

Getting Stable: An Evaluation of the Incentives for Permanent Contracts in Italy*

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Abstract

There is little evidence about the effectiveness of incentives for the conversion of fixed-term contracts into permanent jobs. We aim at filling this gap by studying a recent Italian program, which provides benefits to employers who convert contracts for workers in specific demographic groups (females, younger men). Due to binding funding constraints, the incentives were available only for few days, allowing us to employ a difference-in-differences strategy between very similar short periods of time. By using high-quality administrative microdata for the region of Veneto, we show that the subsidy increased conversions by 83% on average, with no evidence of substitution effects over time or across groups of workers.

JEL: J21, J41, J48

Keywords: fixed-term contracts, permanent employment, diff-in-diffs

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1. Introduction

In the process of reforming the labor market institutions, a government faces a trade-off between the need of increasing labor market flexibility and the desire to guarantee some level of stability to individual workers. On the one hand, the use of fixed-term contracts might bring about efficiency gains. Apart from technological reasons (buffer stock, temporary substitutions, seasonal jobs; see Cappellari et al., 2012), employers can use short-term appointments to increase employees' productivity, by both inducing workers to exert more effort (Engellandt and Riphahn, 2005) and screening them before a permanent hire in order to avoid mismatches. From the worker's point of view, fixed-term contracts may be preferable when the individual is in the process of choosing his/her best occupation, or for workers who are less interested in investing into job-specific human capital (Booth et al., 2002). On the other hand, a widespread use of temporary contracts may increase job-insecurity, therefore affecting the welfare of the workers and their choices. For instance, empirical evidence suggests that job-insecurity may have negative effects on youth emancipation (Becker et al., 2010) and fertility (Modena and Sabatini, 2012; Priftin and Vuri, 2013). On related grounds, temporary contracts are generally associated with a reduced level of training (Arulampalam and Booth, 1998; Arulampalam et al., 2004), and therefore with less chances to increase workers' human capital. Clearly, these disadvantages are likely to be more problematic if individuals experience a series of temporary jobs without access to a permanent one.

A government may wish to regulate the use of fixed-term contracts in order to maximize the difference between the advantages of more flexibility and the costs of job-insecurity. In doing so, it faces relevant constraints. Temporary workers are generally less expensive for firms, because of both smaller social contributions and a less stringent employment protection legislation (EPL). Therefore, making these arrangements more costly might jeopardize fixed-term hiring. By the same token, it can be politically unfeasible to reduce the cost of permanent contracts, which are overwhelmingly protected by trade unions.

One possible way to increase the proportion of stable jobs would be to subsidize direct hires with a permanent contract. This incentive is likely to reduce the inflow of temporary workers and increase the one of long-term employees. However, the subsidy is also likely to diminish the efficiency gains related to the use of flexible contracts. A different solution would be to subsidize employers for conversions from fixed-term to open-end contracts. This scheme allows them to freely hire temporary workers, possibly generating the efficiency gains related to greater flexibility, but at the same time reducing the risk that individuals incur in a series of fixed-term contracts. Given the information asymmetry between workers and potential employers, an incentive for

contract conversion may be more effective than one for direct hires, because it exploits the preference of employers to sign permanent contracts with workers that have already been screened.

In this paper we evaluate the effectiveness of a program that provides monetary incentives for conversions. The ability of a program of this type to reach the stated target should not be taken for granted: employers will turn a position into permanent only if the subsidy is sufficiently generous to compensate the relative gains of keeping it as temporary. In the booming literature on policy evaluation of labor market schemes, the assessment of conversion programs seems to be rather scarce. The available evidence mostly refers to policies that either increased the cost of fixed term contracts or made it cheaper to hire on a permanent basis (for instance, Hernanz et al., 2003; Maurin and Michaud, 2004; Mendéz, 2013). Only few studies (Cipollone and Guelfi, 2003, 2006; Battiloro and Costabella, 2011) evaluated incentives which also applied to the conversion of fixed-term positions.¹

In this paper we contribute to the literature by providing evidence from Italy on a scheme that subsidized conversion from temporary to open-end contracts, the Decree 5 October 2012. The policy did not apply to all groups of workers, as it excluded men aged 30 or more. Furthermore, the funds were limited; as a matter of facts, they were exhausted in a couple of weeks. We elaborate on these features of the scheme and evaluate its effects through a diff-in-diffs strategy, which compares eligible workers with their non-eligible counterparts (older males) over very short periods of time.

Using aggregate time series from the Veneto region, Anastasia et al (2013) showed that for the eligible groups the total number of conversions approximately doubled over the period of validity of the policy with respect to the previous year, and that there was a significant difference between the totals for men aged 29 and men aged 30. Differently from them, we directly use the microdata built from the administrative archives of the same region, a dataset that allows us to track individual fixed-term contracts over time. We focus on how their probability of conversion changed over different periods within 2012. In particular, we distinguish between periods with different exposure to the effects of the scheme (pre-announcement, announcement, treatment, end of funds). Our estimates suggest that the policy increased the probability of transformation by 83% with respect to the counterfactual rate of conversion, with larger effects for men under 30 and women over 30 and smaller impact on younger women. There is no evidence that entrepreneurs postponed conversions during the short period between the announcement and the full implementation of the program, nor that they reduced the conversion rate after the funds were terminated; we also fail to find evidence that the impact is due to substitution between eligible and non-eligible workers. These results are

¹ Portugal introduced, in 2002, a scheme of incentives for the conversion of temporary contracts, but to the best of our knowledge no study has evaluated its effects.

robust to several checks, including a falsification exercise aimed at detecting infra-annual confounding trends. We finally show that the effect seems to have lasted during the eight months after the end of the policy, which is the time extension of the last available data at the moment of writing.

The paper is structured as follows. Section 2 briefly summarizes the related literature. Section 3 outlines the policy. Section 4 presents the data, while Section 5 describes the identification strategy. Section 6 discusses the results. Section 7 compares the scheme with two other similar policies from Italy, to tentatively draw some conclusions for program design.

2. Previous literature

Only a few papers study the effect of programs aimed at modifying the relative profitability of short-term *vs* open-end arrangements. Hernanz et al (2003) evaluate a Spanish reform in 1997 that reduced the cost of open-end contracts. They find a positive effect on flows to permanent jobs. Mendéz (2013) criticize their results, by claiming that their assumption that individuals aged between 30 and 45 could not be hired with the new open-end contracts is not correct, because they could if they had been previously hired as fixed-term workers. Using a different method and evaluating also a previous reform that restricted the use of fixed-term contracts (1994), he provides evidence suggesting that the conversion rate did not increase. Maurin and Michaud (2004) focus on France, where a reform in 2002 increased the cost of terminating a temporary contract instead of converting it into a permanent one. Compared to a subsidy for conversion, this measure also implies an extra-cost in hiring individuals with a fixed-term contract. The authors provide evidence of an increase in the proportion of temporary contracts converted into permanent ones, but they also find that the higher costs induced a decrease in the number of new fixed-term hires and therefore a contraction in the overall employment. More related to our investigation, an Italian program in 2001 provided employers with incentives for hires or conversions with a permanent contract, but conditional on increasing the total workforce of the firm. Cipollone and Guelfi (2003, 2006) find evidence of no aggregate effects, although the results were highly heterogeneous by education and previous employment condition. Battiloro and Costabella (2011) evaluate a subsidy of around 4,500 euros for conversions, that was introduced in 2007 in the Province of Turin in Italy. By comparing the time series in Turin with those from other unaffected provinces, they find no evidence of an increase in the number of conversions.

Our contribution is also related to two other streams of literature. The first one refers to the effects of EPL on job flows. Indeed, the incentives set out by the Decree 5 October 2012 were introduced to compensate the firms for the higher cost of permanent contracts. Several studies

exploit an Italian reform in 1990 that increased the level of EPL for permanent workers in small firms. Their main finding suggest that higher EPL decreases both accessions and separations, consistently with theoretical predictions (Bertola, 1990; Boeri and Jimeno, 2005; Kugler and Pica, 2008). A stronger protection for permanent workers increases their relative cost with respect to temporary employees: Grassi (2009) uses administrative data from Italy and provides evidence that the more stringent EPL induced by the reform reduced the rate of conversion from fixed-term to permanent contracts. Similarly, Schivardi and Torrini (2008) show that larger firms, to which higher EPL applies, tend to make more use of flexible workers.

The other related stream of literature is the one which focuses on the *stepping stone* effect (Booth et al., 2002); that is, on the ability of short-term appointments to promote the access to permanent employment. Empirically, this literature studies the probability that individuals with a fixed-term contract at a certain point in time are employed on a permanent basis later on, and compares them with individuals who were instead unemployed. Booth et al. (2002) find that in the UK standard fixed-term positions lead to permanent jobs later in time. In Italy, the available evidence supports the idea of the stepping stone effect (Ichino et al., 2005; Picchio, 2008; Barbieri and Sestito, 2008; Bruno et al., 2012), although results are highly heterogeneous by type of temporary contract (Berton et al, 2011). Our paper analyze how a subsidy can affect the rate of conversion of temporary contracts into permanent ones; that is, how public money may help the stepping stone effect to take place.

3. The program

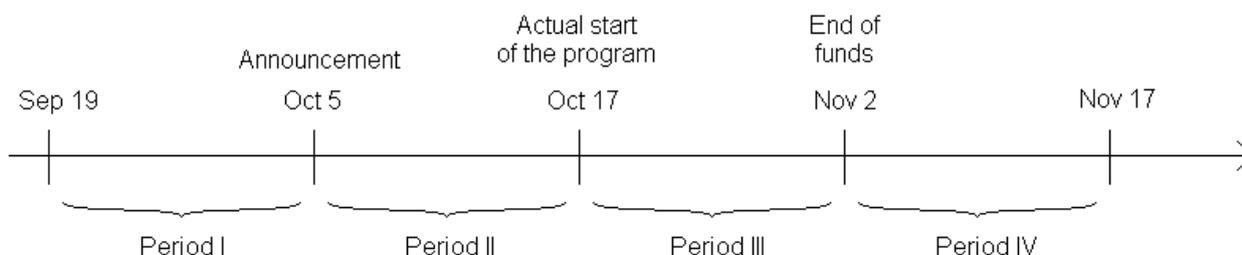
The Decree 5 October 2012 introduced financial incentives for employers who:

- converted temporary contracts for eligible workers into permanent ones; the incentive in this case was equal to 12,000 euros per conversion;
- stabilized workers holding non-standard contracts (*parasubordinati*) or who had concluded a temporary contract in the previous 6 months and had been unemployed thereafter; similarly this incentive amounted to 12,000 euros per stabilization;
- hired workers with a temporary contract, but only if this hire increased the total workforce of the firm. In this case the benefit was between 3,000 and 6,000 euros depending on the length of the contract.

The scheme required that the job lasted for at least 6 months after the conversion/hire.² Eligible workers were men aged less than 30, and women of any age. In case of permanent contracts on a part-time base, the amount of the subsidy was proportionally reduced. Moreover, each employer could request at most 10 incentives.

The Decree made use of a dedicated national fund for the increase in the employment of youth and women. The fund was set up by law 201/2011 (December 2011), but details on how the money would have been used were not fully defined until 5 October 2012, when the Decree introducing the program was approved by the Ministry of Labor and Social Policies jointly with the Ministry of Economics and Finance. We therefore take the latter as the date of announcement of the program. Figure 1 depicts the timeline of the policy.

Figure 1 Timeline of the policy



The incentive applied only to conversions/stabilizations/hires starting from the official date of publication of the Decree: the 17th of October. The program was supposed to be in place up to the 31st of March 2013. At the moment of the formal request, the firms should have already signed (and communicated to the competent administration) the new contract with the eligible worker. Given that funds were limited, employers were allowed to check online, at the time of the application, whether the number of requests made until then was already sufficient to exhaust the total budget. However, if the funds terminated on the day of application, demands would have been funded on a first-come-first-served basis. The 2nd of November the National Institute for Social Security (INPS) communicated that the number of requests received until then was enough to terminate the funds and, therefore, the agency discouraged new requests. Some requests arrived after the 2nd of November. This happened because it was uncertain whether all the demands already presented were actually eligible. Therefore, some employers might have applied, notwithstanding the INPS warning, hoping that they could have still benefited from funding.

² Note that the program did not introduced any constraint as to the variation in total workforce following the conversions/stabilizations.

The only publicly available data on the program come from the website of the Ministry of Labor: at the national level, between 17/10/2012 and 31/03/2013, 44,054 requests were made, of which only 24,581 were accepted.³ The decision on the acceptance was communicated to the firms only in June 2013, to comply with the requirement that the job had lasted at least 6 months. Given that the rules of the game were relatively simple, the selection of requests was mostly based on the order of presentation, rather than on their eligibility compliance.

From the information released by the Ministry of Labor we know that around 90% of all incentives was distributed for conversions or stabilizations of temporary contracts. Considering that the incentive for direct hires with temporary contracts was less generous (and required an increase in the overall workforce), it is not surprising that few requests were made for that option. Compared to temporary workers, the number of *parasubordinati* is much smaller; moreover, this group is highly heterogeneous as for the features of the firm-employee relation. For these reasons in this paper we focus on temporary contract conversions only. We do not consider those who had concluded a temporary contract within 6 months and had been unemployed thereafter. The main issue is that these individuals may also migrate to/from other regions and therefore we do not always observe whether they are subject to a stabilization. Nevertheless, in the robustness section we show that there is no evidence of changes in the number of direct hires during the validity of the incentives.

4. Data

Since March 2008 employers who hire new workers or make alterations to pre-existing contracts are obliged to communicate it to a regional agency through an online system, named *sistema di comunicazione obbligatoria* (Anastasia et al., 2009, 2010). This administrative archive does not provide the complete stock of workers, given that permanent contracts signed before March 2008 do not enter it. However, the archive allows researchers to track the universe of job relationships that started with a temporary contract after March 2008. Considering that standard fixed-term contracts can be signed only for up to 36 months, their entire stock should be observable in the files starting from March 2011.⁴

The quality of these data depends not only on the accuracy of the employers, but also on the skills of the regional agencies in charge of maintaining and validating the archives. In Italy, the region with the longest tradition in analyzing these data is Veneto (Maurizio, 2006), where the local

³ http://www.lavoro.gov.it/Notizie/Pages/20130610_Incentivi_giovani_donne.aspx (last retrieved: 20/01/2014).

⁴ There are few exceptions that involve a small number of workers, in particular contracts for directors that could be signed for 5 years. However, our data are based on a region that was already collecting the data from several years before 2008, and therefore we should be able to observe almost all relevant contracts in 2011 and 2012.

agency (*Veneto Lavoro*) started elaborating a dedicated software since 1996. Moreover, using the flow of the communications, the agency organizes a full set of longitudinal microdata that track single individuals through time. *Veneto Lavoro* makes available to researchers the entire universe of microdata, while at the national level these data are available only for a subset of workers (individuals born in 48 different dates) and, crucially, without any information about contract conversions. The region of Veneto is one of the most important economic area of the country: according to the Labor Force Survey, in 2012 Veneto accounted for 9.5% of total employees in Italy, and for the 8.3% of total employees on a temporary contract.

We focus on the (regional) universe of job relationships that started with a standard temporary contract (*tempo determinato*) and were still active as temporary during 2012. These relationships might either keep their short-term nature, or be converted into permanent positions. We analyze the extent to which there was a change in the event “temporary contract converted into permanent” because of the program. We select only standard temporary contracts; that is, we exclude contracts that are activated to substitute a permanent worker on leave (*per sostituzione*), signed with a temporary employment agency (*interinale o a scopo di somministrazione*), allowing the employee to work in her own place (*a domicilio*), and designed for particular sectors or for other particular reasons.⁵ We do very minor corrections on the raw data, dropping few cases where we observed a change in the nature of the contract without an explicit reason and correcting the date of conversion for some job-relationships where the conversion episode was repeated more than once. We also exclude very few cases (around 0.6% of those job-relationships that were subject to conversions) where the standard temporary contract was converted into a non-standard permanent contract, because it may signal measurement error.

Using the longitudinal information on each job relationship $i=1, \dots, N$ we build a panel over four different periods $t=1, \dots, 4$ in 2012. Each period has a length between 12 and 16 days (the next Section explains the details on these units of time). For each period, we keep only job-relationships that are active; that is, temporary contracts with at least one day of duration. The panel is unbalanced for two reasons: a) new temporary contracts can be signed during the year; therefore new job relationships can enter the panel in any period; b) temporary contracts terminate (either for

⁵ This selection clears possible distortions generated by the specificity of these contracts. Among eligible fixed term workers in the selected periods in 2012, 71.9% had standard contracts, and they represented 90.7% of the contracts that were converted into permanent. Our main results carry through to the more general case, where all these nonstandard contracts are kept into the dataset (apart from those signed with a temporary employment agency which were not eligible for the incentive); in this case the point estimate is smaller, but it is quite close to our main results if measured as a proportion of the counterfactual. Our findings are also undistinguishable if the public sector is dropped from the sample. All these sets of results are available on request.

the natural end of the contract or for other reasons) or are converted into permanent ones; these contracts exit the panel the period after that in which the event takes place.

Our main outcome is a binary variable:

$$y_{it} = \mathbf{1}[\text{job-relationship } i \text{ is converted from temporary to permanent in period } t] \quad (1)$$

so that our results have to be interpreted as the impact of the scheme on the probability that a temporary contract active during a single period t is converted into permanent during the same period.

For each job-relationship we observe some time invariant characteristics: educational level of the worker, gender, sector of activity and citizenship.⁶ We also know two important time variant observables: the worker's age at the start of each period and the job-relationship duration at the end of the period.⁷ Finally, we can identify the employer: this allows us to cluster the standard errors at the firm level and observe how many temporary contracts are referred to the same firm, in a particular moment in time.

From the panel, we drop those observations with missing age or gender (21 in total), and we select only individuals aged between 16 and 65, mainly in order to avoid extreme cases that are likely to signal measurement error. We end up with 593,028 observations, approximately 148 thousand job relationships per period.

5. Identification strategy

We use a diff-in-diffs strategy over different periods within 2012 and different groups in terms of terms of program eligibility, defined on the basis of demographic characteristics. We focus on the effect of the policy on the eligible groups conversion rate, so that our estimates can be interpreted as the Intention To Treat (ITT). This is the effect of interest for a policy maker who wants to understand the overall impact of the program. Our data do not contain information on who actually received the incentive. Nevertheless, in Section 6 we also calculate the actual cost for each *increased* unit of conversion in a scenario in which all transformations for the eligible groups are incentivized.

Following the standard model (Angrist and Pischke, 2009), we assume that the expected potential outcome when not treated (indexed by 0) depends additively on the group g and on the period t :

⁶ We use the information on the educational level in the most recent communication regarding the temporary contract (before the conversion, in case it takes place).

⁷ We censor to 5 years 0.5% of the observations who have longer duration.

$$E[y_{0igt} | g, t] = \mu_g + \lambda_t \quad (2)$$

which implies two basic assumptions (Blundell and MaCurdy, 1999):

A1. The time trend is parallel across groups.

A2. The group effect does not change over time, that is the group composition is (on average) constant.

Secondly, we assume that the effect of the policy is additive, so that the potential outcome when treated (indexed by 1) is simply

$$E[y_{1igt} | g, t] = E[y_{0igt} | g, t] + \delta = \mu_g + \lambda_t + \delta. \quad (3)$$

Exploiting the timeline of the policy (see Figure 1) we define 4 periods of interest:

- Period I: [19/09 - 4/10]; that is, 16 days before the announcement.
- Period II: [5/10 - 16/10]; that is, the 12 days between announcement and the actual start of the program.
- Period III: [17/10 - 01/11]; that is, the 16 days when the incentives were fully available.
- Period IV: [02/11 - 17/11]; that is, the 16 days after INPS declared that funds were (presumably) already finished.

We assume that in Period I the policy should not have had any effect: only on the 5th of October the scheme of incentives was made public, receiving full attention from the media. Differently, most of the activity should have taken place in Period III, given that employers already had enough time to acquire the information. However, the effect of the policy may not be limited to changes during that period if employers substituted conversions over time in order to benefit from the incentives.⁸ To start with, if they were already *fully* informed during Period II, they may have postponed some conversions in order to wait for the scheme to be in place. Moreover, the fact that funds were limited clearly gave them a strong incentive to anticipate in Period III conversions that would have taken place, without the scheme, much later in time. This is the reason why we also analyze the days after the shortage of funding (Period IV). In both cases (periods II and IV) we expect that, if employers substituted conversions over time in order to benefit from the allowances, the effect of the policy should compensate those observed in Period III. The length of periods I and IV was chosen in order to match the period of fully validity of the incentives.

⁸ For a discussion of announcement and implementation effects in diff-in-diffs analysis, see Blundell et al. (2011).

The groups entitled for the scheme are defined by the policy: men over 30 were not eligible, while younger men and women of any age were. We allocate individuals to each group according to their age at the begin of the period.⁹

In order to identify the effect δ , we exploit the structure of entitlement envisaged by the policy. Given that men over 30 were not eligible, we use these workers as a control group to estimate the trend over periods and then use it to clear the time effects for other groups as well, thanks to assumption A1. Once we are able to identify λ_t , we also need to clear out the group effect. In this case, we exploit the period before the announcement (Period I). If, as we argued, the policy could not have any effect at that time, then during that period we observe only y_0 for everyone, and therefore we can use it to identify the differences across groups. Finally, for the eligible workers in the post-announcement periods (II-III-IV) we observe only the outcome when treated (y_1), and therefore we can remove from it the time and group components to get the policy effect δ .

Our results are derived from a specification where the treatment effect δ varies by period of treatment (II-III-IV) and across eligible groups. This is equivalent to a series of 2X2 diff-in-diffs estimates, where the control group is always Men over 30 and the pre-reform period is always Period I. For each single group (men aged less than 30, women aged less than 30 and women aged 30+) or for all the eligible groups altogether, the effects of interest can be identified from the coefficients on the interaction terms from the regression:

$$y_{it} = \beta_0 + \beta_E \mathbf{1}[\text{Eligible}]_{it} + \beta_{II} \mathbf{1}[\text{Period II}]_{it} + \beta_{III} \mathbf{1}[\text{Period III}]_{it} + \beta_{IV} \mathbf{1}[\text{Period IV}]_{it} + \\ + \delta_{II} \mathbf{1}[\text{Eligible}]_{it} \times \mathbf{1}[\text{PII}]_{it} + \delta_{III} \mathbf{1}[\text{Eligible}]_{it} \times \mathbf{1}[\text{PIII}]_{it} + \delta_{IV} \mathbf{1}[\text{Eligible}]_{it} \times \mathbf{1}[\text{PIV}]_{it} + \varepsilon_{it} \quad (4)$$

For δ to identify a causal effect, we need to assume that the policy was an exogenous shock, so that the treated groups were not endogenously chosen among those that would have experienced an increase in conversion rates in any case. This potential threat seems to be hardly realistic: eligibility was targeted on workers that were more likely to be hit by the on-going economic crisis. Furthermore, our estimates are based on relatively short periods of time, next to each other. Hence it is difficult to think that, in the absence of the policy, their conversion rate would have changed abruptly only during the 16 days in which incentives were fully available.

There are other threats to identification. First of all, there may be seasonal trends that diverge across groups. Since we estimate the effect of the scheme for very short periods of time during Fall 2012, group-specific seasonality might unduly confound identification. In the empirical section

⁹ We also replicated the estimates defining the age as referring to the end of the period, with no sensible changes for the results.

below, we check whether this is the case by running a falsification test using 2011 calendar periods analogous to the ones we focus on for the year 2012.

Secondly, the panel is unbalanced. This implies that the group composition is not guaranteed to be stable over time. To lessen this concern, we run the same regressions but adding a large set of covariates (educational level, sector of activity, citizenship, age at the begin of the period, duration of the job-relationship at the end of the period), which should differentiate out overtime variations in group compositions.

Thirdly, we need to assume that in the Period I employers were not aware of the policy, or at least that the available information was not enough for them to already change their decisions in order to later benefit from the incentives. To test this assumption we run a diff-in-diffs regression that compares the different groups between Period I in 2012 and the analogous period in 2011.

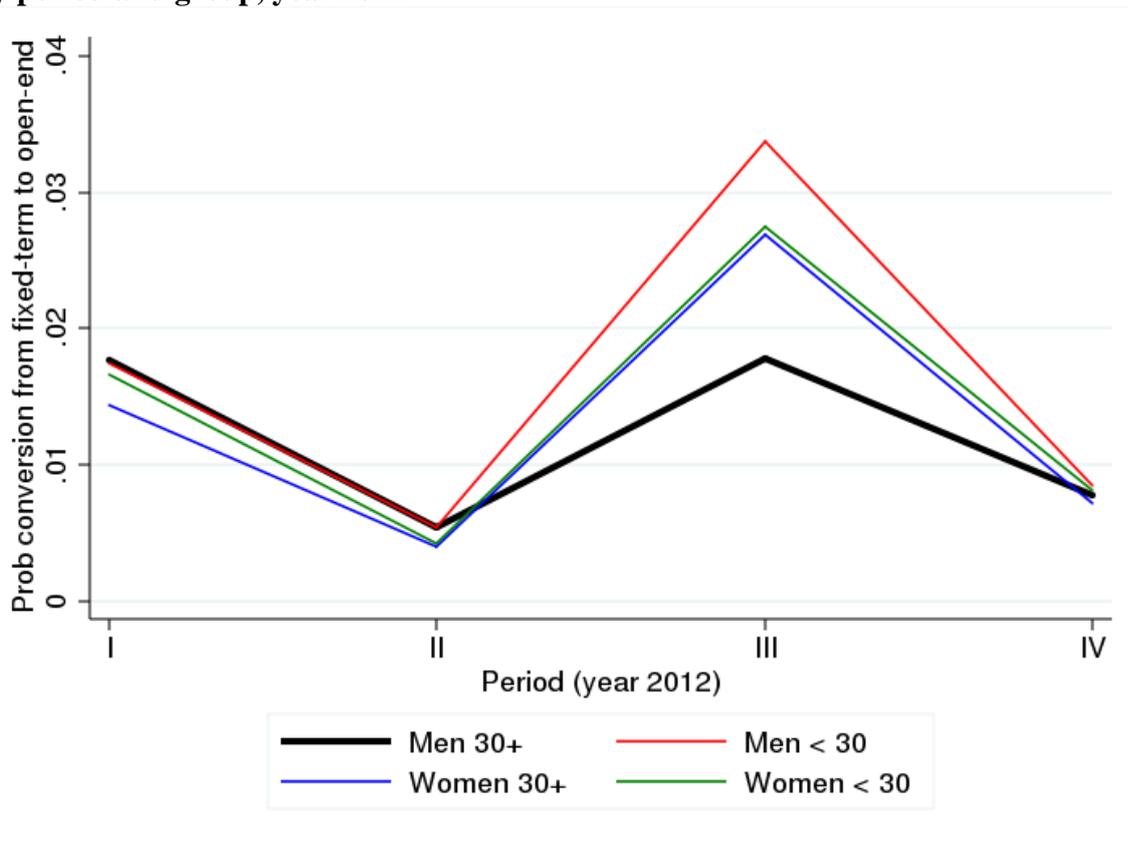
Last but not least, apart from substitution over time, which we directly addressed by looking at periods II and IV, there may be other reactions that counteracted the effectiveness of the scheme. The most likely is that the incentives could have induced the employers to favor workers from the eligible demographic groups and reduce the conversions for the non-eligible (men over 30). We provide evidence on this potential channel of substitution by looking at the change in the conversion rate for non-eligible between the same periods in 2012 and the previous year. Furthermore, as a consequence of the policy, employers could have indirectly subsidized direct hires with permanent contracts by hiring workers with a temporary one and converting it after few days. Similarly, they could have favored conversions with respect to direct hires. We also show what happens during the periods of validity, and relatively to 2011, to the number of jobs starting with a permanent contract.

6. Results

6.1 Main results

Figure 2 shows the rate of conversion for each of the four periods across groups in 2012. Before the announcement (Period I), the rates of conversion are similar for all groups, with only a slightly smaller probability for older women. In Period III the probability that a temporary contract becomes permanent substantially increases for all the eligible groups compared to the non-eligible one. The jump is larger for younger men. The figure also suggests no signal of substitution effects overtime: in Period II and Period IV the rates remain quite similar across groups.

Figure 2 Probability of conversion from temporary to permanent contract during the period, by period and group, year 2012



The diff-in-diffs regressions (Table 1) confirm the findings.¹⁰ Focusing on the entire group of the eligible workers, Column (1), there is no evidence of an anticipation effect during the days between the announcement and the actual date of validity (Period II): although there is a decrease in the overall conversion rate, this does not diverge across eligible and control groups. Likely, employers became fully aware of the scheme when the program was about to start, and therefore, substantial arbitrage across periods was precluded. Differently, there is a significant increase in the conversion rate by 1.3 percentage points during the 16 days in which incentives were available and fully funded (Period III).

Our results also document a positive effect of the scheme in Period IV, that is after the day in which INPS announced that applications were already sufficient to exhaust funds. One possible explanation is that some employers might have realized that the funds were (probably) terminated only after having signed a permanent contract with the temporary worker; alternatively, they might have converted on purpose after the 2nd of November, in the expectation that some public money was left for them to benefit (see Section 3).

¹⁰ We always use standard errors clustered at the employer level to account for potential common shocks across different job-relationships. We also tried with standard errors clustered by sector of economic activity: all the main results on statistical significance carry on.

Table 1 Main results (probability of conversion from temporary to permanent contract during each single period in 2012)

Control group: Men \geq 30 Dependent variable: dummy for conversion	(1)	(2)	(3)	(4)
	All eligibles	Eligible group: Men < 30 Women < 30 Women \geq 30		
Eligible	-0.0021*** (0.0007)	-0.0003 (0.0011)	-0.0011 (0.0011)	-0.0034*** (0.0009)
Period II	-0.0123*** (0.0007)	-0.0123*** (0.0007)	-0.0123*** (0.0007)	-0.0123*** (0.0007)
Period III	0.0001 (0.0009)	0.0001 (0.0009)	0.0001 (0.0009)	0.0001 (0.0009)
Period IV	-0.0099*** (0.0008)	-0.0099*** (0.0008)	-0.0099*** (0.0008)	-0.0099*** (0.0008)
Eligible \times Period II	0.0011 (0.0008)	0.0004 (0.0012)	0.0000 (0.0013)	0.0020** (0.0010)
Eligible \times Period III	0.0130*** (0.0012)	0.0162*** (0.0019)	0.0107*** (0.0019)	0.0124*** (0.0014)
Eligible \times Period IV	0.0020** (0.0009)	0.0010 (0.0014)	0.0014 (0.0014)	0.0027*** (0.0010)
Constant	0.0177*** (0.0007)	0.0177*** (0.0007)	0.0177*** (0.0007)	0.0177*** (0.0007)
Observations	593,028	324,463	304,880	423,177

Note: * $p < .10$ ** $p < .05$ *** $p < .01$. Standard errors clustered for employer in brackets. Estimates are obtained using StataTM 13. See Figure 1 for the definition of periods.

Columns (2)-(4) document the results for each single group of eligible employees. For workers under 30 there is evidence of a positive effect of the policy in Period III. The impact is larger for men and smaller for women. As for the other two periods, there is no statistically significant change with respect to the control group. For older women we still find a positive effect of the policy in Period III. However, there is also evidence of a positive effect in periods II and IV; the magnitude is around 0.2-0.3 percentage points. These findings might suggest a diverging trend for this specific group, which would violate assumption A1, rather than an actual policy effect. In particular, while the impact in Period IV can be rationalized on the basis of the scattered timing of the actual end of the scheme, the effect in Period II is puzzling. If nothing, we would have expected a decrease in the conversion rate for eligible workers in the time window between announcement and begin of validity. To take a cautious stance, we might be overestimating the effect of the program for this specific group. The overestimation should not be a big deal: if we interpolate the observed diverging trend, the bias would be around 0.2-0.3 percentage points, bringing the effect for women aged at least 30 closer to that for younger ones.

Table 2 provides some back-of-the-envelope calculations. Given that we do not know who actually received the incentive, we assume that all eligible conversions in period III were

subsidized.¹¹ This gives an upward estimate for the actual cost, because some conversions may have been excluded from the incentive as a consequence of the shortage of funding. However, this calculation is still of interest, as it shows the effectiveness of the program in a normal situation where all eligible conversions receive the subsidy.

Table 2 Summary of the effects

	All eligibles	Men < 30	Women < 30	Women ≥ 30
Counterfactual conversion rate from temporary to permanent during period III	0.0157	0.0175	0.0167	0.0145
Reform effect in period III	0.0130	0.0162	0.0107	0.0124
Counterfactual number of conversions during period III	1,395	402	307	684
Reform effect in number of conversions during period III	1,156	372	197	589
Reform effect / baseline	83%	92%	64%	86%
% full time on total conversions in period III	62%	84%	56%	50%
Average incentive (euro)	9693	11,047	9,345	9,007
Full cost per increased conversion (euro)	21,392	23,008	23,889	19,472

Note: the number of conversion is calculated as the estimated probability times the number of temporary contracts active in Period III. The second column does not precisely sum up the following three, because the estimate of the effect come from the aggregate model (Table 1, col. (1)). The average cost of a conversion is calculated assuming that all part-time are at half time.

We first compute the proportional increase in conversions due to the program as the ratio between the estimated effect in Period III and the counterfactual conversion rate predicted by the model.¹² As a share of the counterfactual conversions predicted in Period III, the impact of the program amounts to 92% for young men, 86% for older women and 64% for females under 30. The average effect for all the eligible groups is calculated to be 83%. This latter figure implies that in order to increase the number of conversions by one unit, the government has also financed 1.2 conversions that would have taken place even in the absence of the program. Given that our data refer to the universe of all temporary contracts (started with a standard type) for workers aged 15-65 in Veneto, we can also compute the effect of the scheme in terms of number of contracts, by multiplying the number of observations for the estimated probabilities. Among 2,551 conversions observed in Veneto in Period III, 1,156 of them are attributable to the program. Moreover, using the information on whether the converted contract is full or part time, we are also able to estimate the average incentive and the average cost per increased conversion. As reported in Table 2, on average 62% of the job-relationships subject to conversion in Period III were full-time. Assuming that all

¹¹ We also do not know the total amount of incentives that were distributed for conversion that took place in the Veneto region only and exactly during period III.

¹² In the calculation we do not account for the possible presence of an effect in Period IV as well, because it can come from a diverging trend for older women, and its impact is anyway quite small.

the part-times were at half of the standard working time, the average incentive was 9,693 euro.¹³ This implies that the full cost of an actual unit increase in the number of conversions with respect to the counterfactual is 21,392 euro, as it requires an expenditure of 11,700 euro on other transformations that would have taken place even in the absence of the policy.

6.2 Robustness checks

Given that our periods are different both in terms of months and calendar position within the month, the conversion rate across time might be affected by seasonal patterns. In our case, seasonality would bias the results only as long as there are group-specific seasonal trends. To check whether this problem affects our estimates we also replicate the same exercise of Table 1 over the analogous periods in year 2011, when the scheme was not in place (and no similar policy was implemented). That is, we run a falsification experiment. From Figure 3, which mirrors Figure 2 but refers to 2011, we notice no evidence of diverging trends. Table 3 reports the relevant regression estimates. In Column (1), we find evidence of a small (boundary statistically significant at the 10% level; p-value 0.095) drop in Period III for the groups of interest, which would either imply that our results are underestimating the true effect (if the drop would have been there even in the absence of the policy), or that differential (by groups and periods) shocks to conversion rates materialized. However, the estimate for Period III in the falsification exercise is quite small compared to the effect estimated for the same period in 2012, when the policy was effective. If we look at results for the different eligible groups (columns (2)-(4) in Table 3), the small drop in Period III seems to be driven only by older women (again the coefficient is significant only at the 10% level). Notice also that the size of this drop, 0.2 percentage points, would broadly compensate the previously discussed positive bias for this specific demographic group.¹⁴

¹³ This value is similar to the one that can be obtained dividing the total amount spent in Italy by the total number of incentives distributed, using the info available on the website of the Ministry of Labour http://www.lavoro.gov.it/Notizie/Pages/20130610_Incentivi_giovani_donne.aspx (last retrieved: 20/01/2014).

¹⁴ One could also combine the falsification over 2011 and the main estimates over 2012 to obtain triple-difference estimates. We also run this joint regression, obtaining results that are qualitatively similar and support our conclusions. However, given that in 2011 the interaction terms are generally economically small and not statistically significant, we prefer to focus on the diff-in-diffs within 2012 in order to avoid introducing noise in our main estimates.

Figure 3 Probability of conversion from temporary to permanent contract during the period, by period and group, year 2011

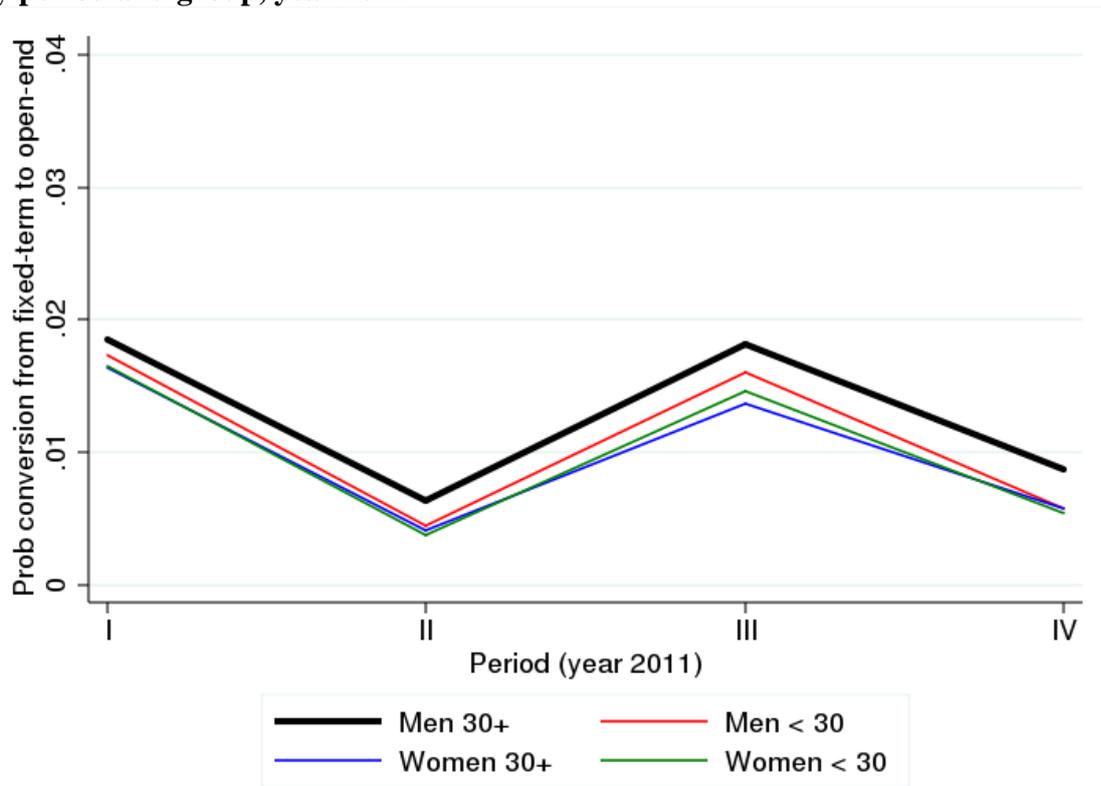


Table 3 Falsification (probability of conversion from temporary to permanent contract during each single period in 2011)

Control group: Men \geq 30 Dependent variable: dummy for conversion	(1)	(2)	(3)	(4)
	All eligibles	Men < 30	Women < 30	Women \geq 30
Eligible	-0.0018** (0.0008)	-0.0012 (0.0010)	-0.0020 (0.0013)	-0.0021** (0.0010)
Period II	-0.0121*** (0.0008)	-0.0121*** (0.0008)	-0.0121*** (0.0008)	-0.0121*** (0.0008)
Period III	-0.0003 (0.0009)	-0.0003 (0.0009)	-0.0003 (0.0009)	-0.0003 (0.0009)
Period IV	-0.0097*** (0.0008)	-0.0097*** (0.0008)	-0.0097*** (0.0008)	-0.0097*** (0.0008)
Eligible \times Period II	-0.0004 (0.0009)	-0.0008 (0.0011)	-0.0006 (0.0014)	-0.0002 (0.0010)
Eligible \times Period III	-0.0018* (0.0011)	-0.0009 (0.0014)	-0.0016 (0.0018)	-0.0024* (0.0013)
Eligible \times Period IV	-0.0012 (0.0009)	-0.0017 (0.0012)	-0.0013 (0.0014)	-0.0008 (0.0011)
Constant	0.0184*** (0.0007)	0.0184*** (0.0007)	0.0184*** (0.0007)	0.0184*** (0.0007)
Observations	614,895	340,214	313,218	438,457

Note: * p<.10 ** p<.05 *** p<.01. Standard errors clustered for employer in brackets. Estimates are obtained using Stata™ 13. See Figure 1 for the definition of periods.

Given that the panel is unbalanced, the group composition is not guaranteed to be stable over time.¹⁵ In order to see whether large changes in the group composition are affecting the results, in Table 4, column (1) we also add to the basic regression (that of Table 1, Column 1) some relevant covariates: dummies for sector of activity (ATECO 2 digits), dummies for educational level, dummy for Italian citizenship, age at the begin of the period, job-relationship duration at the end of the period.¹⁶ The results are basically unchanged. Therefore, overtime variation in group composition seems not to be driving our findings. Column (2) provides the results we obtain by adding the covariates to the falsification experiment. The bizarre effect found previously on Period III is no longer statistically significant.

Table 4 Robustness checks (probability of conversion from temporary to permanent contract during each single period)

	(1)	(2)	(3)	(4)
Control group: Men \geq 30 Treatment group: all eligibles Dependent variable: dummy for conversion	Main results over 2012 with covariates	Falsification over 2011 with covariates	Main results over 2012 with age [26,34)	Falsification over 2011 with age [26,34)
Eligible	-0.0017** (0.0008)	-0.0008 (0.0008)	-0.0004 (0.0015)	-0.0023 (0.0016)
Period II	-0.0129*** (0.0007)	-0.0126*** (0.0008)	-0.0140*** (0.0015)	-0.0156*** (0.0015)
Period III	-0.0009 (0.0009)	-0.0012 (0.0009)	-0.0008 (0.0019)	-0.0012 (0.0019)
Period IV	-0.0117*** (0.0008)	-0.0112*** (0.0008)	-0.0105*** (0.0017)	-0.0134*** (0.0016)
Eligible \times Period II	0.0014* (0.0008)	-0.0003 (0.0009)	0.0002 (0.0017)	0.0023 (0.0018)
Eligible \times Period III	0.0134*** (0.0012)	-0.0017 (0.0011)	0.0153*** (0.0024)	0.0003 (0.0022)
Eligible \times Period IV	0.0026*** (0.0009)	-0.0008 (0.0009)	0.0000 (0.0019)	0.0009 (0.0019)
Constant	0.0405 (0.0276)	0.0115*** (0.0041)	0.0193*** (0.0014)	0.0210*** (0.0014)
Observations	593,028	614,895	145,803	156,147

Note: * $p < .10$ ** $p < .05$ *** $p < .01$. Standard errors clustered for employer in brackets. Estimates are obtained using StataTM 13. See Figure 1 for the definition of periods. Covariates in columns (1)-(2) include dummies for sector of activities (ATECO 2 digits), dummies for educational level, dummy for Italian citizenship, age at the begin of the period, length of the job-relationship at the end of the period. Coefficients are available on request.

One may also criticize the use of treatment and control groups with large differences in terms of average age. Column (3) shows that our results are robust to selecting only individuals around the age cut-off. For this experiment, we make use of the interval [26,34). The estimated effect is larger in percentage points, but quite close to the baseline results if measured as a proportion of the

¹⁵ Younger men and women may also move across groups, if they turn old (according to our grouping) during the period of the analysis. Given the limited time span we focus on, this is not likely to be a major concern.

¹⁶ In case of missing for educational level or sector of activity, we kept the observation but we added a dummy for missing value.

counterfactual. Similar results are obtained by choosing even more stringent age limits (only individuals aged between 29 and 30).

We assumed that firms did not anticipate the policy during Period I, so that we could use it to consistently estimate the group effects. This is not necessarily true if firms were already aware of how the Government would have used the fund established by law 201/2011, which passed in December 2011. To test whether this was the case, we estimate a diff-in-diffs regression with the same control/treatment groups, but considering only two periods: Period I in 2012 and Period I in 2011. If eligible groups were affected by the scheme already in Period I of 2012, then we should find evidence of an effect when comparing it with the analogous period in 2011 (using men over 30 as a control). We estimate the following specification:

$$y_{it} = \gamma_0 + \gamma_E \mathbf{1}[\text{Eligible}]_{it} + \gamma_{2002} \mathbf{1}[\text{year 2012}]_{it} + \delta_I \mathbf{1}[\text{Eligible}]_{it} \times \mathbf{1}[\text{year 2012}]_{it} + \eta_{it} \quad (5)$$

We find that the coefficient on the interaction is not statistically (or economically) significant, being -0.03 percentage points. A similar result holds disaggregating by different groups, as in Table 1, Columns (2)-(4). Therefore there seems to be no evidence that firms anticipated the implementation of the program before its announcement. Similar diff-in-diffs results between Period III in 2012 and the analogous period in 2011 confirm the presence of an effect, although estimates are somewhat larger (around 1.8 percentage points for men under 30 and 1.3 for both groups of women).

Table 5 Test for a reduction in the control group

Dep. var.: dummy for conversion	Men \geq 30
Period II (in 2011)	-0.0121*** (0.0008)
Period III (in 2011)	-0.0003 (0.0009)
Period IV (in 2011)	-0.0097*** (0.0008)
Year 2012	-0.0007 (0.0009)
Year 2012 \times Period II	-0.0002 (0.0011)
Year 2012 \times Period III	0.0004 (0.0013)
Year 2012 \times Period IV	-0.0002 (0.0011)
Constant	0.0184*** (0.0007)
Observations	468,243

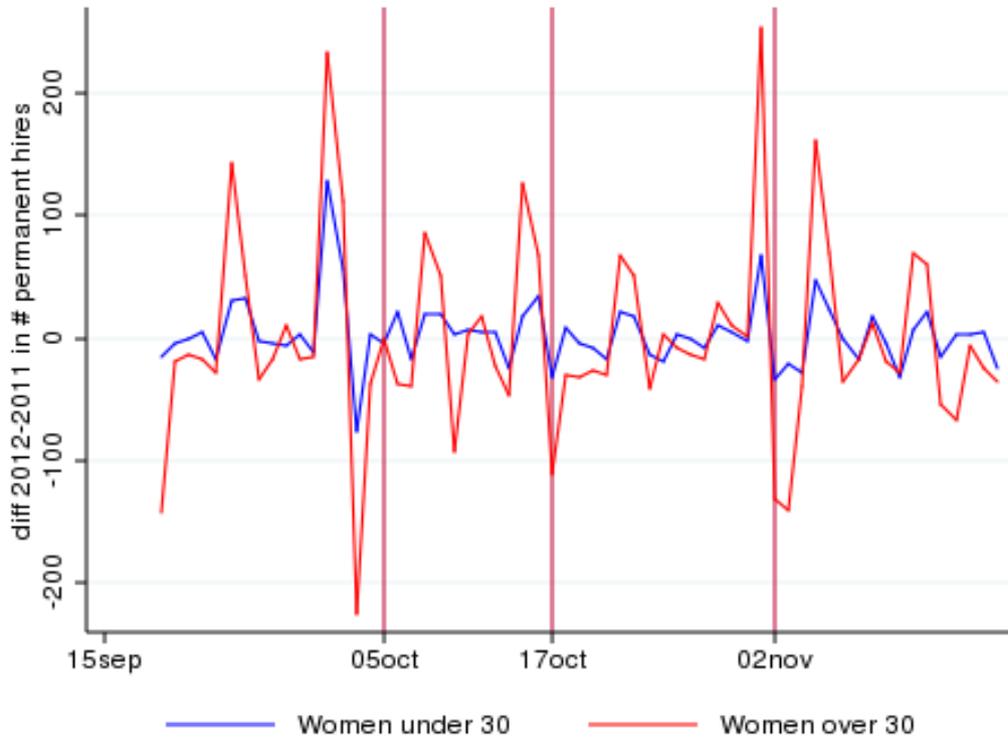
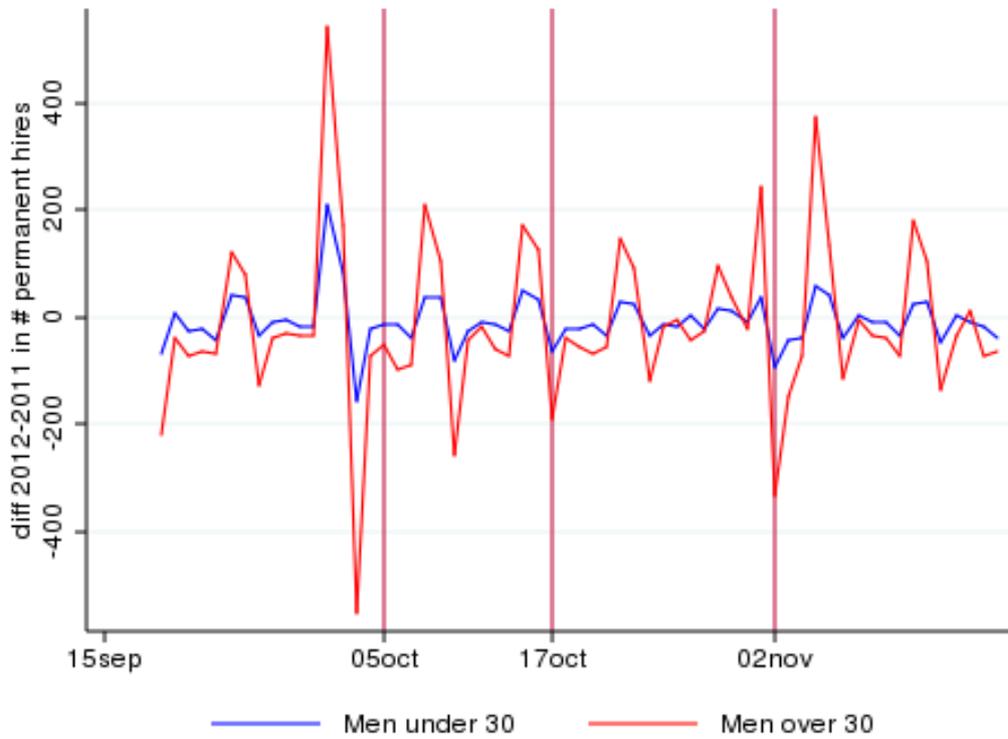
Note: * p<.10 ** p<.05 *** p<.01. Standard errors clustered for employer in brackets. Estimates are obtained using StataTM 13.

Besides anticipation/delay effects, which refer to the eligible groups, employers could have also substituted conversions for the control group in favor of those for the eligible workers. If this happened, we would expect to find the trend over the four periods for the control group to show a dip in Period III. Given that a similar dip could have been present also in 2011, we test whether the trend over periods I to IV was different in 2012 with respect to 2011. Results are reported in Table 5. A test for the interactions between the dummy for 2012 and the dummies for periods II-III-IV being jointly equal to zero fails to reject the null with p-value 0.9381. Therefore the evidence is not against the assumption that the control group has been (on average) unaffected by the policy.

Another possible substitution could take place between conversions (from fixed-term to permanent positions) and direct hires with a permanent contract. In Figure 4 we show the time series of the daily difference between the number of direct hires with a permanent contract in 2012 and in 2011, for different groups. There is no evidence of a change in the number of direct hiring during the period of validity of the policy.

Additional robustness tests have also been implemented. For instance, a possible difficulty is related with the circumstance that, for bureaucratic reasons, conversions are more likely at the turn of a month. In our case, however, the effect is not driven by conversions taking place at the end of October or begin of November: excluding the days in [30/10-02/11] we still find evidence of an impact for the eligible group. In terms of possible anticipation effects, we also worried that countervailing effects on the conversion rates of eligible workers can be found later than Period IV, which does not include the begin of a month. To check for this possibility, we added two subsequent periods of 16-day length, plugging therefore dates until December the 19th, and checked whether negative effects were in place. The results are reassuring. There is only a weak evidence of some small decrease for women aged less than 30 in period [04/12-19/12], with an estimate -0.26 percentage points marginally significant at the 5% level (p-value 0.050), but which is actually similar to the coefficient we found in a falsification on 2011 (-0.17 percentage points, p-value 0.243). Finally, the incentives for conversions could be cumulated with others available for hiring workers that have been previously dismissed through a particular procedure, called *mobilità*. We also replicated all our regressions by excluding these employees, again with no significant changes in the findings.

Figure 4 Difference in the number of direct hires with permanent contracts between 2012 and 2011, by day and group



6.3 Heterogeneity

The results documented so far for the groups of eligible workers might mask relevant heterogeneities. An important issue refers to the impact of the scheme across skill groups. For instance, a policy maker might want to know whether the program works for those who are less endowed with human capital, as their performance in the labor market is usually more problematic. In the following, for simplicity, we only focus on the effects that materialize in Period III.

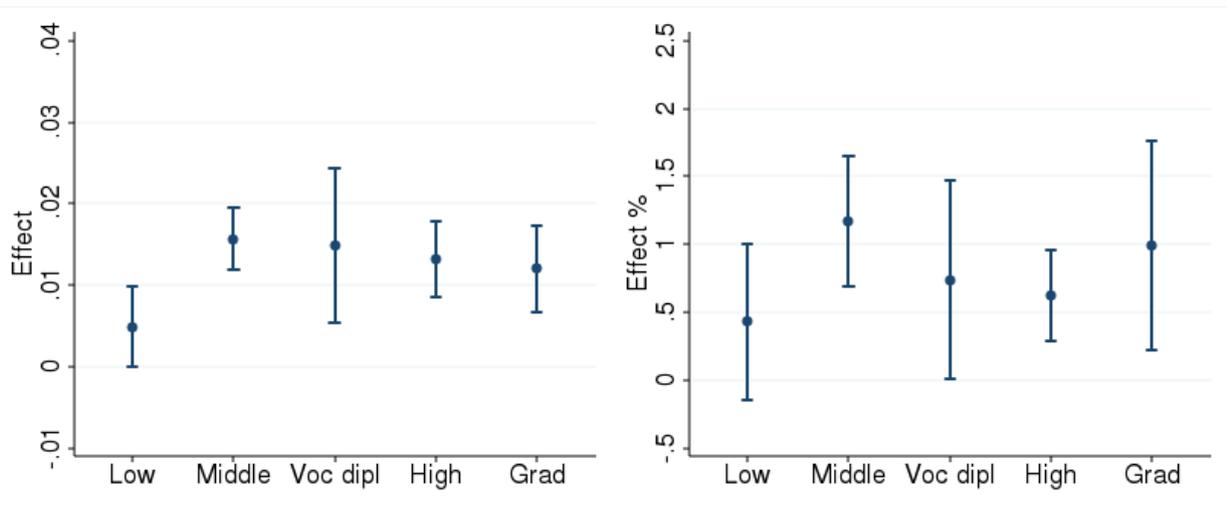
Figure 5 shows, for the eligible groups taken as a whole, the breakdown by educational level of the estimated effect (measured in percentage points, on the left, and as a proportion of the counterfactual rate, on the right).¹⁷ The impact of the scheme seems to be lower for those who have at most completed primary school. However, this is a relatively small group, accounting for only 10.7% of the observations for eligible workers in 2012. Differently, starting from middle school (8th grade) there is no evidence of strong heterogeneity: most of the effects are positive and there is no systematic increase associated with higher qualifications. Furthermore, the effects are all significantly different from zero at the 5% level. Similar conclusions can be taken by looking at the proportional effect, although it must be considered that these estimates are less precise due to the fact that even the baseline rate of conversion is estimated through the same model. Results obtained by breaking down the educational levels for each single group (young men, young women and older females) of eligible workers are qualitatively similar (and available on request). One potential concern is that the reduction in the number of observations, and in particular in the total number of observed conversions, for each cell of group \times period \times educational qualification makes estimates largely imprecise, hindering the ability to detect heterogeneity. To lessen this concern, we also estimated the average effects using only low qualifications (middle school or less) on the one hand, and high qualifications (high school or above) on the other.¹⁸ Results are only marginally modified: the 95% confidence interval for the effect in percentage point is [.0098; .0161] for low qualifications, which is very similar to that estimated for the other group, [.0089; .0163].¹⁹ Overall, it seems safe to conclude that the impact of the scheme was quite homogeneous across skill groups.

¹⁷ The information on the educational qualification is reported by the employer at the moment of the communication to the regional agency. There are 0,7% of the observations with a missing values. Given that for foreign citizen this information is likely to contain measurement error, we also reproduced the graph keeping only Italian citizen, but we found no qualitative differences. It must also be added that the falsification over 2011 fails to reject the null that interaction terms Eligible \times Period III in all educational group are jointly equal to zero with p-value 0.1461.

¹⁸ We did not consider vocational diploma, which are a particular case in between low and high qualification. Nevertheless, they involve only around 5.5% of the eligible observations, and the effect for them is similar to the one for high school graduates.

¹⁹ Given that the baseline rate of conversions is higher for the most educated, these results imply a smaller percentage increase for them, although we always fail to reject the null that the proportional effect is different (with high p-values).

Figure 5 Dif-in-dif by educational level (effect on the rate of conversion for the entire eligible group during period III, in percentage points on the left and as a proportion of the counterfactual rate on the right; 95% confidence interval with s.e. clustered by employer)



Note: “Low” stands for workers who completed at most primary school, “Middle” is the 8th grade, “Voc Dipl” is a vocational diploma that last two or three years after middle school, “High” is for high school, “Grad” stands for university graduates.

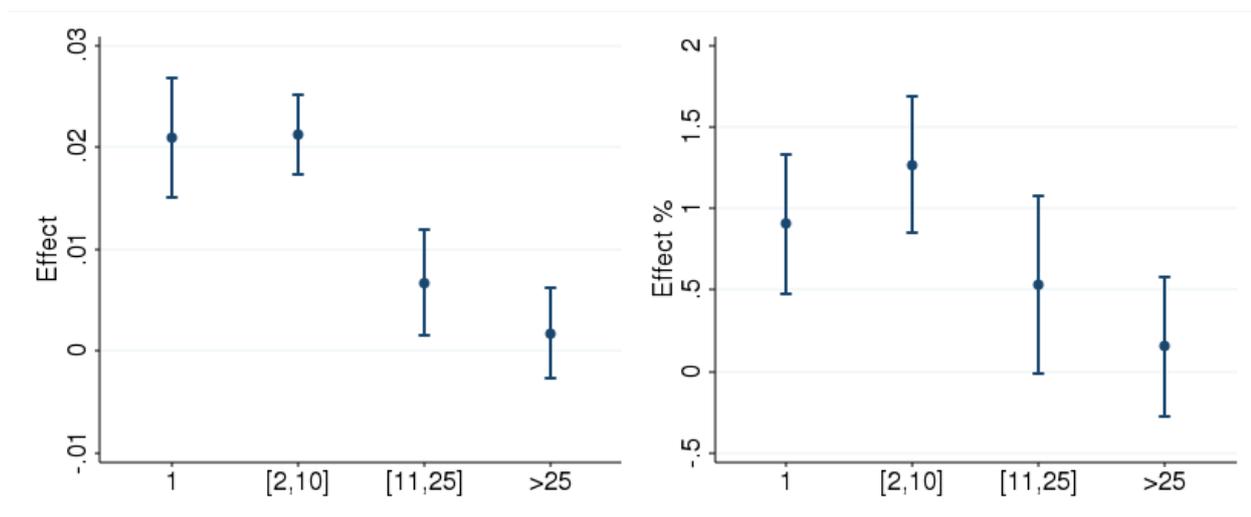
It is also interesting to check to what extent the scheme have impacted on firms with different compositions of their workforce in terms of contractual agreements. For instance, also because of the specific features of their production, some firms might be less interested in signing permanent ones no matter what incentives they could receive. On the other hand, other firms might use short-term positions as a temporary step to bring their workers into the permanent pool. In this latter case, at any point in time, the firms will have many open-end position and only few fixed-term appointments (which will later expire or be converted). Figure 6 breaks down the impact of the program by the number of temporary contracts referring to the same employer (again, measured both in percentage points and as a proportion of the baseline rate of conversion).²⁰ The evidence suggests that the effect is smaller for workers whose employers hold a larger number of temporary contracts.²¹ A statistical test for the equality of the effects across the different categories of firms rejects the null at the 5% level, considering both the effects in percentage points (p-value 0.000) and as a proportion of the baseline (p-value 0.002). One important caveat is that the scheme has a limit

²⁰ Running a falsification over 2011 fails to reject the null that interaction terms Eligible × Period III in all number of temporary contract categories are jointly equal to zero with p-value 0.5872

²¹ As a side-line, results for firms with only one temporary contract are an additional robustness check, given that employers could not substitute between eligible and not eligible workers.

of 10 incentives per employer (see Section 3), which constrained the possibilities of conversion for workers with many colleagues holding a temporary contract.²²

Figure 6 Dif-in-dif by number of temporary contracts held by the employer (effect on the rate of conversion for the entire eligible group during period III, in percentage points on the left and as a proportion of the counterfactual rate on the right; 95% confidence interval with s.e. clustered by employer)



6.4 Effects on permanent employment

Finally, we double-check our findings by looking at the effects of the program on permanent employment measured up to 8 months after the end of Period III. The magnitude of the financial incentives could have induced firms to convert short-term positions into permanent contracts only to obtain the transfer, and later dismiss the worker as soon as the six months duration requirement was met. Although the possibility of this strategic behavior is constrained by the presence of higher EPL for permanent workers, many subsidized conversions occurred in small enterprises, for which EPL is lower.

Our test is the following. We switch our focus from job-relationships to individuals, and keep only those who held a standard temporary contract in Period III. Next, we see whether, among this sample, those eligible for the incentives were more likely to be in a permanent position (not necessarily with the same employers, as they may access permanent employment also by applying with other employers) during the following months; we use February, April and June 2013 (the last date of availability of our data). We implement once more a diff-in-diffs approach and use as pre-

²² We also split the sample according to the number of *eligible* temporary workers. We found that the impact is positive and statistically significant only for employers with at most 10 eligible workers; for firms having more than 10 eligible workers the impact remains positive (though smaller) and not statistically significant at the 5% level.

policy counterfactuals the individuals who held temporary contracts in the 2011 period analogous to Period III, for which their employment status is measured during the first semester of 2012. Formally, we estimate a diff-in-diffs regression of the type:

$$y_{it} = \lambda_0 + \lambda_E \mathbf{1}[\text{Eligible}]_{it} + \lambda_{2002} \mathbf{1}[\text{year 2012}]_{it} + \theta \mathbf{1}[\text{Eligible}]_{it} \times \mathbf{1}[\text{year 2012}]_{it} + \mu_{it} \quad (6)$$

Table 6 describes the results. In the first panel, Column (1) describes the estimates of the probability of conversion during Period III. Columns (2), (3) and (4) provides the estimates of the chance of being in permanent employment in the following months. Our evidence suggests a significant increase in the probability of conversion (at the individual level) in Period III (the magnitude of the effect is larger, but compatible with the results where the unit of observation is the job-relationship). We also find positive and statistically effects as for the likelihood of being permanent in February, April and June 2013. Note that the point estimates for the impact on permanent employment later in time are smaller than the increase in conversion probability in Period III. Although the difference is statistically significant at the 5% level only in one case (April), this finding could signal that a fraction of the subsidized conversions went to individuals who would have accessed permanent employment even in the absence of the incentives.

However, we have to make sure that the eligible group does not show a diverging trend in the probability of accessing permanent employment during the first semester of 2013. In the second panel of Table 6, we run a falsification exercise, by focusing on individuals holding a temporary contract in the following month, between 17/11 and 2/12. As we have already discussed, in this period incentives were not available anymore. According to our evidence of no substitution over time we expect these individuals to be unaffected by the policy. Indeed, column (1) of the second panel in Table 6 shows that there is no effect on the conversion rate of eligible individuals in this period. We can therefore use this group to infer whether eligible workers would have shown a diverging trend in the absence of the incentives. If there is evidence of a downward trend (with respect to men over 30) in the probability of getting a permanent job, we would be underestimating the effects of the incentives. In columns (2)-(4), second panel, we find that the probability of being in permanent employment later in time (in April and June) decreases by around 0.8-0.9 percentage points for eligible workers. Note that this gap is similar to the difference between the effect on conversion and that on permanent employment documented in the first panel.

Table 6 Regressions for the probability of being in permanent employment some months later; individuals holding a temporary contract in a specific period

	DEPENDENT VARIABLE			
	(1)	(2)	(3)	(4)
Control group: Men \geq 30 Treatment group: all eligibles	Dummy for contract conversion in the initial period	Dummy for permanent employment on the 15th of February	Dummy for permanent employment on the 15th of April	Dummy for permanent employment on the 15th of June
Individuals with a temporary contract in period III (in 2011 or 2012)				
Eligible	-0.0037*** (0.0009)	-0.0367*** (0.0029)	-0.0420*** (0.0034)	-0.0498*** (0.0037)
Year 2012	-0.0003 (0.0010)	0.0036 (0.0030)	0.0017 (0.0033)	-0.0042 (0.0034)
Eligible \times Year 2012	0.0152*** (0.0013)	0.0110*** (0.0035)	0.0078** (0.0038)	0.0089** (0.0040)
Constant	0.0187*** (0.0007)	0.1701*** (0.0025)	0.2103*** (0.0029)	0.2425*** (0.0032)
Observations	284,012	284,012	284,012	284,012
Test for equality of interaction term with column (1)		0.2075	0.0428	0.1027
Falsification using period [17/11 - 2/12] (in 2011 or 2012)				
Eligible	-0.0040*** (0.0008)	-0.0342*** (0.0029)	-0.0402*** (0.0034)	-0.0492*** (0.0038)
Year 2012	-0.0001 (0.0009)	0.0092*** (0.0033)	0.0083** (0.0036)	0.0023 (0.0037)
Eligible \times Year 2012	0.0003 (0.0011)	-0.0056 (0.0037)	-0.0091** (0.0041)	-0.0078* (0.0044)
Constant	0.0157*** (0.0006)	0.1609*** (0.0025)	0.2074*** (0.0028)	0.2460*** (0.0033)
Observations	244,950	244,950	244,950	244,950
Test for equality of interaction term with column (1)		0.1028	0.0199	0.0592
Triple difference				
Eligible	-0.0040*** (0.0008)	-0.0342*** (0.0029)	-0.0402*** (0.0034)	-0.0492*** (0.0038)
Year 2012	-0.0001 (0.0009)	0.0092*** (0.0033)	0.0083** (0.0036)	0.0023 (0.0037)
Eligible \times Year 2012	0.0003 (0.0011)	-0.0056 (0.0037)	-0.0091** (0.0041)	-0.0078* (0.0044)
Period III	0.0030*** (0.0009)	0.0092*** (0.0012)	0.0029** (0.0013)	-0.0035** (0.0014)
Period III \times Eligible	0.0003 (0.0011)	-0.0025* (0.0014)	-0.0018 (0.0015)	-0.0006 (0.0016)
Period III \times Year 2012	-0.0002 (0.0013)	-0.0056*** (0.0014)	-0.0067*** (0.0014)	-0.0065*** (0.0015)
Period III \times Eligible \times Year 2012	0.0149*** (0.0017)	0.0166*** (0.0017)	0.0169*** (0.0018)	0.0166*** (0.0018)
Constant	0.0157*** (0.0006)	0.1609*** (0.0025)	0.2074*** (0.0028)	0.2460*** (0.0033)
Observations	528,962	528,962	528,962	528,962
Test for equality of triple interaction term with col (1)		0.3105	0.2787	0.3631

Note: * $p < .10$ ** $p < .05$ *** $p < .01$. Standard errors clustered for employer in brackets. Estimates are obtained using StataTM 13.

Therefore, we find evidence of a diverging negative trend for the eligible group. In order to correct for it, the third panel runs a triple difference regression, which is simply equivalent of subtracting panel 2 from panel 1. The effect of the policy is now captured by the coefficient on the triple difference $\text{Period III} \times \text{Eligible} \times \text{Year 2012}$. The point estimates for employment are now quite stable over time and comparable with the conversion rate, although slightly larger.

We also did an additional check. We replicated the second and third panel of Table 6 by removing, from the group of individuals holding a fixed-term contract between 17/11 and 2/12, those hired by firms who had previously done a conversion in period III. This should minimize the risk of substitution over time and across workers, because it excludes those employers who could have anticipated strategically conversions during period III. Results, available on request, are in line with those presented here.

All in all, our findings suggest that – when measured 8 months after the scheme was over – the impact on permanent employment is still there. More in general, it shows that those individuals who benefited from the increased conversion rate would not have found a permanent job in the absence of the policy. Nevertheless, part of the stability of the effect over time may be due to the fact that the incentives were distributed only under the condition that the contract lasted at least six months after the conversion.

7. Conclusions

Our exercise suggests that the program introduced with the Decree 5 Oct 2012 was effective in stimulating conversions. Compared to the counterfactual scenario, conversions increased by 83%. The additional permanent positions came with a cost: to get one extra-permanent job the government had to finance additional 1.2 conversions that would have taken place even with no public support.

No need to say, the external validity of program evaluation experiments is quite low. Thus, it is not safe to infer from our results policy implications of general validity as for the effectiveness of conversion programs. Having said so, a number of remarks are in order.

First, the scheme we evaluated shows little sign of perverse conducts on the employer side. There is no evidence of strategic behavior, intended to bring conversions forward or backward only to benefit from the scheme. This circumstance might well be explained by the fact that the span of time between announcement and beginning of the program was small and the shortage of financing might have come before than expected (by the employers).

Secondly, it is very difficult to say whether the amount of the subsidy was appropriate to the aim of converting the maximum amount possible of short-term positions. A higher financial support

could have spurred additional conversions; at the same time, it would have made even larger the financial dead-weight loss associated with the conversions that would have been done even without the scheme. Therefore, within the budget constraint envisaged by Decree 5 Oct 2012, increasing the subsidy might not necessarily help with converting more contracts. If one believes that employers' demand for conversions was fully fulfilled at that amount of the subsidy, then it is not unrealistic that a smaller amount of money would have reached a similar effectiveness.

Finally, it might be useful to compare the program with two different schemes of incentives aimed at promoting permanent employment, which were introduced in Italy at different points in time. In year 2000, Law 388 established a tax-credit (of 413 euro per month; 620 in the South of Italy) for each unit increase in the number of permanent workers aged 25 or more with respect to the figure reported by the firm for the pre-policy year. The scheme also required the employers to increase the overall workforce. Essentially, employers could access the incentive by hiring a new worker with a permanent contract, or by converting a fixed-term one but simultaneously hiring a new temporary (or permanent) worker. For this scheme, Cipollone and Guelfi (2006) find no sign of overall effectiveness, as for the probability that individuals enter into permanent employment. However, they find a positive effect for those previously employed with a temporary training contract, suggesting that these incentives may be more likely to have an impact on conversions rather than on new hires. Their estimates were also positive for the unemployed with previous work experience and for more educated workers, while basically zero for those without a high school diploma.

A more recent measure was introduced by a law-decree in June 2013 (the so called "*decreto lavoro*"), which aimed at improving employability of young individuals within the framework of the Youth Guarantee. Starting from August 2013, employers who hired with permanent contracts individuals aged 18-29, either unemployed for at least 6 months or without a high school or vocational diploma, could receive an incentive of 1/3 of the gross salary (with a cap at 650 euro) for 18 months. The same applied for conversions from fixed term to permanent contracts, although the duration of the subsidy was limited to 12 months. In both cases, the employer had to increase his/her overall workforce, similarly to the 2001 policy. As reported by the Ministry of Labor on the 18th of December 2013 the number of applications for this scheme was around 18,000, a relatively small number with respect to the potential amount of incentives allowed by the available funding (around 100,000).

One important difference between these two programs and the one we studied is that they required the employer to increase the workforce. This might be an important reason why the Decree 5 October 2012 seems to have had a larger impact. Clearly, whether a policy maker should or not

impose this additional constraint also depends on the final target of the scheme, which may aim at increasing overall employment and not only the rate of conversion.

Another important difference is that the Decree 5 October 2012 did not finance direct hires with a permanent contract, but only conversions. As argued by Cipollone and Guelfi (2003, 2006), their finding of heterogeneous effects of law 388/2000 can be explained by the fact that employers prefer to hire permanently only individuals with a stronger signal of higher productivity, in particular the more educated and those with previous work experience. Differently from them we find an aggregate positive effect and no significant differences according to the educational level of the worker. This suggests that the incentives for conversions may be more effective, generating less dead-weight loss, because they exploit the stepping-stone effect of fixed-term contracts.

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Online Appendix: additional results

(Not for publication)

Table A1 Number of observations per period and group

	Year 2011				Year 2012			
	Men < 30	Men ≥ 30	Women < 30	Women ≥ 30	Men < 30	Men ≥ 30	Women < 30	Women ≥ 30
Period I	29,652	66,650	21,327	56,612	27,971	64,400	21,633	55,391
Period II	24,958	59,035	18,087	49,263	23,455	57,129	18,297	47,992
Period III	24,588	58,517	18,154	48,688	22,981	56,569	18,341	47,342
Period IV	22,519	54,295	17,153	45,397	20,310	51,648	16,863	42,706

Table A2 Descriptive statistics, year 2012 (periods I to IV pooled)

	Men ≥ 30			Eligible		
	mean	min	max	mean	min	max
<i>Education</i>						
Primary school	.1482202	0	1	.1065426	0	1
Middle school	.4702585	0	1	.3224575	0	1
Vocational diploma	.0534503	0	1	.0549078	0	1
High school	.2208395	0	1	.335775	0	1
Graduate	.099166	0	1	.1755606	0	1
Educ. missing	.0080654	0	1	.0047566	0	1
<i>Economic activity of the firm</i>						
Agriculture	.1639506	0	1	.1457353	0	1
Manufacture	.2343632	0	1	.171082	0	1
Construction	.1192534	0	1	.0273617	0	1
Hotel and restaurants	.1965649	0	1	.2862817	0	1
Services	.2858679	0	1	.3695394	0	1
Italian citizen	.6914157	0	1	.7513722	0	1
Full time	.8343431	0	1	.6281319	0	1
Age (years and months)	42.79326	30.0	64.91666	34.17559	16	64.91666
Duration of job relationship (months)	.4771345	0	5	.4231577	0	5
<i>Number of fixed term employees in the firm</i>						
One	.1883907	0	1	.1621468	0	1
[2,10]	.4568132	0	1	.3785186	0	1
[11,25]	.1673544	0	1	.1759983	0	1
25+	.1874418	0	1	.2833364	0	1
Observations	229,746			363,282		

Table A3 Additional robustness checks (probability of conversion from temporary to permanent contract during each single period in 2012)

	(1)	(2)	(3)	(4)	(5)
		Excluding those in the economic sectors of Public Administration and Health	Excluding those with a national contract	Excluding those with contract duration larger than 3 years	Age defined at the end of the period
Control group: Men \geq 30					
Treatment group: all eligibles					
Dependent variable: dummy for conversion					
Eligible	-0.0029*** (0.0006)	-0.0018** (0.0008)	-0.0015* (0.0008)	-0.0023*** (0.0007)	-0.0021*** (0.0007)
Period II	-0.0109*** (0.0007)	-0.0127*** (0.0008)	-0.0128*** (0.0008)	-0.0123*** (0.0007)	-0.0123*** (0.0007)
Period III	-0.0001 (0.0008)	0.0004 (0.0010)	0.0003 (0.0010)	0.0001 (0.0009)	0.0003 (0.0009)
Period IV	-0.0090*** (0.0007)	-0.0101*** (0.0008)	-0.0102*** (0.0008)	-0.0099*** (0.0008)	-0.0099*** (0.0008)
Eligible \times Period II	0.0018*** (0.0007)	0.0011 (0.0009)	0.0007 (0.0009)	0.0013 (0.0008)	0.0011 (0.0008)
Eligible \times Period III	0.0098*** (0.0010)	0.0144*** (0.0014)	0.0147*** (0.0013)	0.0132*** (0.0012)	0.0128*** (0.0012)
Eligible \times Period IV	0.0023*** (0.0007)	0.0022** (0.0010)	0.0019** (0.0010)	0.0021** (0.0009)	0.0020** (0.0009)
Constant	0.0155*** (0.0007)	0.0184*** (0.0007)	0.0183*** (0.0007)	0.0177*** (0.0007)	0.0177*** (0.0007)
Observations	778,568	519,196	550,489	589,490	592,967
Effect / counterfactual (period III)	0.7813	0.8445	0.8616	0.8530	0.8084

Note: * p<.10 ** p<.05 *** p<.01. Standard errors clustered for employer in brackets. Estimates are obtained using StataTM 13. See Figure 1 for the definition of periods.

Table A4 Additional robustness checks (continued; probability of conversion from temporary to permanent contract during each single period in 2012)

	(1)	(2)	(3)	(4)
Control group: Men \geq 30 Treatment group: all eligibles Dependent variable: dummy for conversion	S.e. clustered at the economic activity (21 categories) classification	Excluding [31/10 – 2/11]	Excluding those in “mobilità”	Only individuals aged [29, 30]
Eligible	-0.0021 (0.0020)	-0.0021*** (0.0007)	-0.0011 (0.0007)	-0.0016 (0.0033)
Period II	-0.0123*** (0.0031)	-0.0123*** (0.0007)	-0.0113*** (0.0007)	-0.0170*** (0.0032)
Period III	0.0001 (0.0018)	-0.0103*** (0.0008)	-0.0003 (0.0009)	-0.0023 (0.0042)
Period IV	-0.0099*** (0.0033)	-0.0105*** (0.0008)	-0.0094*** (0.0008)	-0.0163*** (0.0033)
Eligible \times Period II	0.0011 (0.0016)	0.0011 (0.0008)	0.0005 (0.0008)	0.0009 (0.0037)
Eligible \times Period III	0.0130*** (0.0039)	0.0091*** (0.0010)	0.0124*** (0.0012)	0.0167*** (0.0050)
Eligible \times Period IV	0.0020 (0.0016)	0.0016* (0.0009)	0.0018* (0.0009)	0.0037 (0.0039)
Constant	0.0177*** (0.0043)	0.0177*** (0.0007)	0.0156*** (0.0007)	0.0228*** (0.0028)
Observations	593,028	590,575	526,396	35,750
Effect / counterfactual (period III)	0.8284	1.7176	0.8691	0.8841

Note: * p<.10 ** p<.05 *** p<.01. Standard errors clustered for employer in brackets. Estimates are obtained using Stata™ 13. See Figure 1 for the definition of periods.

Table A5 Replication of Table 6, but removing from the falsification those firms who converted a contract during period III

	DEPENDENT VARIABLE			
	(1)	(2)	(3)	(4)
Control group: Men \geq 30 Treatment group: all eligibles	Dummy for contract conversion in the initial period	Dummy for permanent employment on the 15th of February	the following: April	June
Falsification using period [17/11 - 2/12] (in 2011 or 2012)				
Eligible	-0.0039*** (0.0008)	-0.0352*** (0.0029)	-0.0416*** (0.0035)	-0.0519*** (0.0038)
Year 2012	-0.0005 (0.0009)	0.0112*** (0.0037)	0.0098** (0.0041)	0.0024 (0.0043)
Eligible \times Year 2012	0.0009 (0.0011)	-0.0064 (0.0039)	-0.0096** (0.0042)	-0.0067 (0.0044)
Constant	0.0145*** (0.0006)	0.1561*** (0.0023)	0.2012*** (0.0026)	0.2394*** (0.0029)
Observations	218,034	218,034	218,034	218,034
Test for equality of interaction term with column (1)		0.0542	0.0118	0.0803
Triple difference				
Eligible	-0.0039*** (0.0008)	-0.0352*** (0.0029)	-0.0416*** (0.0035)	-0.0519*** (0.0038)
Year 2012	-0.0005 (0.0009)	0.0112*** (0.0037)	0.0098** (0.0041)	0.0024 (0.0043)
Eligible \times Year 2012	0.0009 (0.0011)	-0.0064 (0.0039)	-0.0096** (0.0042)	-0.0067 (0.0044)
Period III	0.0042*** (0.0009)	0.0140*** (0.0017)	0.0092*** (0.0019)	0.0031 (0.0021)
Period III \times Eligible	0.0002 (0.0011)	-0.0016 (0.0019)	-0.0004 (0.0021)	0.0021 (0.0022)
Period III \times Year 2012	0.0002 (0.0013)	-0.0076*** (0.0022)	-0.0082*** (0.0025)	-0.0066** (0.0028)
Period III \times Eligible \times Year 2012	0.0143*** (0.0017)	0.0174*** (0.0024)	0.0173*** (0.0026)	0.0156*** (0.0028)
Constant	0.0145*** (0.0006)	0.1561*** (0.0023)	0.2012*** (0.0026)	0.2394*** (0.0029)
Observations	502,046	502,046	502,046	502,046
Test for equality of triple interaction term with col (1)		0.1761	0.2253	0.6516

Note: * p<.10 ** p<.05 *** p<.01. Standard errors clustered for employer in brackets. Estimates are obtained using StataTM 13.