LONG-TERM UNEMPLOYMENT AND SUBSIDIES FOR PERMANENT EMPLOYMENT[1]

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Abstract

Many developed countries experienced a steep rise in long-term unemployment (LTU) following the Great Recession. As the probability to find a job decreases with time spent in unemployment, several countries have supplemented training schemes with employment subsidies for the unemployed. The evidence about the impact of these subsidies is mixed, because substitution effects among workers and over time may undermine their effectiveness. We provide new results by investigating the efficacy of a generous subsidy intended to encourage firms to hire individuals in LTU. This program was in force until the end of 2014 in Italy, one of the European countries with the highest long-term unemployment rate. Differently from other policies that target LTU, 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 LTU and not for the short-term unemployed; (ii) the subsidy was significantly more generous in the South of Italy; (iii) the program was in place until 2014. We find that, compared to other workers, long-term unemployed individuals in Southern regions experienced a drop in their relative probability of finding a permanent job after the abrogation of the program. We provide further evidence suggesting that the effect is not driven by substitution over time, across contracts or among workers.

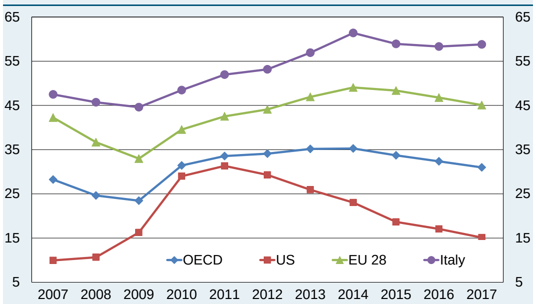
Key words: long-term unemployment, triple difference estimator, employment subsidies, policy evaluation, regional disparities. **JEL Codes**: H25, J08, J64, R23

1. Introduction

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).[3] In 2017, even after the rate fell by almost two percentage points from its peak, more than 8 million individuals living in the European Union were long-term unemployed, almost a half of the total number of unemployed workers. Even in the US one million individuals were still in this condition.

Figure 1

Incidence of long-term unemployment (1)
(percentage)



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

The reason why LTU is a policy concern is twofold. Firstly, unemployment is one of the most significant causes of households' poverty. The probability of finding 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,

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[2] Banca d'Italia. The views in this paper are those of the authors and do not necessarily reflect those of Banca d'Italia.
[3] In the European Union the LTU rate reached 5.1 percent in 2013, almost doubling compared to 2008. The LTU rate is defined as the share of active individuals aged 15-74 who are out of work and have been actively seeking employment for at least one year over the labour force (source: Eurostat).

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).

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. (2015) 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).[4] 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 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).

This paper aims at studying the effects of a subsidy introduced in Italy in 1990 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.[5] Its amount was greater for firms in Southern Italy (100 percent of social contributions for 3 years; only 50 percent for firms in other Italian regions). This differential treatment in favour of Southern regions and long-term unemployed was abrogated in 2015 by the Financial Stability Law, which introduced exemption from social contributions without distinctions between areas and duration of unemployment (although individuals with a permanent contract in the previous six months were excluded).[6]

This is an interesting case study for a number of reasons. First, Italy is one of the few European countries where LTU rate was already 3 percent even in 2007 and, among these countries, it is the only one where the rate sharply increased during the recession (almost 4 percentage points). Second, these figures are even more striking for the South, where LTU rate has steadily doubled the national one, staying at 12 percent still in 2017. Third, participation rate in Italy is particularly low, reflecting structural determinants of the Italian labour market; thus, the long-term unemployed in Italy may be more at risk of leaving the labour force. Furthermore, 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 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. Finally, Italy's public expenditure for

[4] 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).

[5] *Cassa integrazione guadagni* (CIG).

[6] On the effects of the 2015 hiring subsidy look at Sestito and Viviano (2018).

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 (the corresponding figure for the OECD countries being 14 per cent). In 2014 the program involved 260,000 hires in the South, 37,000 in Center-North. The effectiveness of this policy is nevertheless an empirical question.

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, we use a triple difference estimator (DDD) and exploit variation in the relative cost of hiring with a permanent contract across time, regions and worker's unemployment length. We implicitly assume that, without any targeted subsidy, the choice to hire with a permanent contract a long-term unemployed rather than a short-term one would have changed in the same way in Northern and Southern regions.

The choice of using a DDD approach, instead of a simple Difference-in-differences (DD), is motivated by several factors. First, it comes from how the subsidy had been designed in the first place: law 407/90 granted a preferential treatment to long-term unemployed 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 (long- vs. short-term unemployment and South vs. North) evolved over the third dimension (time). We are aware of few studies where the natural experiment is by design so close to a DDD (see Clotfelter et al., 2008, for an exception).

Second, the alternative approach would be to rely on simple DDs. One possibility, for instance, would be to compare long- vs. short-term unemployed when the policy was in place and after its abrogation. This strategy would rely on a strong assumption, namely that the difference between short and long term unemployed would be the same in the two areas in the absence of the policy. Alternatively, one could compare long-term unemployed in the South with those in the North before and after the law was abrogated. Also this strategy relies on a strong assumption, that changes brought by other interventions on the job market between 2014 and 2015 influenced the two areas in the same way. On the contrary, the DDD estimator is based on a weaker assumption: it requires that no contemporaneous shock affects the *relative* outcomes of the treatment group (the eligibles) compared to the control group (the non eligibles) in the same area and year as the treatment (Gruber, 1994). Intuitively, the DDD approach exploits these three dimensions to remove (i) underlying differences between long- and short-term unemployed; (ii) area-specific time trends and (iii) differential time-trends for the long-term unemployed. Despite these advantages, our strategy is still 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 LTU status) and across areas (where individuals move to exploit the preferential treatment). We propose a series of robustness checks to assess whether our results are 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 being hired with a permanent contract. 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, long-term unemployed individuals in Southern Italy experienced a fall in their probability of finding a permanent job relative to short-term unemployed. 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 45 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 contracts or categories of jobseekers.

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 LTU 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. Section 7 concludes.

2. The subsidy granted by law 407/90

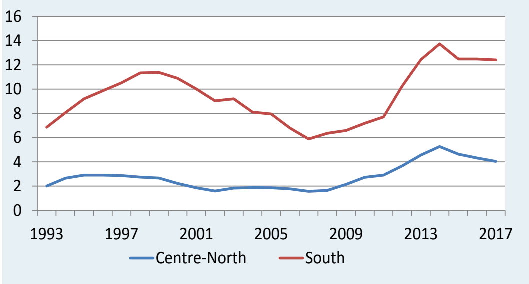
Until 2014, permanent hires of individuals unemployed or covered by the national short-time work compensation scheme for at least 24 months benefited from a subsidy granted by law/90. The subsidy was equal to 100 per cent of social security contributions for three years in the South, 50 per cent in the rest of Italy. The favorable treatment of Southern regions aimed at increasing LTU employability especially where this was more needed (in 1993[7] the incidence of LTU over the labour force was 6.9 per cent in the South against 2.0 in other Italian regions; see figure 2).

Only firms that had not experienced any firings, workers' suspending, 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.[8]

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.

Figure 2

LTU in Italian regions (1)
(percentage)



Source: Istat, Italian labour force survey.
(1) Unemployed individuals from more than one year as percentage of total unemployment.

[7] First date available in the Italian labour force survey.
[8] Source: INPS, *Statistiche in breve*, *Politiche Occupazionali e del Lavoro*.

Because of the abolition of law 407/90, the cost of hiring with a permanent contract an individual unemployed for at least 24 months increased both in Northern and Southern regions. On the contrary, 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 on, and a new insurance scheme against the risk of unemployment covering a broader set of workers (*Nuova Assicurazione Sociale per l'Impiego*). Since these policies were enforced in the same way both across geographic location and along the distribution of unemployment duration, we believe that our identifying assumption still holds.

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	**2014 targeted subsidies** (2)	**2015 generalized subsidies** (3)
South	26.576	24.180
Centre-North	13.966	24.180

(1) We refer to a gross yearly wage of 26,000 euro, a social security contribution rate for the employer equal to 32 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 – see 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.

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[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

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] We follow each worker during her entire job history until age 64. We focus only on the effect of the measure on unemployed jobseekers, and not also on beneficiaries of the short-time work compensation scheme, since we are not able to identify this latter group.

Even if our empirical analysis focuses on years 2014-2015, we use workers' job history before 2009 in order to determine, for each individual, a starting point of her unemployment spell (see Appendix). Indeed, for our purposes, we need to identify the long-term unemployed who are eligible for the subsidy as precisely as possible. 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 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 the country.

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 LTU granted by law 407/90, which were more generous.

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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 wage.

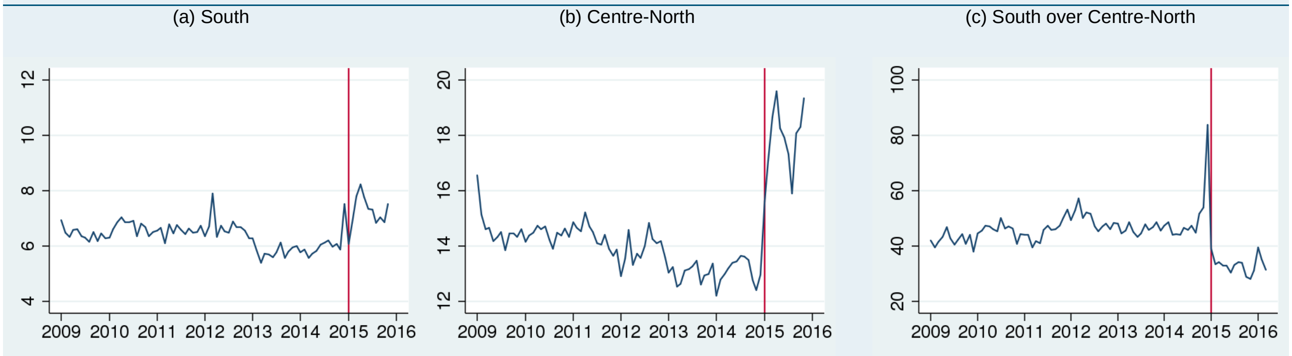
[11] Setting up a panel at daily frequency would lead to a hardly-manageable large dataset, without bringing significant gains. In fact, we are interested in identifying transitions from unemployment to permanent employment in 2014 and 2015. Therefore, observations at weekly frequency are a sufficiently 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.

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 eligibles in the former area, we show two additional pieces of evidence. Figure 4a removes from the time-series those contracts that actually received the incentive 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 unemployed individuals and splits them according to their unemployment duration. Before 2015, relative hires of the long-term unemployed in the South (with respect to the rest of the country) were higher than those of short-term unemployed. This difference disappeared in 2015.

Figure 3

Permanent hires (1)

(monthly data; thousands and percentages)



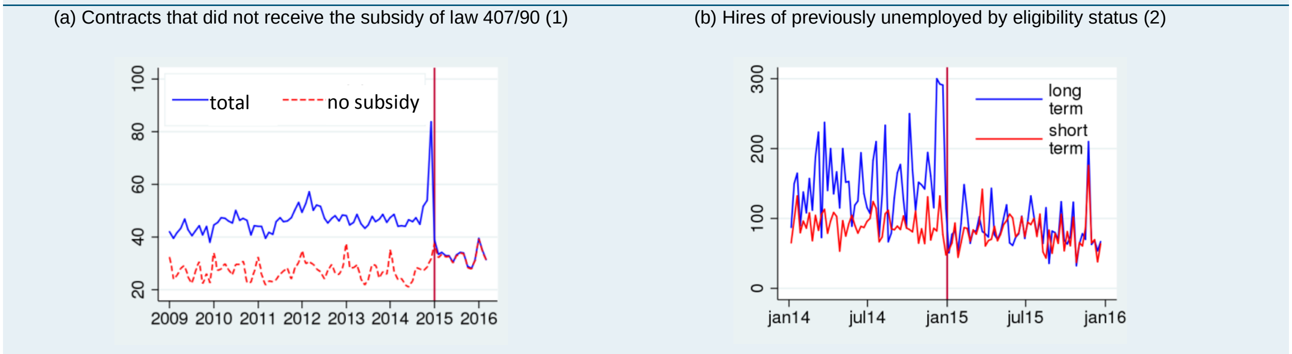
Source: our elaboration on Ministry of Labor data, Campione Integrato delle Comunicazioni Obbligatorie (CICO).

(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)



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.

5. Micro analysis

5.1 Estimation strategy

As shown by the aggregate trends, law 407/90 seems to have had a positive impact on the permanent hires of long-term unemployed 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:

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

The outcome of interest is a dummy y_{iw} equal to 0 if individual *i=1,…,N* in week *w=1,…,W* does not find a permanent job in the subsequent week, and 1 if she finds it. Defining $\lambda(y_{iw}) = Pr(y_{iw} = 1)$, our main logit specification is the following:

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

where $t \in \{2014, 2015\}$ is the year, $g \in \{Centre - North, South\}$ the area, LTU_{iw} is a dummy equal to 1 if unemployment duration is longer than 2 years, and UD_{iw} is the logarithm of unemployment duration. The reference group is made of short-term unemployed in Centre-North regions in year 2014.

This specification allows us to look at several comparisons of interest:

- $\exp(\beta_1)$ is the odds ratio for the long- vs. short-term unemployed in the Centre-North in 2014. This value combines a negative factor coming from the fact that long-term unemployed generally have lower probabilities of finding a new job (due to deteriorating human capital and selection) and a positive component due to the presence of the subsidy we are considering;
- $\exp(\beta_1 + \beta_3 + \beta_5)$ is the same odds ratio of long- vs. short-term for the South, which we expect to be larger because of the presence of higher law 407/90 subsidy.

Notice that $\exp(\beta_5)$ is the double comparison we would have looked at if we had chosen the first simple DD strategy outlined above. But to give it a causal interpretation we should have assumed no other unobservable differences besides the subsidy, in the relative outcome of long- vs. short-term unemployed

[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, but results are unchanged, as we report in the Appendix (Tables A5 and A6).

in the two areas. We instead assume that, in the absence of law 407/90, the difference in time trend (and not in level) by duration of unemployment (LTU) would have been the same in the two areas:

$$\{logit[\lambda(y_{iw})|t = 2015, g = South, LTU_{iw} = 1, UD_{iw}] - logit[\lambda(y_{iw})|t = 2014, g = South, LTU_{iw} = 1, UD_{iw}]\}$$

$$-\{logit[\lambda(y_{iw})|t = 2015, g = South, LTU_{iw} = 0, UD_{iw}] - logit[\lambda(y_{iw})|t = 2014, g = South, LTU_{iw} = 0, UD_{iw}]\} =$$

$$\{logit[\lambda(y_{iw})|t = 2015, g = CN, LTU_{iw} = 1, UD_{iw}] - logit[\lambda(y_{iw})|t = 2014, g = CN, LTU_{iw} = 1, UD_{iw}]\}$$

$$-\{logit[\lambda(y_{iw})|t = 2015, g = CN, LTU_{iw} = 0, UD_{iw}] - logit[\lambda(y_{iw})|t = 2014, g = CN, LTU_{iw} = 0, UD_{iw}]\} \quad (2)$$

This assumption of common *relative* trends essentially states that, in the absence of a preferential treatment for the South, the trend in relative performance of long- vs. short-term unemployed would have been the same in the two areas. Another way to interpret this *relative* trend assumption is that the difference in trends between areas should have been the same for short- and long-term unemployed.

This assumption justifies the use of a triple-difference estimator. Indeed, the difference between the left- and right-hand side of equation (2) is captured by the coefficient on the triple-difference, β_7. Hence, under assumption (2) β_7 would be different from zero 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.

It is important to stress some 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 (the intention-to-treat). We believe this is the impact of interest, as the take-up of the 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 in LTU 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 generalized impact of law 407/90 subsidy on the long-term unemployed, 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, we are unable to fully reconstruct the formal rules to identify the long-term unemployed. This induces measurement error in our estimates, because the control group, i.e. the short-term unemployed, 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 eligible individuals among those that we identify as in LTU, our estimates should be interpreted as a lower bound.

Finally, the dataset that 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 yet entered any record in the administrative system of the "Comunicazioni Obbligatorie". Our results have, therefore, nothing to say about the impact on the long-term unemployed who have never worked before.

As common in these kind of studies, our results are affected by possible issues of substitution. Firstly, employers, learning about the incoming end of subsidies, might have anticipated at the end of 2014 some contracts that they would have signed in the following year. We assess whether our results are driven by those contracts signed when the law was finally approved. Secondly, any benefit for LTU creates an incentive to wait longer in unemployment. We partially address this issue by excluding individuals around the 24 months threshold. However, one may want to fully evaluate how the entire unemployment duration distribution changes because of the policy. We are unable to properly perform this full evaluation using our natural experiment, although in Section 6 we provide some indirect evidence that our conclusions are not biased by this issue, both by looking at the change in outcomes for short-term unemployed in the South between 2014 and 2015 and by analyzing the choice of taking up very short-term contracts that did not reset the unemployment duration (according to the legal definition). Thirdly, 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 Section 6 we also look at other types of exit towards these contracts[13]. Finally, individuals might move across the country to exploit the difference in the intensity of the subsidy. We run a robustness check excluding those unemployed workers that found a job in a different area with respect to where their last job was located.

In the main analysis the outcome is an odds ratio. 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.[14]

5.2 Defining eligibles and non-eligibles

The group of individuals eligible for the subsidy granted by law 407/90 includes those whose unemployment duration is longer than two years. In order to avoid including observations with extremely long unemployment span, we disregard durations longer than 3 years.

On the other hand, 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 unemployment 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 unemployment duration is complex, as we described above, we do not have the true unemployment duration, but only an approximation affected by measurement error. Hence, a sharp cutoff in unemployment duration able to separate the eligibles from the non-eligibles 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 unemployment duration between 6 and 18 months.

[13] 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 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.

[14] 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).

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

	Long-term unemployed				Short-term unemployed			
	South		Centre-North		South		Centre-North	
	2014	2015	2014	2015	2014	2015	2014	2015
female (%)	41.59	42.48	47.70	48.54	40.50	40.59	48.47	48.84
	[49.29]	[49.43]	[49.95]	[49.98]	[49.09]	[49.11]	[49.98]	[49.99]
highschool	63.81	64.95	65.00	65.85	65.87	66.59	65.76	66.07
dropouts (%)	[48.06]	[47.71]	[47.70]	[47.42]	[47.42]	[47.17]	[47.45]	[47.35]
age	39.50	39.93	40.30	40.73	38.52	39.42	39.07	39.99
	[11.32]	[11.35]	[11.26]	[11.26]	[11.42]	[11.32]	[11.29]	[11.14]
unempl.	138.29	139.41	138.60	140.31	61.47	61.40	61.62	61.58
dur. (weeks)	[17.80]	[17.53]	[17.74]	[17.33]	[17.03]	[17.63]	[17.01]	[17.47]
perman. job	0.37	0.45	0.14	0.30	0.42	0.63	0.25	0.44
find. rate (%)	[6.06]	[6.68]	[3.79]	[5.44]	[6.50]	[7.91]	[5.03]	[6.65]
Individuals	20,648	20,848	36,839	38,005	36,252	27,151	66,605	49,398
Observations	483,250	487,867	886,864	926,800	810,713	555,111	1,517,132	1,055,323

Standard deviations are in square brackets.

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 unemployment 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 a short-term unemployed and not finding a job, she then is classified as long-term unemployed, or, on the contrary, she appears first as long-term unemployed, then exits the unemployment status and eventually re-enters as short-term unemployed. Therefore, we deal with an unbalanced panel of 6.7 million observations. Table 2 provides summary statistics for the subpopulations of interest.

5.3 Results

In what follows we focus on the probability that an unemployed jobseeker finds a permanent job. Table 3 reports results of the baseline regression for a logit model. 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 unemployment duration (in logarithm). The first dummy (Long-Term Unemployed – *LTU*) is equal to one if the individual has an unemployment 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 an unemployment duration between 6 and 18 months. The second dummy (*2015*) is equal to one if the individual is observed in 2015, zero if she is observed in 2014. The third dummy (*South*) is equal to one if the individual lives in Southern Italy, zero if she lives in the North or Centre. Then, we report the odds ratios for the three double interactions and, finally, the triple interaction.

Once both the area-specific economic cycle and the unemployment duration are taken into account[16], the odds of finding a permanent job in Southern region is greater than one; this suggests that unemployed 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 unemployment

[16] Among the controls we include also monthly dummies and their interactions with the *South* dummy.

spells.[17] If this is the case, the composition of unemployed individuals may be different in the two areas and in Southern regions individuals with the same unemployment duration may be relatively less detached from the labour market.

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 unemployed. Also, the odds ratio on the LTU dummy ($\exp(\beta_1)$) is smaller than one, indicating the deterioration rate is more than linear with respect to unemployment duration. In other words, being a long-term unemployed makes it more difficult to find a permanent job.

However, this difference was smaller for the long-term unemployed 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 long-term unemployed jobseekers in the South was effective in rising their chances of finding a job with a permanent contract. In fact, the coefficient on the interaction is the one we would have recovered by using a simple DD comparing long- versus short-term unemployed across areas when the policy was in place. Nevertheless, as already argued, this comparison is far from being conclusive, given that the short- versus long-term unemployed comparison in the South might differ from the one in the Centre-North for reasons other than law 407/90.

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 long-term with respect to the short-term unemployed between 2014 and 2015 was worse in the South. This reinforces the result from the simple double comparison. The relative advantage of eligibles individuals living in the South, for which we find evidence in 2014 (as the odds ratio on *LTUxSouth* was larger than one), seems to have disappeared in 2015 (as the combined odds ratio of *LTUxSouth* and *LTUx2015xSouth* is 1.53x0.69=1.06). This is in line with the dynamic of the stronger subsidy granted by law 407/90 to permanent hires of eligibles in the South, which was abolished and replaced by the new (almost) universal subsidy in 2015.

Law 407/1990 seems, therefore, to have had a positive effect on the chances of accessing a permanent employment. Using eq. (1) we can simulate the counterfactual probability of finding a permanent job for the long-term unemployed in 2014 in the South if the policy was not present, i.e.

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

[17] In Souther regions workers tend to look for a job less actively and relying on slower channels. Indeed, according to the Italian Labour 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 were 55% in Centre North and 45 in the South. The shares of people who turned either to relatives, friends, acquaintances or unions were respectively 69 and 73.

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 law 407/90. The effect is non-negligible, since the subsidy was raising the weekly chances of finding a permanent job by 45 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.

Table 3 – Logit model – Odds ratios.

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***	0.6606***
	(0.0166)	(0.0165)
LTU	0.8450***	0.8603***
	(0.0346)	(0.0353)
2015	1.7602***	3.0814***
	(0.0380)	(0.8464)
South	1.6794***	3.2703***
	(0.0395)	(0.9884)
LTU x 2015	1.1807***	1.1704***
	(0.0475)	(0.0472)
LTU x South	1.5326***	1.5190***
	(0.0671)	(0.0666)
2015 x South	0.8497***	0.2325***
	(0.0273)	(0.0944)
LTU x 2015 x South	**0.6897*** **	**0.6929*** **
	(0.0391)	**(0.0393)**
Constant	0.0126***	0.0032***
	(0.0012)	(0.0007)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0118	0.0229

Robust standard errors, clustered by 174,843 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.

One issue we do not consider directly is by what extent the subsidy might have shaped the entire distribution of unemployment duration, as it might have given an incentive to wait longer in unemployment. However, from our results we can draw some evidence this is not the case, as the likelihood of transitioning to a permanent contract for the short-term unemployed in the South vs. their counterpart in the Centre-North deteriorated after the removal of law 407/90 (*2015xSouth* in Table 3), while we would have expected the opposite if the law had given them a strong incentive to wait to reach the LTU status.

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 of Table 3 we add a set of controls (gender, education, age, age squared) and monthly dummies, together with their double and triple 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.

Figure 5 – Probability of finding a permanent job in the subsequent week in 2014 for the long-term unemployed in the South, observed (as estimated by the model) and counterfactual (in the absence of law 407/90)

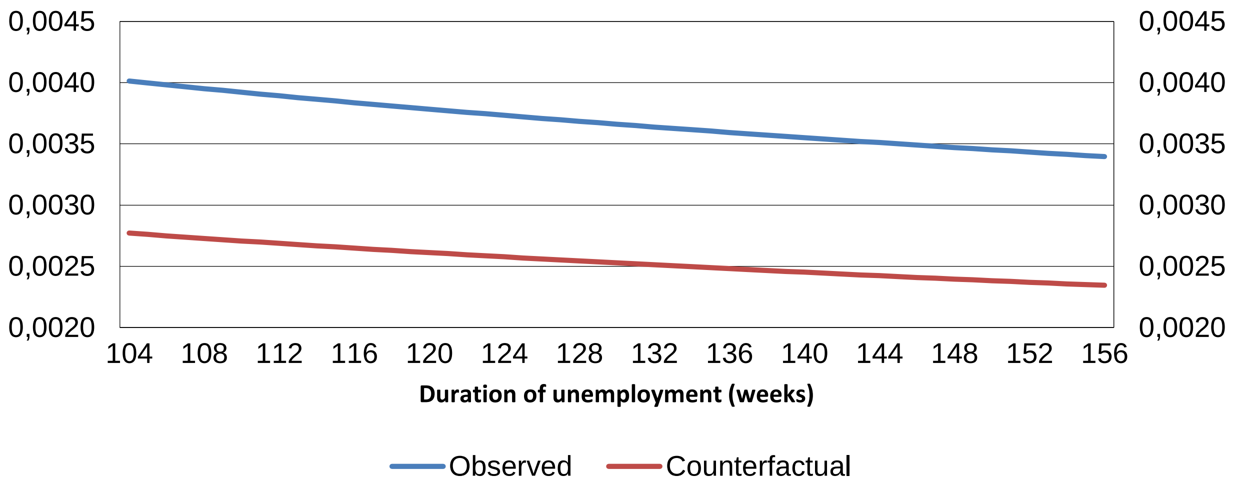


Table 4 – Logit model – Odds ratios – Falsification test: years 2013-2014 (1)

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***	0.6998***
	(0.0179)	(0.0179)
LTU	0.7754***	0.7822***
	(0.0340)	(0.0343)
2014	1.0216	0.4340***
	(0.0227)	(0.1258)
South	1.5819***	1.1003
	(0.0369)	(0.3299)
LTU x 2014	1.0393	1.0407
	(0.0496)	(0.0498)
LTU x South	1.4537***	1.4406***
	(0.0702)	(0.0696)
2014 x South	1.0637*	2.5285**
	(0.0346)	(1.0496)
LTU x 2014 x South	**1.0626**	**1.0664**
	(0.0689)	**(0.0692)**
Constant	0.0100***	0.6407
	(0.0010)	(0.0187)
Observations	7,335,744	7,335,744
Pseudo R-squared	0.0098	0.0214

Robust standard errors, clustered by 189,664 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) These regressions identify treated and controls using the same criteria in table 5 (see Appendix). Since years of interest differ, also the sample of individuals may differ. (2) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2014* and *South* dummies.

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 triple difference coefficient would have been zero. 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 4).

6. Robustness checks and additional outcomes

6.1 Are results driven by substitution over time?

In Table 5 we repeat the same analysis of table 3, 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.

Table 5 – 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***	0.6294***
	(0.0182)	(0.0179)
LTU	0.9077**	0.9201*
	(0.0417)	(0.0423)
2015	1.6622***	4.6767***
	(0.0406)	(1.4468)
South	1.6791***	2.8071***
	(0.0432)	(0.9010)
LTU x 2015	1.1812***	1.1904***
	(0.0534)	(0.0540)
LTU x South	1.4094***	1.4082***
	(0.0688)	(0.0689)
2015 x South	0.8608***	0.2636***
	(0.0313)	(0.1178)
LTU x 2015 x South	**0.7315*** **	**0.7322*** **
	(0.0470)	**(0.0472)**
Constant	0.0150***	0.0030***
	(0.0017)	(0.0007)
Observations	5,105,412	5,105,412
Pseudo R-squared	0.0109	0.0219

Robust standard errors, clustered by 172,877 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.

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 both the odds ratio on the double interaction (*LTUxSouth*) and the one on the triple interaction (*LTUx2015xSouth*) get closer to one, meaning milder effects, the changes are relatively small.

6.2 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 unemployment 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. We look first at exits from the unemployment status 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 6 reports the estimated odds ratios. The triple interaction (*LTUx2015xSouth*) does not highlight significant differences for long-term unemployed 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.

Table 6 – 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***	0.7054***
	(0.0160)	(0.0160)
LTU	0.6558***	0.6742***
	(0.0208)	(0.0214)
2015	0.7661***	0.1832***
	(0.0136)	(0.0400)
South	0.6524***	4.3519***
	(0.0133)	(1.1223)
LTU x 2015	1.3126***	1.2961***
	(0.0430)	(0.0423)
LTU x South	0.9079**	0.9121**
	(0.0407)	(0.0408)
2015 x South	1.0768**	0.6068
	(0.0365)	(0.2624)
LTU x 2015 x South	**0.9852**	**0.9840**
	(0.0641)	**(0.0637)**
Constant	0.0245***	0.0335***
	(0.0022)	(0.0050)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0119	0.0217

Robust standard errors, clustered by 174,843 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.

Notice also that, when the subsidy was available, both long- and short-term unemployed 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 that this concern is not stronger for the long-term compared to short-term unemployed. Our identification strategy does not provide a full assessment for the short-term unemployed alone. Nevertheless, the interaction *2015xSouth* indicates the differential trend for short-term unemployed in the South after law 407/90 was abolished (compared to short-term unemployed in the North). If individuals at the beginning of their unemployment spell were avoiding fixed-term contracts in order to become long-term unemployed 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 we include them, thus suggesting that no substitution in favor of these contracts was in place even for the short-term unemployed.

Table 7 – 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)	0.7787***	0.7808***
	(0.0138)	(0.0139)
LTU	0.7968***	0.8013***
	(0.0218)	(0.0220)
2015	0.8873***	0.2945***
	(0.0137)	(0.0557)
South	1.3931***	0.6199**
	(0.0220)	(0.1191)
LTU x 2015	1.1424***	1.1370***
	(0.0316)	(0.0315)
LTU x South	1.0081	0.9960
	(0.0314)	(0.0312)
2015 x South	1.0741***	1.1905
	(0.0243)	(0.3303)
LTU x 2015 x South	**0.9820**	**0.9830**
	(0.0399)	**(0.0401)**
Constant	0.0257***	0.0110***
	(0.0018)	(0.0015)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0066	0.0136

Robust standard errors, clustered by 174,843 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple 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 7 we consider the probability of being employed with such a short-term contract, which technically does not constitute an exit from the “legal” definition of unemployment. The triple difference is again not significant, indicating that the long-term unemployed in the South didn’t disproportionately use these contracts. A similar concern, however, applies also to the short-term unemployed 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.

6.3 Are results driven by substitution across areas?

In table 8 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 8 – 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)	0.6666***	0.6637***
	(0.0173)	(0.0173)
LTU	0.8129***	0.8291***
	(0.0346)	(0.0354)
2015	1.7711***	3.5668***
	(0.0393)	(1.0142)
South	1.7029***	3.9688***
	(0.0416)	(1.2500)
LTU x 2015	1.2161***	1.2060***
	(0.0507)	(0.0504)
LTU x South	1.6056***	1.5907***
	(0.0729)	(0.0723)
2015 x South	0.8297***	0.1597***
	(0.0276)	(0.0674)
LTU x 2015 x South	**0.6603*** **	**0.6627*** **
	(0.0388)	**(0.0390)**
Constant	0.0118***	0.0027***
	(0.0012)	(0.0006)
Observations	6,508,696	6,508,696
Pseudo R-squared	0.0120	0.0232

Robust standard errors, clustered by 168,408 individuals, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.

6.4 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 contributions paid by the employer, the effect on wage depends on how much of this gain is shared with the employee. In table 9 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 the 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 long-term unemployed 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 jobseekers, whose bargaining power is most likely very low.[18]

[18] 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.

Table 9 – Ordinary least squares regression

	Dependent variable: Wage in logarithm No controls	With controls (1)
Log (duration)	-0.0096	0.0020
	(0.0282)	(0.0275)
LTU	0.0854	0.0347
	(0.0648)	(0.0612)
2015	0.0163	-0.6096*
	(0.0253)	(0.3389)
South	0.0373	-1.1301***
	(0.0268)	(0.3511)
LTU x 2015	-0.0376	0.0019
	(0.0647)	(0.0611)
LTU x South	-0.0522	-0.0095
	(0.0674)	(0.0641)
2015 x South	0.0343	0.6680
	(0.0362)	(0.4400)
LTU x 2015 x South	**-0.0329**	**-0.0459**
	(0.0778)	**(0.0751)**
Constant	6.9206***	7.0279***
	(0.1107)	(0.2936)
Observations	4,037	4,037
Adjusted R-squared	0.002	0.078

Robust standard errors, clustered by 3,912 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.

7. 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 incentive scheme, 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 jobseekers. In particular, we find that the subsidy granted by law 407/90 was able to counteract the deterioration in employability associated with long-term unemployment. The disadvantage in accessing permanent jobs for long-term unemployed (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, a substitution across areas or with fixed-term contracts and there is no evidence that it led to an increase in wages.

One issue that we do not discuss in the paper is by what extent the subsidy might shape the entire distribution of the unemployment spells. For instance, the subsidy might give an incentive to wait longer in unemployment – 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 short-term unemployed 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, further analysis is needed to shed light on this issue, possibly by means of a structural model that considers changes in the entire unemployment duration distribution.

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Appendix

In what follows we describe in more detail how we built the dataset. First, computing unemployment duration was not straightforward, since very short-term contracts do not reset, just suspend, the unemployment duration counter. The time limit necessary to consider a period as "short" changed repeatedly during time and across areas. 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 unemployment duration

	Eligibles (1)		**Non-eligibles** (1)	
Rules (2)	**South**	**Centre-North**	**South**	**Centre-North**
Baseline	66.31	16.97	21.94	2.29
Simplified	65.92	15.60	23.04	2.42
Income	71.63	16.20	17.97	1.19

(1) We define as eligibles those individuals with unemployment duration between two and three years, and as non-eligibles those with unemployment duration between 6 and 18 months. (2) The baseline rule defines as short-term contracts, that suspend the unemployment 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 unemployed 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 unemployment 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).[19] Starting from the first job loss that satisfies these requisites, we track the individual over the following years. We increase unemployment duration by one week in every

[19] 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.

following Monday, as long as she does not find a new job.[20] Unemployment 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, unemployment duration is set back to zero until the individual loses her job again.

Table A2 – Logit model – Odds ratios – Restricted unemployment 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***	0.5877***
	(0.0593)	(0.0570)
LTU	0.8545**	0.8790*
	(0.0629)	(0.0648)
2015	1.7269***	3.6382***
	(0.0572)	(1.4260)
South	1.5853***	2.6499**
	(0.0577)	(1.1309)
LTU x 2015	1.2683***	1.2735***
	(0.0709)	(0.0714)
LTU x South	1.7365***	1.7389***
	(0.1049)	(0.1053)
2015 x South	0.9239	0.2727**
	(0.0456)	(0.1562)
LTU x 2015 x South	**0.6001*** **	**0.5964*** **
	(0.0472)	**(0.0470)**
Constant	0.0178***	0.0060***
	(0.0072)	(0.0030)
Observations	3,385,610	3,385,610
Pseudo R-squared	0.0107	0.0202

Robust standard errors, clustered by 131,399 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.

As explained in the main text, we define as control group those individuals with unemployment duration between 6 and 18 months. In this way we aim at minimizing the classification error, i.e. the percentage of hires that, according to our calculation of unemployment 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 LTU 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 A3-A4).

[20] 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.

We also used a more restrictive definition for the non-eligible group, reducing the unemployment 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, reported in Table A2, are qualitatively equal to those reported in Table 3, but the odds ratio of the triple interaction is lower. This result suggests that when compared to a more similar control group, the long-term unemployed result to have suffered more from the abolition of the subsidy designed for them.

Table A3 – 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)	0.7757***	0.7614***
	(0.0471)	(0.0462)
LTU	0.6147***	0.6062***
	(0.0679)	(0.0673)
2015	1.8384***	10.6833***
	(0.0955)	(7.1386)
South	1.5201***	8.5650***
	(0.0842)	(6.1313)
LTU x 2015	1.4074***	1.4488***
	(0.1540)	(0.1599)
LTU x South	2.0737***	2.1218***
	(0.2438)	(0.2507)
2015 x South	0.7916***	0.1017**
	(0.0609)	(0.0960)
LTU x 2015 x South	**0.6131*** **	**0.6036*** **
	(0.0892)	**(0.0882)**
Constant	0.0071***	0.0006***
	(0.0017)	(0.0003)
Observations	1,095,781	1,095,781
Pseudo R-squared	0.0102	0.0239

Robust standard errors, clustered by 28,582 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.

Finally, in Tables A5 and A6 we perform a further robustness check by estimating the same model as in Table 5 with the difference that the unemployment 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 unemployment 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.

Table A4 – 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***	0.6337***
	(0.0159)	(0.0159)
LTU	0.8463***	0.8664***
	(0.0350)	(0.0359)
2015	1.7728***	3.3731***
	(0.0382)	(0.9337)
South	1.5754***	4.7827***
	(0.0372)	(1.4786)
LTU x 2015	1.2085***	1.1917***
	(0.0492)	(0.0486)
LTU x South	1.5870***	1.5733***
	(0.0705)	(0.0700)
2015 x South	0.8573***	0.2234***
	(0.0274)	(0.0932)
LTU x 2015 x South	**0.6650*** **	**0.6661*** **
	(0.0381)	**(0.0382)**
Constant	0.0150***	0.0036***
	(0.0015)	(0.0008)
Observations	6,725,038	6,725,038
Pseudo R-squared	0.0116	0.0230

Robust standard errors, clustered by 174,752 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple 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*	0.9913
	(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	0.9925	1.0173
	(0.0770)	(0.0790)
2015	1.7610***	3.0793***
	(0.0380)	(0.8456)
South	1.6794***	3.2783***
	(0.0396)	(0.9908)
LTU x 2015	1.1843***	1.1740***
	(0.0477)	(0.0474)
LTU x South	1.5329***	1.5196***
	(0.0670)	(0.0666)
2015 x South	0.8496***	0.2328***
	(0.0273)	(0.0945)
LTU x 2015 x South	**0.6889*****	**0.6919*****
	(0.0390)	**(0.0393)**
Constant	0.0039***	0.0010***
	(0.0005)	(0.0002)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0119	0.0230

Robust standard errors, clustered by 174,843 individuals, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(1) We control for gender, education, age, age squared and monthly dummies, together with their double and triple 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	1.8121***	1.8181***
	(0.1121)	(0.1126)
Unemployment duration 2	1.7493***	1.7363***
	(0.1091)	(0.1084)
Unemployment duration 3	1.5108***	1.5109***
	(0.0945)	(0.0946)
Unemployment duration 4	1.4760***	1.4898***
	(0.0925)	(0.0934)
Unemployment duration 5	1.3161***	1.3105***
	(0.0830)	(0.0827)
Unemployment duration 6	1.3701***	1.3631***
	(0.0754)	(0.0750)
Unemployment duration 7	1.3381***	1.3350***
	(0.0434)	(0.0434)
Unemployment duration 8	1.1296***	1.1232***
	(0.0379)	(0.0377)
Unemployment duration 9	1.0640*	1.0663*
	(0.0361)	(0.0362)
LTU	0.7653***	0.7789***
	(0.0470)	(0.0479)
2015	1.7597***	3.0718***
	(0.0380)	(0.8435)
South	1.6794***	3.2750***
	(0.0396)	(0.9898)
LTU x 2015	1.1858***	1.1750***
	(0.0477)	(0.0474)
LTU x South	1.5332***	1.5197***
	(0.0670)	(0.0666)
2015 x South	0.8497***	0.2330***
	(0.0273)	(0.0945)
LTU x 2015 x South	**0.6886*** **	**0.6919*** **
	(0.0390)	**(0.0393)**
Constant	0.0016***	0.0004***
	(0.0001)	(0.0001)
Observations	6,723,060	6,723,060
Pseudo R-squared	0.0120	0.0230

Robust standard errors, clustered by 174,843 individuals, 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 gender, education, age, age squared and monthly dummies, together with their double and triple interaction with *2015* and *South* dummies.