume that the firm has a linear production function [19] with μ_j representing the relative cost (with respect to the overall wage costs) of the firm's output:

$$Y_j = \int_{\underline{s}}^{\overline{s}} \lambda s ds \tag{12}$$

where the worker productivity is proportional to her/his type s. Defining $f_s = \lambda s$, equations 9 and 10 will still hold, and thus wages will be set according to equation 11. It is now worth noticing that a condition that is always implicitly satisfied in the baseline model with firm's rents is that hiring the worker is profitable for the firm, that is, $T_j\mu_j \geq wsj$. Considering this condition together with equation 11, it follows that:

$$T_j\mu_j f_s \geq b_s. \tag{13}$$

Our relevant model's extension is to assume that the reservation wage b_s is still increasing in the worker productivity, as in the original model, but less than proportionally[20]:

$$\frac{\partial b_\lambda(s)}{\partial s} \leq 1 \ . \tag{14}$$

We share the main rationale of the hypothesis that workers' reservation wages depend on their abilities, in particular because they mirror workers' outside options. This is the case when we consider the main outside options, which are moving or searching for a job in the informal economy. However, both of these options have fixed monetary and non-monetary costs. Furthermore, other outside options, such as relying on welfare or family transfers, are less likely to be related to productivity. As a result, it seems reasonable to assume that the reservation wage is less than proportionally increasing in worker productivity.

This assumption, by making the constraint in equation 13 binding, allows us to derive the marginal job framework. Indeed, considering equation 13, for a given firm productivity $T_j\mu_j$, there will be a limit regarding the productivity type of workers $s_m(j)$ below which the firm j will not hire any worker. Defining $b_\lambda(s) = \frac{b_s}{\lambda s}$ as the correspondent increasing function, and rearranging equation 13, we can write:

$$s_m(j) = b_\lambda^{-1}(T_j\mu_j) \ ; \tag{15}$$

where b_λ^{-1} is the inverse function of $b_\lambda(s)$, which is thus decreasing in s.

Correspondingly, given a type s that defines the worker productivity, there will be a minimal firm productivity $T\mu_m(s)$ below which no firm will find it profitable to hire a worker of type s, that is:

$$T\mu_m(s) = b_\lambda(s) \ . \tag{16}$$

[19] Card et al. (2018) also consider the non-linear case with imperfect substitutability between workers types and show that this implies non-linearities in the wage equation but does not significantly affect the main results.

[20] For the sake of simplicity, we hold the implicit original hypothesis of $b(s)$ to be a continuous function.

6.2 Tax credit and marginal jobs

We now turn to the analysis of the effect of the tax credit. We consider a policy design that involves only additional hirings, whereas the matches already realised follow the hypotheses of the model sketched in the previous subsection. This is not relevantly different than assuming that firms replace expired matches using the same wage-setting conditions of the previous model and create new jobs through the tax credit, which is the case investigated in the empirical analyses.

Our goal is to show that, for a given worker type s, the average wage of the additional subsidised matches might be lower than the wages of the matches activated without the tax credit.

The tax credit proportional to the wage cw for additional workers changes the firm first-order condition to:

$$w_{sj}[\frac{1+e_{sj}}{e_{sj}}] = \frac{1}{1-c}T_j\mu_j\lambda s \tag{17}$$

Accordingly, the profitability condition loosens and changes to:

$$T_j\mu_j \geq (1-c)b_\lambda(s) \ . \tag{18}$$

This implies that the thresholds $T\mu_m(s)$ and $s_m(j)$ in this case can be expressed as:

$$T\mu^c_m(s) = (1-c)b_\lambda(s) \qquad s^c_m(j) = b_\lambda^{-1}(\frac{1}{1-c}T_j\mu_j) \tag{19}$$

Since b_λ^{-1} is a decreasing function in the type index s, both thresholds will decrease:

$$T\mu^c_m(s) < T\mu_m(s) \qquad s^c_m(j) < s_m(j) \tag{20}$$

Indeed, the additional effect of the tax credit on employment entails exactly the activation of matches that would not have been profitable at the prevailing equilibrium wage because of the low productivity of workers and firms. This outcome will involve both the supply and demand side of the market. On the supply side, workers of lower-productivity type that previously were not hired by any firm are now hired. On the demand side, less productive firms that could not afford to increase employment can now hire new workers.

The wage for type s additional worker is:

$$w^c_{sj} = \frac{1}{1+\beta}b_s + \frac{1}{1-c}\frac{\beta}{1+\beta}T_j\mu_j\lambda s \qquad T_j\mu_j \in (T\mu^c_m(s); T\mu_m(s)) \tag{21}$$

According to the rent-sharing mechanism, the additional worker of type s, hired by the firms yielding a productivity included in the interval $(T\mu^c_m(s); T\mu_m(s))$, shares part of the rent induced by the tax credit.

However, on average, this may not give her/him a wage higher than the average wage of similar workers employed without the tax credit because of the lower productivity of the firm that increases net jobs through the subsidy. Indeed, considering equation 11 and equation 21, the wage difference between an additional worker of type s hired by the firm j_1 with the tax credit and a similar worker already employed by the firm j_2 (or newly hired by that firm without the tax credit) is:

$$w^c_{sj_2} - w_{sj_1} = \frac{\beta}{1+\beta}\lambda s(\frac{1}{1-c}T_{j_1}\mu_{j_1} - T_{j_2}\mu_{j_2}) \ ; \tag{22}$$

with:

$$T_{j_1}\mu_{j_1} \in (T\mu^c_m(s); T\mu_m(s)) \qquad T_{j_2}\mu_{j_2} \in (T\mu_m(s); T\mu_M) \ ; \tag{23}$$

where $T\mu_M$ sets the upper bound of firms' productivity.

When $T_j\mu_j(s)$ is close to $T\mu_m$, the difference above may be positive. However, in all other cases, the wage difference will have the opposite sign. That is, additional workers hired with the tax credit would have higher wages than workers of the same s type hired at market conditions only if compared with those employed in firms at the bottom of the firms' productivity distribution. If we look instead at non-marginal firms, the wage gap is negative.

The overall effect will thus depend on the difference between the productivity $\bar{T\mu}^c$ of the firms hiring new workers of type s with the tax credit and the average productivity $\bar{T\mu}$ of firms already employing or hiring new workers of type s without the tax credit:

$$\bar{w}^c(s) - \bar{w}(s) = \frac{\beta}{1+\beta}\lambda s[\frac{1}{1-c}\bar{T\mu}^c - \bar{T\mu}] \tag{24}$$

where:

$$\bar{T\mu}^c = \frac{1}{j(T\mu_m) - j(T\mu_m^c)} \int_{j(T\mu_m^c)}^{j(T\mu_m)} Tj\mu_j \frac{L_j(s)}{L(s)} dj \tag{25}$$

$$\bar{T\mu} = \frac{1}{j(T\mu_M) - j(T\mu_m)} \int_{j(T\mu_m)}^{j(T\mu_M)} Tj\mu_j \frac{L_j(s)}{L(s)} dj \tag{26}$$

and $j(T\mu)$ is the firm[21] with productivity $T\mu$, whereas $\frac{L_j(s)}{L(s)}$ is the share of type s workers in firm j.

As a result, the average wage of workers hired with the tax credit will be lower than the wage of similar workers already employed or newly hired without the tax credit whenever the subsidy does not compensate the difference between the productivity of firms already employing this type of worker and firms that have hired this type of worker thanks to the tax credit. Said differently, conditioned on workers of type s, the average wage effect of the tax credit is negative whenever the lack of profitability of previously non-activated matches is significantly dependent on the conditions of firms' productivity.

6.3 Gender-biased wage effect of the tax credit

In terms of the model of the previous subsection, women have a lower estimated coefficient β, thus they receive less of a firm's wage premium related to different fixed effects, and show a biased distribution across firms with lower fixed effects, thus their employment distribution $\frac{L_j(s)}{L(s)}$ is biased towards lower-productivity firms (Card et al., 2016).

As a result, when we consider gender differences, we have to take in account that, in the case of women, we have two opposite effects on the wage impact of the tax cut in equation 24 since the term $\frac{\beta}{1+\beta}$ is lower compared to men and the term $\bar{T\mu}^c - \bar{T\mu}$ is higher (in absolute value), on average. Hence, a stronger negative impact of the tax credit for women would be justified by the dominance of the segregation effect, that is, the relative bias of women's employment towards lower-productivity firms. The predominance of the segregation effect is compatible also with a different interpretation of the gender gap that is offered by the same imperfect labour market model. As (Card et al., 2018) note, a lower rent sharing is compatible with a same parameter β when we have a higher variance of the idiosyncratic component ϵ_{ij} since for a $\tau > 1$, the utility function:

$$u_{i,s,j} = \beta_s \ln(w_{sj} - b_s) + a_{sj} + \tau\epsilon_{ij} \ ; \tag{27}$$

[21] We assume, without loss of generality, $j(T\mu)$ to be increasing all over the interval $(T\mu_m^c; T\mu_M)$.

corresponds to the same preference space of the monotonic transformation as:

$$v_{i,s,j} = \frac{\beta_s}{\tau} \ln(w_{sj} - b_s) + \frac{a_{sj}}{\tau} + \epsilon_{ij} \ . \tag{28}$$

Furthermore, according to the rationale of the model, in a pooled equilibrium where women have higher variance of the idiosyncratic component, since they give more weight to non-monetary rather than to monetary aspects of the job, they would self-select in firms offering lower-paid jobs. In this case, the wage effect of the tax credit would always be worse for women than for men. It is worth noting that since the wage effect in equation 24 refers to a worker of a given s type, the compositional effect related to different ability distributions by gender is excluded.

Summing up, in a framework of imperfect labour markets, the evidence of a stronger negative effect of the tax credit for women suggests an interpretation of the gender wage differentials that is not based on different bargaining powers (implicitly, the parameter β of the utility function) per se but can be grounded instead on the higher sensitivity of women to subjective non-monetary aspects of the jobs. This specification is thus coherent with the segregation and the discrimination channels explaining the gender-biased wage effect of the tax credit.

7 Conclusive remarks

Hiring incentives have become popular *de facto* industrial and regional development place-based policies in Italy over the last ten years. This paper offers a comprehensive analysis of the wage impact of hiring credits, targeting the lagging regions in Southern Italy from the onset of the Great Recession up to 2014, when major similar policies at the national level occurred.

We estimate the effect of hiring credits on wages and run separate analyses for the male and female subsamples. This framework enables us to critically evaluate the economic rationale for place-based policies that put strong emphasis on reducing firms' labour costs to stimulate economic activity in disadvantaged areas characterised by a gender-based divide.

Our main finding is that the policy produced an overall negative effect on wages that is more pronounced for newly hired women compared to men. Our results also display that the tax cut has had the unintended consequence of worsening women's segregation in bad jobs, thereby contributing to widening the gender wage dispersion. We interpret these findings in light of the existing literature on wage determination with worker and firm heterogeneity and propose an extension of the model of monopsony in the labour market provided by Card et al. (2016, 2018) to clearly explain the main channels of the empirical results.

A more general remark concerns the effect of the policy on the average productivity of the economic system. We do not directly address this issue in the paper, but we show that by introducing the tax credit policy, it is worth sustaining low-productivity matches that firms would not have formed without the decrease in labour costs. This means that the policy advantages marginal firms and moves low-productivity workers, particularly women, from home production or from the informal sector to the formal sector. This shift may have a negative effect on the average productivity of the new jobs activated, which is the other side of the increase in wage dispersion. That is, the tax cut on labour costs of firms in lagging regions translates into a transfer to marginal firms, reinforcing the low competitiveness of the economic sector, an outcome that is exactly the opposite of the intended objective of these measures.

Overall, this study strongly suggests that a different mix of welfare, active labour market and development measures is required to promote opportunities in the highly segmented labour market of Southern Italy. Generally, as Kline and Moretti (2014) stress, making the tax system more progressive or strengthening means-tested programmes and targeting transfers based on income or demographic characteristics remain more direct, and potentially more efficient, ways to help those in need in lagging regions. In addition, the evidence suggests that disadvantaged female workers who face high unemployment rates and fragmented careers due to massive search and mobility frictions would require a mix of government measures aimed at improving their skills and productivity, on the one hand, and changes in the structure and remuneration of jobs at the firm level, on the other hand, in order to enhance more flexible working arrangements and facilitate women's work-family balance. Finally, an industrial policy aimed at increasing the productivity of firms would represent a preferable strategy for improving job creation in lagging regions compared to place-based policies that rely on subsidies to firms attempting to outweigh the competitive disadvantages due to localised market failures.

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Appendix A: Estimated results of the latent class method

Table A1: Model selection

Number of classes	Log likelihood	Estimated parameters	AIC	BIC
I	-3116615	35	6233301	6233654
II	-2975582	67	5951298	5951975
III	-2908770	102	5817743	5818774
IV	-2932161	135	5864592	5865956

Table A2: Posterior probabilities

	Marginal probability	Std. Error	[95% Confidence Interval]
Class I	0.594	0.004	(0.585 0.604)
Class II	0.301	0.004	(0.291 0.310)
Class III	0.104	0.001	(0.101 0.106)