LONG-TERM UNEMPLOYMENT AND SUBSIDIES FOR PERMANENT EMPLOYMENT[*]

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Abstract

We provide new evidence on the effectiveness of hiring subsidies targeting the long-term unemployed investigating a generous policy in force until the end of 2014 in Italy. Differently from other similar policies, this program was particularly ambitious as it promoted only permanent employment, which at the time still benefited from a strong employment protection legislation. To achieve identification, we use a triple difference estimator, where we exploit three sources of variation: (i) the subsidy was only for individuals in long-term unemployment and not for the short-term unemployed; (ii) it was significantly more generous in the South of Italy; (iii) it was in place until 2014. We find that the relative probability of finding a permanent job by eligible individuals in Southern regions experienced a drop after the abrogation of the program. The effect does not seem to be driven by substitution over time, across contracts or among jobseekers. A cost-benefit analysis shows that the policy was globally in surplus.

Key words: long-term non-employment, triple difference estimator, employment subsidies, place-based policy, regional disparities.

JEL Codes: H25, J08, J64, R23

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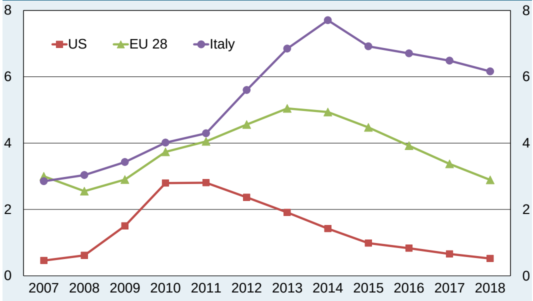
1. Introduction

Figure 1

Long-term unemployment rate (1)
(percentage)



Source: OECD.
(1) Unemployed for more than one year as percentage of labor force.

Long-term unemployment (LTU) is one of the main legacies of the Great Recession. Since 2008, many developed countries have experienced a steep rise in LTU rate, which was particularly striking in European countries (figure 1).[1] In 2018, even after the rate fell by two percentage points from its peak, more than 7 million individuals living in the European Union were long-term unemployed, two fifths of the total number of unemployed workers. Even in the US almost one million individuals were still in this condition.

The reasons why LTU is a policy concern are multiple. Firstly, unemployment is one of the most significant causes of households' poverty. The probability to find a job tends to decrease with time spent in unemployment, because both workers' human capital and the intensity of job search may decline over time. LTU might therefore increase poverty persistence. Secondly, since workers detached from the labour market do not compete for jobs, long-term unemployed jobseekers play only a reduced role in compressing wages and, thus, in decreasing the total amount of unemployment (Machin and Manning, 1999).[2]

These concerns about the consequences of LTU motivated a number of different policies across developed countries. In particular, many countries addressed the problem by using active labour market policies (ALMPs), whose effects are typically more positive in the attempt to combat LTU than short-term unemployment (Bentolila and Jansen, 2016).

At the same time, the number of scientific evaluations of these programs has exploded. In particular, Card et al. (2018) undertake a review of the recent literature, by assembling a sample of 207 evaluation studies that provide 857 separate estimates of program effectiveness, and Card et al. (2016) repeat the analysis from the particular viewpoint of long-term unemployed jobseekers. Among ALMPs, the authors show that larger gains have been observed for programs that emphasize human capital accumulation. They include training provision and promotion of search effort (through job search requirements, sanctions and even direct help).[3] Other policies tried instead to encourage firms to hire long-term unemployed through subsidies. The evidence on their effectiveness is mixed: the share of programs that had a significant positive effect (less than 60 percent) is lower than for other ALMPs. One reason why subsidies are less effective is the risk that hiring would have taken place even without any public intervention. In this case subsidies may

[1] In the European Union the LTU rate reached 5.0 percent in 2013, almost doubling compared to 2008. The LTU rate is defined as the share of active individuals who are out of work and have been actively seeking employment for at least one year over the labour force.

[2] More generally, Austin et al. (2018) discuss "three types of externalities associated with non-employment: pure fiscal losses from reduced taxes and increased social spending; social spillovers born by family and friends; and not working spillovers where one individual not working increases the chance that other individuals don't work", because of decreased demand for local products, which reduces local labor demand, reduced stigma of not working, or if the not working enjoy being with each other.

[3] As far as training programs are concerned, even if in the short term they are poorly effective (Heckman et al, 1999), in the long term their effects seem to be positive and significant. Job search assistance and search requirements typically have an even greater beneficial impact, especially in the short term: these programs often lead to a reduction in the unemployment spell even if only some workers are being employed in a new job (Card et al, 2015; Manning, 2009 and Petrongolo, 2009 on UK).

lead only to a substitution among workers and over time. Thus, they may have detrimental effects on people who are not targeted, as they face stronger job competition from those who are (Crépon et al., 2013).

The structure of the policy makes it a good example of spatially targeted employment subsidy, which Austin et al. (2018) identify as the most effective place-based policy if target areas are those with the highest elasticity of employment to wages. They provide evidence that such areas are those where non-employment is higher. Moreover, distressed areas are characterized by lower prices, which provide additional support to spending more in places where costs are lower, and lower macroeconomic costs of supporting not working, since inflationary pressure due to reduced unemployment is more limited compared to full employment areas. Finally, they find that redistribution across space is more likely to enhance welfare when migration is lower. However, they admit that “high not working rate areas might have social problems that lead even fewer people to be on the margin of working”, displaying “extremely inelastic labor demand, so that few new jobs will be created because of a subsidy”. Therefore, the effectiveness of this policy is, ultimately, an empirical question.

This paper aims at studying the effects of a subsidy introduced in Italy by law no. 407 of Dec. 29th 1990 and in force until the end of 2014. The program targeted only firms that hired with a permanent contract a worker who had been unemployed for at least 24 months or covered by the national short-time work compensation scheme.[4] Its amount was greater for firms in Southern Italy (100 percent of social security contributions for 3 years; only 50 percent for firms in other Italian regions). This differential treatment in favor of Southern regions and long-term unemployed was abrogated in 2015 by the Financial Stability Law, which introduced exemption from social security contributions without distinctions between areas and duration of unemployment (although individuals with a permanent contract in the previous six months were excluded).[5]

This is an interesting case study for a number of reasons. First, Italy is one of the European countries where the LTU rate increased the most during the recession (5 percentage points from 2007 to 2014), and even more in Southern regions (8 percentage points), where LTU rate has steadily doubled the national one. Second, participation rate is particularly low, especially in the South, where the gap with the national average increased from 6 percentage points in the ‘90s to 11 percentage points during the last decade; thus, the long-term unemployed may be more at risk of leaving the labour force. Third, this program was a relatively big one. Italy’s public expenditure for recruitment incentives has been equal to 0.2 per cent of GDP in the period 2004-2015, twice the OECD average, representing 36 per cent of total public expenditure for ALMPs[6]. In 2014 the program involved 260,000 hires in the South, 37,000 in Center-North. Finally, this policy was particularly ambitious for its focus on permanent contracts. Being these contracts generally more expensive for firms, not only in terms of social security contributions, but also because of the stronger employment protection legislation (Grassi, 2009), employers may find it riskier to hire people detached from the labour market with a permanent contract.

We use a sample of administrative micro-data about job flows (*Campione Integrato delle Comunicazioni Obbligatorie*, CICO) and select unemployed individuals that lost their job between 2009 and 2013, for whom we can observe the labour market history until the end of 2015. To achieve identification, instead of using simple diff-in-diff estimators (for instance comparing eligible vs non eligible individuals across areas), we employ a triple difference estimator (DDD) that exploits variation in the relative cost of hiring with a

[4] *Cassa integrazione guadagni* (CIG).
[5] On the effects of the 2015 hiring subsidy look at Sestito and Viviano (2018).
[6] The corresponding figure for the OECD countries is 14 per cent.

permanent contract across time, regions and worker's unemployment length. This choice suits the design of the subsidy and its recent history: law 407/90 granted a preferential treatment to unemployed for at least two years in the South compared to those living in the North, until this preferential treatment ended abruptly with the abrogation of the law at the end of 2014. Therefore, it seems natural to compare how this advantage along two dimensions (eligible vs. non eligible and South vs. North) evolved over the third dimension (time). Intuitively, the DDD approach exploits these three dimensions to remove (i) underlying differences between eligible and non eligible unemployed; (ii) area-specific time trends and (iii) differential time-trends for the eligible. We implicitly assume that, without any targeted subsidy, the choice to hire with a permanent contract an eligible rather than non eligible unemployed would have changed in 2015 in the same way in Northern and Southern regions.

Despite these advantages, our DDD strategy is still only able to capture the differential effect on the eligible versus non eligible unemployed. The positive effect on the former might come at the expense of the latter, for instance if the unemployed will be pushed to wait until reaching the eligibility status in order to benefit from the policy. The estimate is also affected by possible issues of substitution over time (where firms anticipate the end of subsidies), across different types of contracts (where individuals have an advantage in avoiding short-term contracts that would end their eligibility status) and across areas (where individuals move to exploit the preferential treatment). Through a series of robustness checks we provide evidence that our results are not driven by these issues.

Pasquini et al. (2018) also provide an evaluation of law 407/90 subsidies using CICO data and find a positive effect on the probability of getting a job. They use a Regression Discontinuity Design (RDD), looking at unemployed workers in a bandwidth of two weeks around the 24 months threshold to become eligible for the subsidy. Since individuals close to the threshold have a strong incentive to wait, we believe this approach may be flawed by the effect of intertemporal substitution, inducing an upward bias in the estimates. Moreover, their result is not comparable with our own, since their outcome variable is the share of unemployed jobseekers who find any kind of job, while we look at the probability of being employed with a *permanent* contract, as the subsidy only benefitted this type of contract.

We find that, after the abrogation of law 407/90, eligible individuals in Southern Italy experienced a fall in their probability of finding a permanent job relative to non eligible unemployed in Center-North. This implies that the targeted subsidy, in place until the end of 2014, was effective in rising their chances in the labor market. We estimate a 41 per cent higher probability of finding a permanent job in the subsequent week and argue that the effect is not driven by substitution over time or across areas, type of contract or category of jobseekers. Moreover, we find that the benefits deriving from jobs created thanks to the policy, measured by tax revenue and social security contribution paid by the employees, outweigh the costs of the policy, given by the amount of the subsidy.

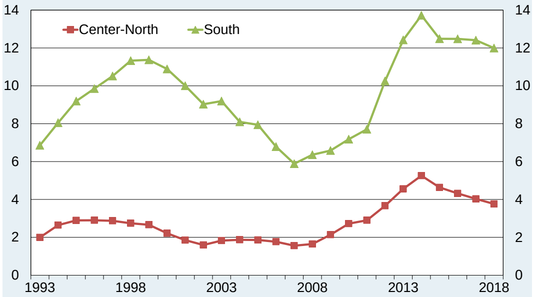
The rest of the paper is organized as follows. In Sections 2 and 3 we describe the subsidy and our dataset, respectively. Section 4 describes aggregate trends to highlight differences across regions, time and eligibility status. In Section 5 we describe the empirical strategy adopted, define our treatment and control groups, and present the results. Section 6 provides evidence that our results are not driven by substitution effects over time and across workers by performing robustness checks and reporting results on additional outcomes. In section 7, we perform a cost-benefit analysis. Section 8 concludes.

2. The subsidy granted by law 407/90

Until 2014, permanent hires of individuals unemployed or covered by the national short-time work compensation scheme for at least 24 months benefited from a subsidy granted by law 407/90. The subsidy was equal to 100 per cent of social security contributions for three years in the South, 50 per cent in the rest of Italy. The favorable treatment of Southern regions aimed at increasing LTU employability especially where this was more needed. Indeed, the incidence of LTU over the labour force is strongly higher in the South than in Centre-North (see figure 2). From 2008 to 2014 both areas experienced a sharp increase in LTU rate, but the former has been stably three times the latter.

Figure 2

LTU in Italian regions (1)
(percentage)



Source: Istat, Italian labour force survey.
(1) Unemployed for more than one year as percentage of labor force.

Only firms that had not experienced any firings, workers' suspensions, voluntary resignation or the end of a temporary contract in the previous six months were eligible for the policy. Despite the fact that in June 2012 the rules defining firm's eligibility were relaxed, the usability of this subsidy remained quite low. Accordingly, the take-up was especially low in Centre-North, where the gain from the subsidy was smaller. On average, almost 300,000 hires per year benefited from the subsidy from 2012 to 2014; almost 90 percent of them in Southern regions.[7]

The Financial stability law for 2015 abolished law 407/90 and introduced a new non-targeted and non-conditional subsidy (with a cap to 8,060 euro per year for three years) to all permanent hires signed from January to December 2015. With respect to previous policies, a more extensive audience could take advantage of the subsidy: the only constraint to workers' eligibility was not having been employed with a permanent contract in the six months before the new hire and not having worked with a permanent contract for the same firm asking the subsidy in the three months before the law was passed (October-December 2014). In 2015, 630,000 hires benefited from the subsidy; about 30 per cent of them in Southern regions.

Because of the abolition of law 407/90, the relative cost of hiring with a permanent contract an individual unemployed for at least 24 months increased both in Southern and in Northern regions. Moreover, individuals who were not long-term unemployed and did not have a permanent contract in the previous six months experienced a strong cut in social security contributions, as they were previously not eligible for similar allowance. Furthermore, the new 2015 subsidy was introduced along with a broader labour market reform package, the Jobs Act, which was passed at the end of 2014. In particular, this reform introduced a cut in firing costs for all new permanent contracts signed from March 7th 2015 onward, and a new insurance scheme against the risk of unemployment covering a broader set of workers (*Nuova Assicurazione Sociale per l'Impiego, NASpI*[8]). Since these policies didn't affect contemporaneously the *relative* outcomes of eligible and non eligible in the same area and year as the treatment, we believe our identifying assumption still holds.

[7] Source: INPS, *Statistiche in breve*, *Politiche Occupazionali e del Lavoro*.

[8] Moreover, the duration of NASpI, in force since May 1st 2015, is equal to half the period for which the worker paid social security contributions in the previous four years. Instead, previous unemployment insurance scheme related duration with age, with a minimum of eight months for under 50 and a maximum of 16 months for over 54.

In table 1 we compute the amount of the three-year exemption for a gross annual wage of 26,000 euro in the two policy regimes.

Table 1 – Cumulated three-year exemption from social security contributions for permanent hires (1)

Area	**Unemployment duration**	**2014** (2)	**2015** (3)	**Difference (2015-2014)**
South	Short	0	18.720	18.720
	Long	20.274	18.720	-1.554
	Difference (L-S)	20.274	0	-20.274
Centre-North	Short	0	18.720	18.720
	Long	10.088	18.720	8.632
	Difference (L-S)	10.088	0	-10.088

(1) We refer to a gross yearly wage of 26,000 euro, a social security contribution rate for the employer equal to 24 per cent of the gross wage, an INAIL premium equal to 485 euro for Centre-North and 518 euro for the South. – (2) The exemption granted by law 407/90 concerned permanent hires of unemployed individuals and those covered by the short-time work compensation scheme for at least 24 months. – (3) The exemption granted by the Financial Stability Law for 2015 concerned all permanent hires, from January to December 2015, of individuals who did not have a permanent contract in the previous 6 months.

Hiring trends between 2014 and 2015 were strongly affected by these interventions. Sestito and Viviano (2018) provide evidence that both the new subsidy and the reduction in firing costs had a significant positive impact on gross permanent hires. However, the novelties introduced in 2015 applied to both our treatment and control groups (in the way we defined them in Section 3) homogeneously across the country. Therefore, if law 407/90 had never been present, the two groups would have been affected similarly by these job market interventions across different geographic areas, and this justifies the assumption of common *relative* trends that is behind our estimator (see Section 5.1).

3. Data

We use a sample of administrative micro-data about the so-called *Comunicazioni Obbligatorie*, which contains information concerning job positions. Starting from 2009, whenever an employment contract is signed, terminated or changed, employers must electronically submit this information to the Regional agency in charge of active labor market policies, which forwards it to the Ministry of Labor. The administrative archive built on these communications contains, therefore, information on all contracts that were signed, terminated or changed starting from 2009.[9] The Ministry releases a sample of micro-data relative to all workers born on 24 dates (the 1st and 15th day of each month).[10] In this work we use the December 2015 release.

Starting from this dataset we build a weekly panel, by recording job status (unemployed, employed with a fixed-term contract or a permanent contract) for each worker in every Monday between January 2009 and December 2015.[11] Even if our empirical analysis focuses on years 2014-2015, we use workers' job history

[9] For contracts that were signed before then but were changed or terminated after January 1st, 2009, employers had to submit the entire job history and therefore they are fully included in the archive. On the contrary, the archive does not contain any information on contracts signed before 2009 that were neither changed nor terminated thereafter.

[10] Every record contains the following information: employer and employee anonymized identifiers, dates in which the position is created and destroyed, employee's year of birth, gender, region of birth, nationality, schooling, region of residence, region of work, sector of activity, job contract type, full- or part-time status, role, any hiring subsidy granted, reason of job destruction and, for a subsample, wage.

[11] Setting up a panel at daily frequency would lead to a hardly-manageable large dataset, without bringing significant gains. In fact, we are interested in identifying transitions from unemployment to permanent employment in 2014 and

since 2009 in order to determine, for each individual, a starting point of her unemployment spell (see Appendix for more details). We follow each worker during her entire job history until age 64. We focus only on the effect of the measure on the non employed, and not also on beneficiaries of the short-time work compensation scheme, since we are not able to identify this latter group.

The definition of unemployment status relevant for law 407/90 differs from the one of the International Labour Organization (ILO), commonly adopted in Labour Force Surveys, where people aged 15 and over are classified as unemployed if they are without work, are available to start working within two weeks and sought employment at some time during the previous four weeks. Furthermore, ILO calculates unemployment duration since the loss of the last job.

On the contrary, according to law 407/90 definition, the unemployment status does not require any frequent job search action. Looking at the rules applying in 2013-14, individuals need to be registered as unemployed in a Job Centre (*Centri per l'impiego*), formally declare to be willing to work and, in principle, accept adequate job offers. Furthermore, the unemployment duration is not set back to zero, but just suspended, during short periods of employment. The time limit necessary to consider a period as "short" changed repeatedly during time and across areas. We chose the one prevalent in our period of analysis, which was six months for the Centre-North, 4 or 8 months for the South according to whether individuals were younger or older than 25 years. The policy also had an additional rule, according to which individuals were still considered unemployed if they got jobs earning less than the no tax area limit (8,000 euro per year). Since for a large fraction of our observations the information for wage is missing, we prefer not to employ it to avoid using a potentially strongly selected sample. In the Appendix we show that results would be similar if we consider this income-threshold rule or if we use a simplified rule homogeneous across areas.

4. Trends in permanent hires

Figure 3 shows the time series of hires with a permanent contract in the two geographic areas. After 2015 hires went up both in the South and the Centre-North, as expected with a new legislation that introduced more generous benefits for most of the new open-ended contracts. Figure 3c shows that the rise is relatively larger in the Centre-North. In December 2014, slightly before the change in the system, firms in the South increased the relative number of permanent hires, as shown also by the peak in figure 3a. This could be explained by some firms taking advantage of the older benefits for the long term unemployed granted by law 407/90, which were more generous in that area.

To understand whether the change in the relative number of hires between the South and the rest of the country might be due to the disappearance of the preferential treatment for the eligible in the former area, we show two additional pieces of evidence. Figure 4a removes from the time series those contracts that actually received the subsidy of law 407/90. Without considering them, there is no strong change in the ratio between the two series in 2015 and the pick of December 2014 disappears. Since it is difficult to draw conclusions from the actual receipt of the subsidy, which strongly reflects the endogenous choice of firms, figure 4b provides the same South/Centre-North comparison but looks only at non employed individuals

2015. Therefore, observations at weekly frequency are a sufficiently good approximation, since the maximum measurement error is six days. The only moment in which this approximation is problematic is in the week across the two years, because we risk to wrongly attribute to year 2015 transitions occurred in the last days of 2014. To obviate to this measurement error issue, we eliminate observations concerning both the last Monday of 2014 and the first Monday of 2015.

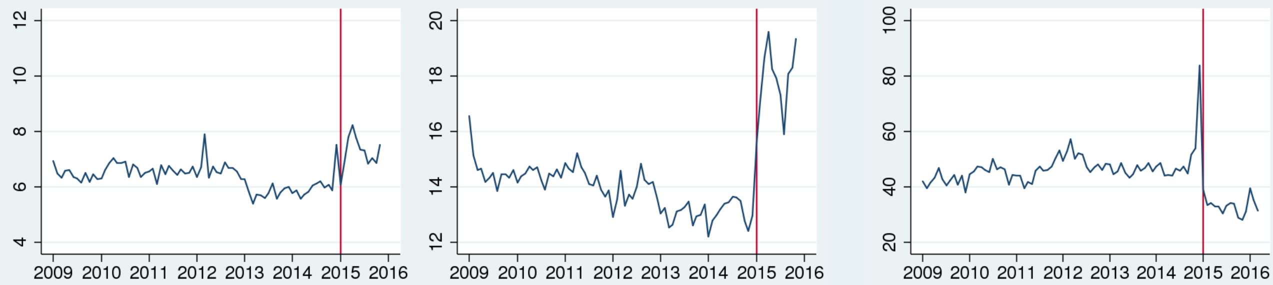
and splits them according to their non employment duration. Before 2015, relative hires of the eligible in the South, with respect to the rest of the country, were higher than those of non eligible. This difference disappeared in 2015.

Figure 3

Permanent hires (1)

(monthly data; thousands and percentages)

(a) South | (b) Centre-North | (c) South over Centre-North



Source: our elaboration on Ministry of Labor data, *Campione Integrato delle Comunicazioni Obbligatorie* (CICO).

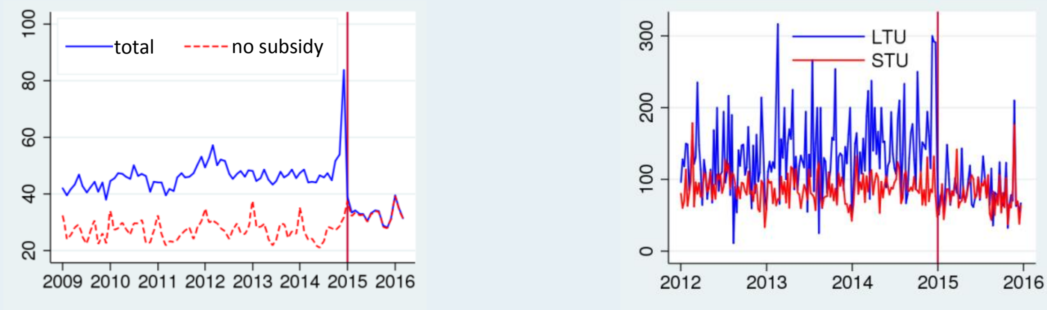
(1) Seasonally adjusted series, obtained by subtracting from the raw data the OLS estimate of hires on a set of separate monthly dummies until September 2014, when the Financial Stability Law had not been announced yet.

Figure 4

Permanent hires: South over Centre-North

(percentages)

(a) Contracts that did not receive the subsidy of law 407/90 (1) | (b) Hires of previously non employed by eligibility status (2)



Source: our elaboration on Ministry of Labor data, *Campione Integrato delle Comunicazioni Obbligatorie* (CICO).

(1) Monthly data. Seasonally adjusted series, obtained by subtracting from the raw data the OLS estimate of hires on a set of separate monthly dummies until September 2014, when the Financial Stability Law had not been announced yet. (2) Weekly frequency. LTU (STU) are individuals with non employment duration of at least (less than) two years as defined in Section 5.2.

5. Micro analysis

5.1 Estimation strategy

As shown by the aggregate trends, law 407/90 seems to have had a positive impact on permanent hires of eligible individuals in Southern regions. In order to provide further evidence, we switch to a micro-level analysis, which also allows us to perform several additional robustness checks to assess the sensitivity of our conclusions. We focus on a panel composed of all unemployed individuals over the weeks between January 2014 and December 2015 and estimate a discrete-time hazard model which predicts the probability to find a permanent job in the subsequent week, conditional on the logarithm of unemployment

duration[12]. As standard in the literature, we use a logit specification, assuming therefore proportional odds of exiting unemployment in each week. All the comparisons between groups are therefore expressed in odds ratios, although we will also use the predictions to make comparison about the probability of getting a permanent job. In the period of analysis, we are able to exploit three different sources of variation:

- law 407/90 targeted only individuals with unemployment duration of at least two years;
- the subsidy was more generous in Southern regions;
- the preferential treatment for long-term unemployed in Southern regions disappeared in 2015, when the new system of generalized hiring subsidies and firing rules was introduced.

The outcome of interest is a dummy y_{iw} equal to 1 if individual *i=1,…,N* in week *w=1,…,W* finds a permanent job in the subsequent week, 0 if she doesn't.

Defining $\lambda(y_{iw}) = Pr(y_{iw} = 1 | \, y_{iw-s} = 0, \forall \, s < w)$, one could start from a DD that compares the eligible and the non eligible across the two areas when law 407/90 was still in place (year 2014):[13]

$$logit[\lambda(y_{iw})|g, LTU_{iw}] = \gamma_0 + \gamma_1 LTU_{iw} + \gamma_2 1[g = South] + \gamma_3 LTU_{iw} \times 1[g = South] + \varepsilon_{iw} \qquad (1)$$

where $g \in \{Centre - North, South\}$ is the area, LTU_{iw} is a dummy equal to 1 if unemployment duration is longer than 2 years. The reference group is made of non eligible individuals[14] in Centre-North regions.

In (1) the double comparison of interest is $\exp(\gamma_3)$, which captures the across-area difference in the LTU vs. STU performances. We expect it to be positive because the subsidy was much larger in Southern regions. If we assume that, absent the policy, the difference between the eligible and the non eligible had been the same in the two areas, then $\exp(\gamma_3)$ would capture the causal effect of interest, i.e. the impact of the more generous subsidy granted to permanent hires in the South.

This assumption is rather strong, and might be violated if structural differences between the South and the Centre North affect differently LTU and STU probabilities of being hired with a permanent contract. We therefore opt for a triple difference estimator, exploiting the fact that the differential treatment for Southern LTU workers was abolished in 2015. This allows us to use three control groups (STU in each area and LTU in the Centre-North) to control for underlying differences in the LTU vs. STU comparison across areas. With respect to the DD strategy outlined above, the DDD estimator is based on a weaker assumption: it requires that no contemporaneous shock affects the *relative* outcomes of the treatment group (the eligible) compared to the control group (the non eligible) in the same area and year as the treatment (Gruber, 1994).

The DDD specification is the following:

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[12] We also estimate models where the unemployment duration enters in a more flexible way, like a third order polynomial function and a piecewise constant function, and results are unchanged, as we report in the Appendix (Tables A5 and A6).

[13] An alternative would be to focus only on the eligible and compare them across the two areas before and after the abrogation of law 407/90. This strategy would require that changes in macroeconomic conditions and the overall reform of the labor market introduced in 2015 would have affected the two areas in the same way. This assumption is hard to believe, given the strong structural differences between the less developed Southern regions and the rest of the country.

[14] From now on we use the abbreviation LTU (STU) to refer to the group of individuals eligible (non eligible) for the subsidy, whose non-employment duration is at least two years (six months or one year, according to the specification).

$$
\begin{aligned}
&logit[\lambda(y_{iw})|t, g, LTU_{iw}] = \\
&\qquad \beta_0 + \beta_1 LTU_{iw} + \beta_2 1[t = 2015] + \beta_3 1[g = South] \\
&\qquad\qquad + \beta_4 LTU_{iw} \times 1[t = 2015] + \beta_5 LTU_{iw} \times 1[g = South] \\
&\qquad\qquad\qquad + \beta_6 1[t = 2015] \times 1[g = South] + \beta_7 LTU_{iw} \times 1[t = 2015] \times 1[g = South] + \varepsilon_{iw} \qquad (2)
\end{aligned}
$$

where $t \in \{2014, 2015\}$ is the year. The coefficient in the triple interaction captures the difference across areas in the trend of relative performance of LTU vs. STU, which is given by *A-B*, where

$$
\begin{aligned}
A = &\{logit[\lambda(y_{iw})|t = 2015, g = South, LTU_{iw} = 1] - logit[\lambda(y_{iw})|t = 2014, g = South, LTU_{iw} = 1]\} \\
&-\{logit[\lambda(y_{iw})|t = 2015, g = South, LTU_{iw} = 0] - logit[\lambda(y_{iw})|t = 2014, g = South, LTU_{iw} = 0]\} \quad (3)
\end{aligned}
$$

and

$$
\begin{aligned}
B = &\{logit[\lambda(y_{iw})|t = 2015, g = CN, LTU_{iw} = 1] - logit[\lambda(y_{iw})|t = 2014, g = CN, LTU_{iw} = 1]\} \\
&-\{logit[\lambda(y_{iw})|t = 2015, g = CN, LTU_{iw} = 0] - logit[\lambda(y_{iw})|t = 2014, g = CN, LTU_{iw} = 0]\} \quad (4)
\end{aligned}
$$

The causal interpretation of this coefficient lies on the assumption that, in the absence of a preferential treatment for the South (i.e. in the absence of law 407/90), the trend in relative performance of LTU vs. STU would have been the same in the two areas (common *relative* trends).[15] Under this assumption, β_7 is different from zero only if the preferential treatment of LTU in Southern Italy granted by law 407/90 had an impact on the chances of finding a permanent job. If the impact was positive, then β_7 should turn out to be negative (and $\exp(\beta_7) < 1$), because in 2015 the advantage for LTU workers disappeared.

One crucial issue in the interpretation of the DDD result is that there were two contemporaneous policy changes: the targeted subsidy of law 407/90 was abolished, but a generalized subsidy was introduced and the employment protection legislation was weakened through the Jobs Act. In the DDD estimates we do not separately identify the effect of the two latter policies (generalized hiring subsidy and Jobs Act), which are captured by $1[t = 2015]$ and $1[t = 2015] \times 1[g = South]$. Nevertheless, given that between 2014 and 2015 the incentives for LTU in the South basically were unchanged (with a small decrease), one might be concerned that β_7 might actually capture the impact of the generalized subsidy on the STU in the South. This can be seen by reversing the logic followed up to now: STU in the South, relative to LTU living in the same area, experienced an increase in the subsidy equal to 20.274 euro in our simulation reported in the last column of table 1, while the relative increase for STU in the Centre-North was only 10.088 euro. Hence, we might actually be capturing the impact of the new generalized subsidy. The simple DD across areas is helpful to disentangle the two possible interpretations: the results, discussed below, show that there was a relative premium before 2015 for LTU in the South, and this premium disappeared when the old targeted subsidy was abolished.[16]

It is important to stress some other issues related to the interpretation of our results and to which specific population they refer to. Firstly, we focus only on the impact of the policy on eligible individuals, even if they do not actually benefit from the subsidy. Therefore, our parameter of interest can be interpreted as an Intention-To-Treat and not a treatment effect. We believe this is the effect of interest, as the take-up of the

[15] Another way to interpret this *relative* trend assumption is that the difference in trends between areas should have been the same for LTU and STU.

[16] Obviously, this might also imply that the generalized subsidy had an impact on all types of workers, but, as discussed above, we cannot separately identify its effect from other concurrent changes, in particular, the employment protection legislation weakening.

subsidy was left to the decision of firms and workers. Since strict firm's eligibility criteria limited the usability of the subsidy, especially in Centre-North, where its amount was smaller, a fraction of eligible individuals did not benefit from the policy. Then, we may underestimate the size of the effect on treated individuals.

Secondly, our estimates do not recover the impact of law 407/90 subsidy on all the eligible, but rather the effect of the more generous subsidy in part of the country. These results are nevertheless interesting to understand whether these policies might have an impact on the more disadvantaged areas, as it is the case of the Italian South.

Thirdly, as discussed in Section 3, the eligibility criterion is difficult to measure precisely. This induces measurement error in our estimates, because the control group, i.e. the non eligible, also includes individuals that, nevertheless, benefited from the policy (see Section 5.2 for a more elaborate discussion). This measurement error affects also the comparison of trends and not only levels. As we document in table A1 of the Appendix, despite misclassification, there is still a larger fraction of beneficiaries among those that we identify as LTU. Therefore, our estimates should be interpreted as a lower bound.

Finally, the dataset we use allows us to observe only unemployed individuals that have lost a previous job. Those who are searching a first job cannot be observed, as they have not entered any record yet in the administrative system of the *Comunicazioni Obbligatorie*. Our results have, therefore, nothing to say about the impact on the individuals who have never worked before.

In the empirical specification the outcome is an odds ratio. This specification allows to interpret the estimates as the percentage change in the predicted odds ratio due to a unit change in the independent variable regardless of the value of the other variables. From a survival analysis point of view, we treat time in discrete units (weeks), as an approximation of the true daily frequency, and therefore we use a discrete model (Jenkins, 2005). We use a logit model, which is more standard in the survival analysis literature, but the main results carry through by using a linear probability model.[17] Although identification does not require other covariates, as standard in the survival analysis literature we also include the logarithm of non-employment duration (UD_{iw}) as a control (in the Appendix we also show that the results are unaffected if we include a polynomial of UD_{iw} to account for non-linearities). This is important because our sample is unbalanced and therefore in different weeks and areas the average unemployment duration might differ. We also include demographic controls, like gender, nationality, education, age, age squared, controls related to the specific labor market, like dummies for 19 major industries of previous employment and incidence of irregular work in each region by macro-sector cell, and monthly dummies, together with their simple and double interactions with *2015* and *South* dummies. These interaction terms are important because in repeated cross-section studies, as our own, one needs to examine if the samples are selected over time in the same way from comparable populations (Meyer, 1995). Moreover, in all our analysis we compute the cluster-robust standard errors where clusters are made of classes in which individuals don't change their eligibility status. However, all results carry through by clustering at the individual level.

[17] In this case, however, the falsification test on 2013-14 is less neat, which might indicate that the parallel trend assumption holds when it is expressed in terms of proportional odds (the logit model) but not as a difference in hazard rates (the linear probability model).

5.2 Defining eligible and non eligible

The group of individuals which we define as eligible for the subsidy granted by law 407/90 includes those whose non-employment[18] duration is longer than two years. In order to avoid including observations with extremely long non-employment span, we disregard durations longer than 3 years.

Table 2 – Descriptive statistics: means and standard deviations computed over individuals in each category

	Eligible				Non eligible			
	South		Centre-North		South		Centre-North	
	2014	2015	2014	2015	2014	2015	2014	2015
female (%)	41.6	42.5	47.7	48.5	40.5	40.6	48.5	48.8
	[49.3]	[49.4]	[50.0]	[50.0]	[49.1]	[49.1]	[50.0]	[50.0]
highschool	63.8	65.0	65.0	65.9	65.9	66.6	65.8	66.1
dropouts (%)	[48.1]	[47.7]	[47.7]	[47.4]	[47.4]	[47.2]	[47.5]	[47.4]
foreign born	12.2	13.9	32.9	33.8	15.0	15.3	36.1	36.4
(%)	[32.8]	[34.6]	[47.0]	[47.3]	[35.7]	[36.0]	[48.0]	[48.1]
age	39.5	39.9	40.3	40.7	38.5	39.4	39.1	40.0
	[11.3]	[11.4]	[11.3]	[11.3]	[11.4]	[11.3]	[11.3]	[11.1]
agriculture	12.0	13.4	5.6	5.9	12.5	14.6	4.9	5.6
(%)	[32.5]	[34.0]	[23.0]	[23.6]	[33.1]	[35.3]	[21.6]	[22.9]
manufac-	13.1	12.4	15.4	14.7	12.7	11.8	15.4	13.9
turing (%)	[33.7]	[33.0]	[36.1]	[35.4]	[33.3]	[32.2]	[36.1]	[34.6]
construction	16.6	15.2	10.8	10.0	17.1	16.5	10.3	9.7
(%)	[37.2]	[35.9]	[31.1]	[30.3]	[37.7]	[37.1]	[30.4]	[29.7]
services (%)	58.4	59.1	68.2	69.4	57.7	57.1	69.4	70.8
	[49.3]	[49.2]	[46.6]	[46.1]	[49.4]	[49.5]	[46.1]	[45.5]
non-empl.	138.3	139.4	138.6	140.3	61.5	61.4	61.6	61.6
dur. (weeks)	[17.8]	[17.5]	[17.7]	[17.3]	[17.0]	[17.6]	[17.0]	[17.5]
perman. job	0.4	0.5	0.1	0.3	0.4	0.6	0.3	0.4
find. rate (%)	[6.1]	[6.7]	[3.8]	[5.4]	[6.5]	[7.9]	[5.0]	[6.7]
Individuals	20,643	20,844	36,843	38,009	36,246	27,142	66,612	49,407
Obs.	483,097	487,736	887,017	926,931	810,543	554,902	1,517,302	1,055,532

Standard deviations are in square brackets.

The control group should be made of individuals for which we expect a similar time trend in the absence of the policy. At first sight, it would seem reasonable to select individuals with non-employment duration just below the two-year threshold. However, this does not lead to the selection of a good control group for two reasons. First, since the computation of non-employment duration is complex, as we described above, we do not have the true non-employment duration, but only an approximation affected by measurement error. Hence, a sharp cutoff in non-employment duration able to separate the eligible from the non-eligible can lead us to wrongly attribute eligible individuals to the control group and vice-versa. Second, a sharp cutoff has another disadvantage due to strategic behavior by firms, who would prefer to hire individuals just above the threshold, compared to those just below, in order to get the subsidy until it was in place. Therefore, defining as control group individuals just below the threshold would violate the Stable Unit Treatment Value Assumption (SUTVA), because the treatment, i.e. being eligible for the subsidy, would affect also individuals in the control group. For these reasons, we define as control group those individuals with non-employment duration between 6 and 18 months. In the Appendix we also discuss a robustness

[18] We talk about non-employment instead of unemployment because our dataset only allows us to know periods in which individuals are not engaged in any labor contract, but not if they are actively looking for a job.

check restricting the definition of non eligible to a 12-18 month window, in order to select individuals resembling the eligible group more closely. Symmetrically, we restrict also the window defining the treatment group to 24-30 months, instead of 24-36. Results are qualitatively similar and suggest an even stronger effect of the policy.

Our final dataset is made of 174,843 individuals, observed at weekly frequency from January 2014 to December 2015, until they find a job, reach age 65, or exceed the thresholds of 36 or 18 months of non-employment duration, for eligible and non-eligible individuals, respectively. Notice that the same individual can be classified as both eligible and non-eligible at different points in time if, starting as STU and not finding a job, she then is classified as LTU, or, on the contrary, she appears first as LTU, then exits the non-employment status and eventually re-enters as STU. Therefore, we deal with an unbalanced panel of 6.7 million observations. Table 2 provides summary statistics for the subpopulations of interest, which are mainly made of low-skilled prime aged Italian man, who previously worked in the service sector.

5.3 Main results

In what follows we focus on the probability that a non-employed finds a permanent job, conditional on not having found one in the previous six months at least. Table 3 reports results of the (logit) across-areas DD performed on 2014. The dependent variable is a dummy equal to one if the individual finds a permanent job in the subsequent week, zero otherwise. In the first column there are no other controls apart from the dummies needed for the triple difference and non-employment duration (in logarithm). The unit of analysis is individual-week. The first dummy (*LTU*) is equal to one if the individual *i* in week *w* has a non-employment duration between two and three years (being therefore eligible, until 2014, for the hiring subsidy granted by law 407/90), and it is equal to zero if the individual has a non-employment duration between 6 and 18 months. The second dummy (*South*) is equal to one if the individual lives in Southern Italy, zero if she lives in the North or Centre.

Surprisingly, the odds of finding a permanent job in Southern regions is greater than one, even if we control for demographic characteristics; this suggests that non-employed individuals are more likely to find a permanent job in the South than in the Centre-North. This result may reflect geographical differences in job search, which in Southern regions may imply longer non-employment spells.[19] If this is the case, the composition of non-employed individuals may be different in the two areas and in Southern regions individuals with the same non-employment duration may be relatively less detached from the labor market. Another possible explanation may be related to the fact that Southern regions are characterized by higher levels of irregular work: on average 19 per cent of workers in the period 2009-2015 vs. 10 percent in Center-North. This may imply a higher attachment to the labor market for individuals in the South compared to those in the North with the same time span since last regular job loss. In fact, when we control for the regional rate of irregular work, differentiated by sector of previous job, the odds ratio on the *South* dummy becomes statistically non significant.

As expected, the odds ratio on log(duration) is smaller than one, implying that the odds of finding a permanent job decrease the longer the individual has been non-employed. Also, the odds ratio on the LTU

[19] In Southern regions workers tend to look for a job less actively and relying on slower channels. Indeed, according to the Italian labor force survey, in 2014-2015 people out of employment did something to look for a job on average 6 months before the interview in Centre-North and 7 in the South. The share of people who looked for a job on the web was 55 percent in Centre North and 45 in the South. The share of people who turned either to relatives, friends, acquaintances or unions was respectively 69 and 73.

dummy ($\exp(\beta_1)$) is smaller than one, indicating the deterioration rate is more than linear with respect to non-employment duration. In other words, being non-employed for at least two years (LTU) makes it more difficult to find a permanent job. However, this difference was smaller for the LTU living in the South, as the dummy *LTUxSouth* has a positive impact on the chances of finding a permanent job, being its odds ratio larger than one. This suggests that the greater subsidy granted to eligible individuals in the South was effective in rising their chances of finding a job with a permanent contract. The coefficient on the double interaction is hardly affected by the introduction of other control variables (interacted with the *South* dummy).

As already argued, the DD exercise is far from being conclusive, given that the eligible versus non eligible comparison in the South might differ from the one in the Centre-North for reasons other than law 407/90. In table 4 we therefore exploit the change over time in this double comparison, by looking at the triple difference. Overall, in 2015 the odds of finding a permanent job for the individuals in the sample is higher compared to 2014. This captures both the effect of labor market reforms introduced in 2015 (see Sestito and Viviano, 2018) and other changes that occurred over time. The interaction *2015xSouth* shows an odds ratio smaller than one, which implies that the improvement occurred in 2015 was less strong in this area. However, the improvement seems to have been larger for the LTU, as the interaction *LTUx2015* is larger than one. These three coefficients (on year 2015, *2015xSouth* and *LTUx2015*) use the different control groups to capture the underlying trends by area and LTU status.

The triple interaction is therefore the trend for the treated group (the LTU in the South) net of these common trends. The associated odds ratio is smaller than one and statistically significant. This implies that the relative trend for the eligible with respect to non eligible between 2014 and 2015 was worse in the South. This is in line with the dynamic of the stronger subsidy granted by law 407/90 to permanent hires of LTU in the South, which was abolished and replaced by the new (almost) universal subsidy in 2015. As explained above, individuals in treatment and control groups change over time. For the triple difference estimator not to pick up spurious correlations, observable characteristics across these groups should be similar in the pre-treatment and post-treatment periods (and between areas). To check whether compositional changes affect our results, in the second column we add a set of demographic controls, sector of previous job, incidence of irregular work and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies, in order to be sure that the two groups can be considered identical in all observable characteristics. Our coefficient of interest, the triple interaction, is basically unchanged.

Importantly, the odds ratio on the triple interaction reinforces the result from the simple double comparison: it is the change in the double difference across areas and LTU status, essentially the opposite of the within-2014 across-areas diff-in-diff. The relative advantage of eligible individuals living in the South, for which we find evidence in 2014 (as the odds ratio on *LTUxSouth* was larger than one), has disappeared in 2015. Another algebraically equivalent way to see this result is to separate the second DD that composes the DDD exercise. In table 5 we show the DD that compares STU and LTU across areas in 2015: in the presence of similar subsidies for both LTU and STU across areas, the odds ratio on the double interaction is close to 1. The fact that the relative advantage for the LTU in the South disappears when the preferential treatment is removed suggests that our estimates can be attributed to the subsidy we are studying, rather than to other contemporary changes.

Table 3 – Logit model – Odds ratios. – Across areas comparison, year 2014

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6942***	0.7038***
	(0.0251)	(0.0255)
LTU	0.8056***	0.8269***
	(0.0390)	(0.0402)
South	1.6771***	1.1794
	(0.0395)	(0.4194)
LTU x South	**1.5354*** **	**1.5195*** **
	(0.0672)	**(0.0667)**
Constant	0.0105***	0.0005***
	(0.0015)	(0.0001)
Observations	3,697,959	3,697,959
Pseudo R-squared	0.0106	0.0389

Cluster-robust standard errors, where clusters are made of 159,444 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *South*.

Table 4 – Logit model – Odds ratios. – DDD, years 2014-15

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6618***	0.6710***
	(0.0166)	(0.0168)
LTU	0.8443***	0.8665***
	(0.0345)	(0.0356)
2015	1.7587***	1.1229
	(0.0379)	(0.3426)
South	1.6772***	1.1804
	(0.0395)	(0.4196)
LTU x 2015	1.1817***	1.1430***
	(0.0475)	(0.0462)
LTU x South	1.5353***	1.5194***
	(0.0672)	(0.0667)
2015 x South	0.8513***	0.4641
	(0.0273)	(0.2261)
LTU x 2015 x South	**0.6883*** **	**0.7058*** **
	(0.0390)	**(0.0402)**
Constant	0.0126***	0.0006***
	(0.0012)	(0.0002)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0118	0.0382

Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

Table 5 – Logit model – Odds ratios. – Across areas comparison, year 2015

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6359***	0.6448***
	(0.0219)	(0.0222)
LTU	1.0381	1.0303
	(0.0435)	(0.0431)
South	1.4283***	0.5474*
	(0.0321)	(0.1836)
LTU x South	**1.0565**	**1.0720***
	(0.0385)	**(0.0392)**
Constant	0.0258***	0.0015***
	(0.0035)	(0.0004)
Observations	3,025,101	3,025,101
Pseudo R-squared	0.0063	0.0313

Cluster-robust standard errors, where clusters are made of 134,657 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *South*.

Law 407/90 seems, therefore, to have had a positive effect on the chances of accessing a permanent employment. Using eq. (1) we can simulate the counterfactual conditional probability of finding a permanent job for the eligible in 2014 in the South if the policy was not present, i.e.

$$logit[\lambda(y_{iw})|t = 2014, \widehat{g = South}, LTU_{iw} = 1, UD_{iw}] = \hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_3 + \hat{\beta}_5 + \hat{\beta}_7 + \hat{\beta}_{UD} UD_{iw}$$

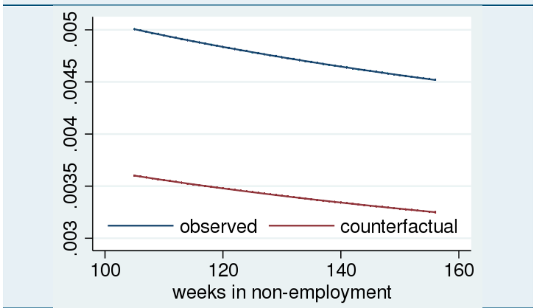
and compare it with the observed one, as estimated by the model, i.e.

$$logit[\lambda(y_{iw})|t = 2014, \widehat{g = South}, LTU_{iw} = 1, UD_{iw}] = \hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_3 + \hat{\beta}_5 + \hat{\beta}_{UD} UD_{iw}.$$

We plot the results in Figure 5. The counterfactual refers to the situation without the targeted subsidy. The effect is non-negligible, since the subsidy was raising the weekly chances of finding a permanent job by 41 per cent. To get an idea of how big the effect is we compare it with that found by Sestito and Viviano (2018) for the generalized hiring subsidy of 2015. The authors find an increase of 100 per cent in the monthly probability of finding a permanent job for those who were not working in the previous period. They show that most part of the effect is due to the hiring subsidy, and only a small part to firing cost reduction granted by the Jobs Act which was passed in the same year. Being of the same order of magnitude, we are reassured about the plausibility of our result.

Figure 5

Counterfactual analysis (1)
(conditional probability)



(1) Conditional probability of finding a permanent job in the subsequent week in 2014 for the eligible in the South, observed (as estimated by the model) and counterfactual (in the absence of the targeted subsidy).

One issue we do not consider directly is by what extent the subsidy might have shaped the entire distribution of non-employment duration, as it might have given an incentive to wait longer in non-employment. However, from our results we can draw some indirect evidence this is not the case, as the likelihood of transitioning to a permanent contract for the non eligible in the South vs. their counterpart in

the Centre-North deteriorated after the removal of law 407/90 (*2015xSouth* in table 4), and it becomes non significant once controls are included, while we would have expected the opposite if the law had given them a strong incentive to wait to reach the LTU status.

The meta-analysis by Card et al. (2015) suggests that active labor market policies are more effective for females. We also analyzed the split sample by gender (tables A7 and A8 in the Appendix), but the odds ratio on the triple interaction is quite similar across the two groups. If nothing, the effect is actually a bit smaller among females.

Our analysis assumes that, absent the change in law 407/90, the double comparison across LTU status and areas would not have changed between 2014 and 2015, so that the odds ratio on the triple interaction would have been one. As an indirect test for the plausibility of this assumption, we run a placebo regression for the years 2013-2014. Reassuringly, we do not find a statistically significant coefficient for the triple interaction term (table 6).

Table 6 – Logit model – Odds ratios – Falsification test: years 2013-2014 (1)

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (2)
Log (duration)	0.6980***	0.7123***
	(0.0179)	(0.0183)
LTU	0.7753***	0.7991***
	(0.0340)	(0.0352)
2014	1.0223	0.3509***
	(0.0227)	(0.1130)
South	1.5822***	0.4525**
	(0.0369)	(0.1567)
LTU x 2014	1.0385	1.0236
	(0.0496)	(0.0491)
LTU x South	1.4540***	1.4201***
	(0.0702)	(0.0688)
2014 x South	1.0621*	2.0851
	(0.0346)	(1.0192)
LTU x 2014 x South	**1.0643**	**1.0823**
	(0.0690)	**(0.0705)**
Constant	0.0100***	0.0006***
	(0.0010)	(0.0002)
Observations	7,335,744	7,335,744
Pseudo R-squared	0.0098	0.0369

Cluster-robust standard errors, where clusters are made of 236,129 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) These regressions identify treated and controls using the same criteria as in tables 3-5. Since years of interest differ, also the sample of individuals may differ. (2) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2014* and *South* dummies.

5.4 Effect on wages

Apart from increasing their chances of obtaining a permanent contract, the subsidy could also potentially rise the entry wage of the beneficiaries. Given that the subsidy consisted of a strong deduction of social security contributions paid by the employer, the effect on wage depends on how much of this gain is shared with the employee. An increase in wage might also be observed if the eligible, knowing that the

subsidy raises their likelihood of finding a job, wait longer before accepting a job offer.

In table 7 we select from our dataset only those individuals for which we have the information on wages along the whole job history and perform a regression of the logarithm of wage on the same regressors used in previous tables. Apart from the negative coefficient for the *South* dummy (when we include all the controls), all other coefficients are not statistically significant. This means that the only effect of the policy for the eligible was to increase their chances of getting a permanent job, while the reduction of hiring costs did not trickle down to the employees. This is reasonable considering that the policy target includes disadvantaged individuals, whose bargaining power is most likely very low.[20]

Table 7 – Ordinary least squares regression

	Dependent variable: Wage in logarithm	
	No controls	With controls (1)
Log (duration)	0.0218	0.0202
	(0.0181)	(0.0171)
LTU	0.0741**	0.0003
	(0.0354)	(0.0341)
2015	0.0876***	0.2377
	(0.0159)	(0.2247)
South	0.1004***	-0.4903**
	(0.0170)	(0.2384)
LTU x 2015	-0.0462	-0.0139
	(0.0367)	(0.0360)
LTU x South	-0.0531	-0.0060
	(0.0381)	(0.0369)
2015 x South	-0.0526**	-0.0218
	(0.0239)	(0.3230)
LTU x 2015 x South	**0.0093**	**0.0055**
	(0.0469)	**(0.0458)**
Constant	6.6215***	6.4569***
	(0.0716)	(0.1899)
Observations	9,791	9,791
Adjusted R-squared	0.010	0.135

Cluster-robust standard errors, where clusters are made of 9,496 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

6. Robustness checks

As common in these kind of studies, our results are affected by possible issues of substitution. Firstly, the subsidy of law 407/90, which applies only to permanent contracts, might have displaced temporary contracts, leading to a null net employment creation. In Subsection 6.1 we look at exit towards these contracts[21].

[20] The effect of the subsidy could have been even negative. Adamopoulou and Viviano (2018) find that the more general subsidy introduced in 2015 had a negative effect on wages.

[21] In the case of interval-censoring, one can prove that it is possible to estimate separate logit models for each possible type of exit from the unemployment status, under some assumptions. In particular, we need to assume that

Secondly, any benefit for LTU creates an incentive to wait longer in non-employment. We partially addressed this issue by excluding individuals just below the 24-month threshold. However, one may want to fully evaluate how the entire non-employment duration distribution changes because of the policy. We are unable to properly perform this full evaluation using our natural experiment. In the previous Section we provided some indirect evidence that our conclusions are not biased by this issue, given that the change in outcomes for non eligible in the South between 2014 and 2015 was worse than for their counterpart in the Centre-North, while we would have expected the opposite if – when the law 407/90 was in place – they had an incentive to wait longer in non-employment. In Subsection 6.1 we provide additional evidence analyzing whether the change of policy affected the take-up of very short-term contracts that did not reset the unemployment duration (according to the legal definition).

Thirdly, employers, learning about the incoming end of the subsidy, might have anticipated at the end of 2014 some contracts that they would have signed in the following year. In Subsection 6.2 we assess whether our results are driven by those contracts signed after the law was announced.

Finally, individuals might move across the country to exploit the difference in the intensity of the subsidy. In subsection 6.3 we run a robustness check excluding those non employed that found a job in a different area with respect to where their last job was located.

6.1 Are results driven by substitution with other types of contract?

The analysis carried out so far considered only the outcome "exit to permanent jobs" as dependent variable, disregarding other possible exits, namely getting another type of contract, reaching retirement age, or being right censored because the non-employment spell exceeds the three-year threshold. In what follows we explore the outcome "exit to fixed-term contracts" in order to see if the positive impact on permanent contracts came at the expenses of temporary ones, reducing the net employment gains. This might also happen if the policy has been raising the reservation wage of the LTU, because they knew the subsidy was increasing their likelihood to receive, at some point in the future, an offer for a permanent job.

We look first at exits from non-employment to fixed-term contracts that last more than 6 months (4/8 months for the South), because, as discussed in Section 3, according to law 407/90 individuals were still considered unemployed if they got shorter term contracts. Table 8 reports the estimated odds ratios. The triple interaction (*LTUx2015xSouth*) does not highlight significant differences for the eligible living in the South. We can interpret this result as evidence that law 407/90 did not imply a simple substitution of fixed-term contracts with permanent jobs, but rather a net employment gain.

Notice also that, when the subsidy was available, both eligible and non eligible had an incentive to avoid taking "long" fixed-term contracts, because such contracts reset unemployment duration, losing possible gains associated with LTU. The fact that the odds ratio on the triple interaction term is close to one suggests this concern is not stronger for the eligible compared to non eligible. Our identification strategy does not provide a full assessment for the non eligible alone. Nevertheless, the interaction *2015xSouth* indicates the differential trend for non eligible in the South after law 407/90 was abolished (compared to non eligible in the North). If individuals at the beginning of their non-employment spell were avoiding fixed-term contracts in order to become eligible and benefit from law 407/90, then we expect them to become relatively more likely to take these contracts in 2015. Without demographic and time controls there is some evidence that this could be the case, but the odds ratio become smaller than one and not significant once

events only happen at the boundaries of the interval, which seems appropriate in our case (as contracts usually start on Monday). See section 9.3 of Jenkins (2005) for further reference.

we include them, thus suggesting that no substitution in favor of these contracts was in place even for the short-term unemployed.

Table 8 – Logit model – Odds ratios – Transitions to “long” fixed-term contracts

Dep. variable: Dummy equal to one if the individual finds a “long” fixed-term job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.7045***	0.7187***
	(0.0160)	(0.0164)
LTU	0.6558***	0.6800***
	(0.0208)	(0.0216)
2015	0.7662***	0.2304***
	(0.0136)	(0.0558)
South	0.6526***	5.5836***
	(0.0133)	(1.6296)
LTU x 2015	1.3125***	1.2901***
	(0.0431)	(0.0423)
LTU x South	0.9080**	0.9027**
	(0.0407)	(0.0405)
2015 x South	1.0763**	0.4938
	(0.0364)	(0.2395)
LTU x 2015 x South	**0.9856**	**0.9816**
	(0.0641)	**(0.0638)**
Constant	0.0245***	0.0574***
	(0.0022)	(0.0096)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0119	0.0311

Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don’t change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

A similar issue concerns “short” fixed-term contracts. As opposed to before, we expect a higher likelihood to use these contracts until law 407/90 was in place, because they did not reset unemployment duration. In table 9 we consider the conditional probability of being employed with such a short-term contract, which technically does not constitute an exit from the “legal” definition of unemployment. The odds ratio on the triple interaction is again not significant, indicating that the eligible in the South did not disproportionately use these contracts. A similar concern, however, applies also to the non eligible in the South. Again, our identification strategy cannot provide clean evidence about this group alone, but it is useful to highlight that the odds ratio on the interaction *2015xSouth* – once we include demographic and time controls – does not indicate a differential trend with respect to the Centre-North after the law was abrogated, providing evidence that no substitution was in place in favor of this contract.

Table 9 – Logit model – Odds ratios – Transitions to “short” fixed-term contracts

Dep. variable: Dummy equal to one if the individual finds a “short” fixed-term job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.7786***	0.7755***
	(0.0140)	(0.0138)
LTU	0.7968***	0.7837***
	(0.0220)	(0.0216)
2015	0.8875***	0.3254***
	(0.0137)	(0.0695)
South	1.3933***	0.4548***
	(0.0220)	(0.0997)
LTU x 2015	1.1426***	1.1602***
	(0.0309)	(0.0316)
LTU x South	1.0080	0.9833
	(0.0315)	(0.0306)
2015 x South	1.0737***	0.9482
	(0.0243)	(0.3054)
LTU x 2015 x South	**0.9815**	**1.0058**
	(0.0391)	**(0.0404)**
Constant	0.0257***	0.0265***
	(0.0018)	(0.0039)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0066	0.0323

Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don’t change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

6.2 Are results driven by substitution over time?

In table 10 we repeat the same analysis of table 4, but with a restricted sample, that excludes the fourth quarter of 2014 and the first quarter of 2015. We do so because our result may be entirely driven by an anticipation effect. Indeed, the Financial Stability Law for 2015 was announced at the end of October 2014, and employers in the South could have anticipated to the last quarter of 2014 those hires they had in program for the beginning of 2015. In so doing, they could have got the larger benefit granted by law 407/90, which would have expired at the end of 2014. Specularly, employers in the Center and North found it more convenient to wait until 2015 in order to benefit from the more generous hiring subsidy granted by the Financial Stability Law for 2015, by postponing to the new year those hires they had in program for the end of 2014. The results are robust to the exclusion of these two periods. Although the odds ratio on the triple interaction (*LTUx2015xSouth*) gets closer to one, meaning a milder effect, the change is relatively small.

Table 10 – Logit model – Odds ratios – Excluding the fourth quarter of 2014 and the first of 2015

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6388***	0.6416***
	(0.0182)	(0.0183)
LTU	0.9070**	0.9236*
	(0.0416)	(0.0425)
2015	1.6610***	1.7528
	(0.0405)	(0.6053)
South	1.6774***	0.9991
	(0.0432)	(0.3825)
LTU x 2015	1.1821***	1.1642***
	(0.0534)	(0.0529)
LTU x South	1.4116***	1.4064***
	(0.0689)	(0.0690)
2015 x South	0.8622***	0.4854
	(0.0314)	(0.2646)
LTU x 2015 x South	**0.7302*** **	**0.7526*** **
	(0.0469)	**(0.0487)**
Constant	0.0151***	0.0005***
	(0.0017)	(0.0002)
Observations	5,105,412	5,105,412
Pseudo R-squared	0.0109	0.0369

Cluster-robust standard errors, where clusters are made of 217,774 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

6.3 Are results driven by substitution across areas?

In table 11 we report a further robustness check, where we exclude from the sample individuals that moved from South to Centre-North or vice versa. We do so in order to check whether our result is influenced by people moving where the most profitable subsidies apply. The results are robust to this sample restriction. Specifically, the odds ratio on the triple interaction term does not change, being statistically significant. This is not surprising since labor mobility is quite low: only about 6,000 individuals were excluded from the sample over a total of 175,000 in the full specification.

Table 11 – Logit model – Odds ratios – Excluding individuals moving across areas

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6687***	0.6759***
	(0.0171)	(0.0173)
LTU	0.8245***	0.8497***
	(0.0345)	(0.0358)
2015	1.7729***	1.1673
	(0.0390)	(0.3636)
South	1.6923***	1.2649
	(0.0408)	(0.4600)
LTU x 2015	1.2065***	1.1641***
	(0.0495)	(0.0480)
LTU x South	1.5821***	1.5555***
	(0.0707)	(0.0698)
2015 x South	0.8456***	0.4386*
	(0.0277)	(0.2179)
LTU x 2015 x South	**0.6675*** **	**0.6854*** **
	(0.0385)	**(0.0398)**
Constant	0.0118***	0.0005***
	(0.0012)	(0.0001)
Observations	6,575,184	6,575,184
Pseudo R-squared	0.0121	0.0385

Cluster-robust standard errors, where clusters are made of 215,997 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

7. Cost-benefit analysis

We finally perform a cost-benefit analysis, where we compare the cost of the subsidy, i.e. the three-year exemption from social security contribution paid by the employer, with the benefits, measured by tax revenue and social security contribution paid by the employee generated by job creation as in Cahuc et al., 2019. We do not consider savings on unemployment benefit since the Italian unemployment insurance scheme does not cover the unemployed for more than two years. Moreover, we are not able to quantify other social costs associated with long-term non-employment, like costs suffered by family and friends and possibly spillovers that encourage more non-employment, as discussed in the introduction. We made this computation just for contracts signed in the South, since our identification makes sense for this subgroup only, which is also the most expensive in terms of cost of the policy.

More specifically, on the cost side we just considered the part of the subsidy exceeding the amount granted to firms in Center-North, since what we estimate is the effect of the *higher* subsidy for firms in the South. While we compute these costs for all subsidized contracts, the benefits are instead computed just for those additional contracts that would have not been signed absent the policy. This fraction is recovered from our estimated change in conditional probability of finding a permanent job: if we use the estimate taken from our main specification in table 4 (i.e. increased probability of finding a permanent job for the eligible individuals in the South equal to 41 percent), the share of contracts signed because of the policy is equal to 41/(100+41), i.e. 29 percent of all eligible contracts. Of course, as outlined in the introduction and justified

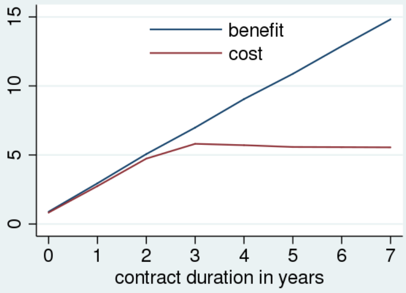
by indirect evidence, our estimates rest on the assumption that no substitution occurs between long- and short-term non employed, types of contracts, geographic areas or time periods.

As we report in table A1 in the Appendix, only 66 percent of eligible long-term non-employed in the South did actually benefit from the subsidy in 2014. Therefore, we divide the share of eligible contracts due to the policy by the fraction of eligible contracts that actually got the subsidy, i.e. 29 divided by 66 equals 44 percent of eligible contracts that got the subsidy and would have not been signed without the policy. As shown in figure 6 we find that benefits outweigh costs even for short contract duration.

Figure 6

Cost-benefit analysis (1)

(thousand euro)



(1) Average cost and benefit of the subsidy by contract duration.

8. Discussion and conclusions

In recent years the Italian labor market has undergone several reforms, the Jobs Act being a last example, which received a lot of attention. A different subsidy, which was abolished at the same time, was instead almost entirely neglected, despite having been in place for 25 years.

By exploiting the timing of the abolition, eligibility criteria, and geographical variation of the subsidy generosity, we perform a policy evaluation exercise which leads us to conclude that, in fact, the policy measure had been effective in promoting the employability of long-term non-employed. In particular, we find that the subsidy granted by law 407/90 was able to counteract the deterioration in employability associated with long-term non-employment. The disadvantage in accessing permanent jobs for long-term non-employed (vs. short-term ones) was smaller in Southern regions, were the subsidy was larger. When, in 2015, the preferential regime granted by law 407/90 was removed, this difference disappeared. This positive effect on permanent employment does not seem to be due to an anticipation effect, substitution across areas, with fixed-term contracts or among jobseekers, and there is no evidence that it led to an increase in wages. Moreover, a cost-benefit analysis shows that revenues from subsidized jobs outweighed their costs.

One issue that we do not discuss in the paper is by what extent the subsidy might shape the entire distribution of the non-employment spells. For instance, the subsidy might give an incentive to wait longer in non-employment – also by repeatedly taking very-short term contracts that were not changing the "legal" status of unemployed. Our results do not seem to indicate that this is the case, as (i) the subsidy does not seem to impact the chances of taking "long" fixed-term contracts, that reset the legal unemployment duration to zero, nor "short" ones, and (ii) the likelihood of transitioning to a permanent contract for the non-eligible in the South vs. their counterpart in the Centre-North deteriorated after the removal of law 407/90, while we would have expected the opposite if the law had given them a strong incentive to wait to reach the LTU status. Nevertheless, our main analysis recovers only the relative impact on LTU vs. STU workers and does not identify the heterogeneous effect of the policy across geographic areas. Further analysis is needed to shed light on this issue, possibly by means of a structural model that considers changes in the entire non-employment duration distribution.

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Appendix

In what follows we describe in more detail how we built the dataset. First, computing non-employment duration was not straightforward, since very short-term contracts do not reset, just suspend, the non-employment duration counter. The time limit necessary to consider a period as “short” changed repeatedly during time and across areas. Therefore, the calculation of unemployment duration had been subject to several changes between 1990 and 2012. Between 2002 and 2012 the suspension was granted for temporary contracts shorter than 8 months (lowered to 4 months for individuals aged 25 or younger, 30 years if college graduate). The rule was simplified to contracts shorter than 6 months by law 92/2012, and slightly modified to include contracts of exactly 6 months by law 76/2013. For some Southern regions the rule remained 4 or 8 months (for younger than 25, 30 if college graduate, or older, respectively) during the whole period, and therefore we prefer it.

Table A1 – Percentage of permanent contracts which received the subsidy by law 407/90 in 2014, by eligibility status defined according to three different rules for computing non-employment duration

	Eligible (1)		**Non-eligible** (1)	
Rules (2)	**South**	**Centre-North**	**South**	**Centre-North**
Baseline	66.31	16.97	21.93	2.31
Simplified	65.92	15.60	23.06	2.42
Income	72.05	16.16	20.87	1.34

(1) We define as eligible those individuals with non-employment duration between two and three years, and as non-eligible those with non-employment duration between 6 and 18 months. (2) The baseline rule defines as short-term contracts, that suspend the non-employment duration counter, those shorter than 6 months in Centre-North and 4/8 months in the South. The simplified rule defines as short-term contracts those shorter than six months everywhere. The income rule uses the same definition as the baseline rule, plus an additional condition on income (the income earned by the worker must not exceed 8.000 euro).

Moreover, since our goal is to identify, as precisely as possible, long-term non-employed who are eligible for the subsidy, we exclude from the sample workers who had self-employment events, as these workers follow other rules concerning the computation of unemployment duration. We also exclude from the definition of permanent job contracts those relative to domestic workers hired by households, those in the agricultural sector, agency workers and work-sharing agreements, as they are not subject to the policy measures.

As we do not know workers’ job history before 2009, we need, for each individual, a starting point in which non-employment duration is equal to zero, in order to avoid the problem of left-censoring. For this reason, we select only workers that, between 2009 and 2013, experienced the termination of a job lasting more than 6 months (4 or 8 months in the South).[1] Starting from the first job loss that satisfies these requirements, we track the individual over the following years. We increase non-employment duration by one week in every following Monday, as long as she does not find a new job.[2] Non-employment duration is kept constant if the individual finds a job lasting less than the time limit described above. If, instead, the contract exceeds these limits, non-employment duration is set back to zero until the individual loses her job again.

As explained in the main text, we define as control group those individuals with non-employment duration between 6 and 18 months. In this way we aim at minimizing the classification error, i.e. the percentage of

[1] The contracts may have started at any point in time before 2009, because the sample includes any contract that was subject to a change (firing and termination included) since 2009, independently from its starting date.

[2] One issue is that individuals may have other part-time jobs that we do not observe because they started before 2009 and they were neither changed nor terminated thereafter. We cannot address this problem with the available data. According to the Italian Labour Force Survey, in 2009-2015 only 0.6 per cent of employees had more than one employment contract.

hires that, according to our calculation of non-employment duration, should not be eligible for the subsidy but were nevertheless subsidized. We can use the information about the actual receipt of the subsidy in 2014 to understand how large this classification error is. In table A1 we focus on permanent contracts and look at the percentage of them which benefited from the subsidy granted by law 407/90. As expected, the fraction is larger in the South. More importantly, it is more than three times higher among those that we define as eligible, although it is still not negligible among those that we assign to the control group. Hence, despite the classification error, the distinction by predicted eligibility status is still informative. A simplified rule, where we consider as short periods of employment those below 6 months for everyone irrespective of geographic area of work, leads to a higher classification error. Using instead a more complex rule – where we also account for the low-income limit – would improve precision, but we would lose a sizeable amount of observations for which the information about wage is not available. Hence, we prefer to focus on the baseline rule in the main text, but we also show that our results are robust to changing the definition (see tables A2-A3).

We also used a more restrictive definition for the non-eligible group, reducing the non-employment duration window to 12-18 months, in order to select individuals resembling the eligible group more closely (symmetrically, we restrict also the window defining the treatment group to 24-30 months, instead of 24-36). In this case the percentage of wrongly attributed hires to the control group is higher. Nevertheless, results (table A2) are qualitatively equal to those reported in table 4. The odds ratio of the triple interaction are even lower, suggesting therefore an even stronger effect of law 407/90.

Finally, in tables A5 and A6 we perform a further robustness check by estimating the same model as in table 4 with the difference that the non-employment duration enters with a more flexible specification: instead of a logarithmic function, we first estimate a third order polynomial function (table A5), then a piecewise constant function (table A6), where we partitioned non-employment duration into ten intervals using deciles of the distribution as cut-points (which happened to fall at week 35, 43, 52, 61, 70, 106, 117, 129, 142), and defined a dummy variable for each of them, assuming the hazard rate to be constant within intervals, and allowing it to differ between them. Our main results are not affected by the choice of the functional form.

Finally, in tables A7 and A8 we report results for the split sample by gender. The effect of the policy seems milder for females.

Table A2 – Logit model – Odds ratios – Income rule

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.7361***	0.7373***
	(0.0367)	(0.0369)
LTU	0.6575***	0.7028***
	(0.0578)	(0.0621)
2015	1.7675***	4.1009**
	(0.0749)	(2.4038)
South	1.5622***	5.1591**
	(0.0704)	(3.3998)
LTU x 2015	1.3875***	1.3285***
	(0.1213)	(0.1173)
LTU x South	2.0137***	1.9277***
	(0.1883)	(0.1814)
2015 x South	0.8648**	0.2444
	(0.0547)	(0.2197)
LTU x 2015 x South	**0.6127*** **	**0.5977*** **
	(0.0716)	**(0.0704)**
Constant	0.0086***	0.0001***
	(0.0017)	(0.0001)
Observations	1,646,053	1,644,079
Pseudo R-squared	0.0113	0.0549

Cluster-robust standard errors, where clusters are made of 54,396 (first column) and 54,353 (second column) classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies. 1974 observations are dropped because, among jobseekers in "International organizations and Public administration" in 2015 in Center-North and "energy and extraction" sectors in 2015 in South and , nobody gets a permanent job.

Table A3 – Logit model – Odds ratios – Simplified rule

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6351***	0.6449***
	(0.0159)	(0.0162)
LTU	0.8461***	0.8710***
	(0.0349)	(0.0361)
2015	1.7727***	1.2661
	(0.0382)	(0.3886)
South	1.5754***	1.3260
	(0.0372)	(0.4758)
LTU x 2015	1.2084***	1.1685***
	(0.0491)	(0.0477)
LTU x South	1.5875***	1.5649***
	(0.0705)	(0.0698)
2015 x South	0.8573***	0.6483
	(0.0274)	(0.3174)
LTU x 2015 x South	**0.6652*** **	**0.6742*** **
	(0.0380)	**(0.0388)**
Constant	0.0150***	0.0007***
	(0.0015)	(0.0002)
Observations	6,725,038	6,725,038
Pseudo R-squared	0.0116	0.0388

Cluster-robust standard errors, where clusters are made of 221,081 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

Table A4 – Logit model – Odds ratios – Restricted non-employment duration intervals

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6132***	0.5968***
	(0.0593)	(0.0579)
LTU	0.8540**	0.8903
	(0.0629)	(0.0657)
2015	1.7258***	1.1881
	(0.0571)	(0.5173)
South	1.5837***	1.0379
	(0.0576)	(0.5197)
LTU x 2015	1.2691***	1.2419***
	(0.0709)	(0.0696)
LTU x South	1.7388***	1.7344***
	(0.1050)	(0.1050)
2015 x South	0.9252	0.3354
	(0.0457)	(0.2323)
LTU x 2015 x South	**0.5994*** **	**0.6007*** **
	(0.0472)	**(0.0474)**
Constant	0.0178***	0.0011***
	(0.0072)	(0.0006)
Observations	3,385,610	3,385,610
Pseudo R-squared	0.0107	0.0350

Cluster-robust standard errors, where clusters are made of 176,319 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

Table A5 – Functional form for characterizing duration dependence: third order polynomial

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Unemployment duration	0.9906*	0.9924
	(0.0055)	(0.0055)
Unemployment duration squared	1.0000	1.0000
	(0.0001)	(0.0001)
Unemployment duration cubed	1.0000	1.0000
	(0.0000)	(0.0000)
LTU	0.9916	1.0272
	(0.0769)	(0.0798)
2015	1.7594***	1.1219
	(0.0380)	(0.3423)
South	1.6772***	1.1826
	(0.0395)	(0.4204)
LTU x 2015	1.1854***	1.1463***
	(0.0477)	(0.0463)
LTU x South	1.5357***	1.5196***
	(0.0671)	(0.0666)
2015 x South	0.8513***	0.4642
	(0.0273)	(0.2261)
LTU x 2015 x South	**0.6875*** **	**0.7046*** **
	(0.0389)	**(0.0401)**
Constant	0.0039***	0.0002***
	(0.0005)	(0.0001)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0119	0.0383

Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

Table A6 – Functional form for characterizing duration dependence: piecewise constant function

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Unemployment duration 1	1.8120***	1.7741***
	(0.1121)	(0.1100)
Unemployment duration 2	1.7493***	1.7046***
	(0.1092)	(0.1065)
Unemployment duration 3	1.5107***	1.4872***
	(0.0945)	(0.0931)
Unemployment duration 4	1.4759***	1.4689***
	(0.0925)	(0.0922)
Unemployment duration 5	1.3160***	1.2938***
	(0.0831)	(0.0818)
Unemployment duration 6	1.3700***	1.3479***
	(0.0753)	(0.0742)
Unemployment duration 7	1.3380***	1.3235***
	(0.0434)	(0.0430)
Unemployment duration 8	1.1296***	1.1164***
	(0.0379)	(0.0375)
Unemployment duration 9	1.0640*	1.0631*
	(0.0361)	(0.0361)
LTU	0.7647***	0.7873***
	(0.0470)	(0.0485)
2015	1.7582***	1.1190
	(0.0379)	(0.3414)
South	1.6771***	1.1818
	(0.0395)	(0.4201)
LTU x 2015	1.1869***	1.1473***
	(0.0477)	(0.0464)
LTU x South	1.5359***	1.5197***
	(0.0671)	(0.0666)
2015 x South	0.8513***	0.4635
	(0.0273)	(0.2257)
LTU x 2015 x South	**0.6873*** **	**0.7046*** **
	(0.0389)	**(0.0401)**
Constant	0.0016***	0.0001***
	(0.0001)	(0.0000)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0120	0.0384

Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. The dummy "Unemployment duration 10" has been excluded to avoid perfect collinearity. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

Table A7 – Logit model – Odds ratios – Male sub-population.

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.6176***	0.6339***
	(0.0188)	(0.0193)
LTU	0.8444***	0.8784**
	(0.0427)	(0.0446)
2015	1.6889***	1.1157
	(0.0452)	(0.4365)
South	1.5131***	0.8762
	(0.0432)	(0.3998)
LTU x 2015	1.1899***	1.1471***
	(0.0600)	(0.0582)
LTU x South	1.6405***	1.5913***
	(0.0881)	(0.0857)
2015 x South	0.9029***	0.4273
	(0.0353)	(0.2714)
LTU x 2015 x South	**0.6684*** **	**0.6866*** **
	(0.0466)	**(0.0481)**
Constant	0.0216***	0.0010***
	(0.0026)	(0.0003)
Observations	3,609,022	3,609,022
Pseudo R-squared	0.0119	0.0318

Cluster-robust standard errors, where clusters are made of 119,277 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies.

Table A8 – Logit model – Odds ratios – Female sub-population.

Dependent variable: Dummy equal to one if the individual finds a permanent job in the subsequent week		
	No controls	With controls (1)
Log (duration)	0.7678***	0.7536***
	(0.0337)	(0.0332)
LTU	0.8150***	0.8399**
	(0.0568)	(0.0587)
2015	1.9151***	1.0027
	(0.0704)	(0.5247)
South	1.8337***	1.0611
	(0.0766)	(0.6764)
LTU x 2015	1.1567**	1.1223*
	(0.0773)	(0.0752)
LTU x South	1.3993***	1.3833***
	(0.1059)	(0.1049)
2015 x South	0.7532***	0.9575
	(0.0429)	(0.8263)
LTU x 2015 x South	**0.7445*** **	**0.7572*** **
	(0.0727)	**(0.0744)**
Constant	0.0048***	0.0002***
	(0.0008)	(0.0001)
Observations	3,114,038	3,113,168
Pseudo R-squared	0.0110	0.0416

Cluster-robust standard errors, where clusters are made of 101,899 (first column) and 101,876 (second column) classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for nationality, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *2015* and *South* dummies. 870 observations are dropped because, among jobseekers in "energy and extraction" sector in 2014 in Center-North, nobody gets a permanent job.