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

PRELIMINARY VERSION – DO NOT CITE

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Abstract

We evaluate the impact of an Italian scheme (Ministerial Decree 5 October 2012) that provided monetary incentives to firms willing to convert fixed-term labour contracts into permanent ones. The policy applied to specific demographic groups (females, younger men) and, due to binding funding constraints, was in place for a short period of time. Therefore, it allows us to study to what extent the effect of the scheme is due to substitutability across demographic groups and (within-group) over time. Our exercise focuses on the region of Veneto, for which we have a high-quality administrative dataset made available by Veneto Lavoro, and is based on a difference-in-differences identification strategy. We show that the subsidy increased conversions by 83% on average, with no evidence of substitution effects over time or across groups of workers. Results show no significant differences across educational levels.

JEL: J21, J41, J48

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1. Introduction

In the process of reforming the labour market institutions, a government faces a trade-off between the need of increasing labour market flexibility and the desire to guarantee some level of stability to individual workers. On the one hand, the use of fixed term contracts generates efficiency gains. Apart from technological reasons (buffer stock, temporary substitutions, seasonal jobs; see Cappellari, 2012), employers can use them to increase employees' productivity, both by inducing them to exert more effort (Engellandt and Riphahn, 2005), and by screening workers before a permanent hire in order to avoid mismatches.[1] On the other hand, a widespread use of temporary contracts may increase job-insecurity, therefore affecting their welfare and their choices. For instance, empirical evidence suggests that it may have negative effects on youth emancipation (Becker et al, 2010) and on fertility (Modena and Sabatini, 2012). Furthermore, 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.[2] Clearly, these disadvantages are likely to be more problematic if individuals experience a series of temporary jobs without access to a permanent one.

The government may wish to regulate the use of fixed-term contracts in order to maximise the difference between the advantages of more flexibility and the costs of job-insecurity. In doing so it faces two constraints. First of all, temporary workers are generally less expensive for firms, both because of smaller social contributions and because of lower employment protection legislation (Grassi, 2009). Secondly, it can be politically unfeasible to reduce the cost of permanent contracts.

One possible way to increase the proportion of stable jobs would be to subsidise direct hires with a permanent contract, or to increase the cost of the fixed-term ones. Both are likely to reduce the inflow of temporary workers and increase the one of long-term employees. However, these mechanisms also reduce the efficiency gains related to the use of flexible contracts. A different solution would be to subsidise employers for conversions from fixed-term to open-end contracts. This allows them to freely hire temporary workers, possibly generating the efficiency gains related to greater flexibility, but at the same time it reduces the risk that individuals incur in a series of fixed-term contracts.

The effectiveness of these policies is nevertheless an empirical question: whether employers increase their use of permanent workers depends on the extent to which they take advantage of the

[1] 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 have preferences for non-standard careers and therefore are less interested in investing into job-specific human capital (Booth et al., 2002).
[2] This may affect innovation activity; see Franceschi and Mariani (2013) for a discussion.

greater flexibility of temporary contracts and to the expected stream of profits associated to the single worker. The available evidence is rather scarce and mostly refers to policies that either increased the cost of fixed term contracts or made it cheaper to hire on a permanent basis. Mendez (2013) evaluated two reforms in Spain during the 90s, which restricted the use of fixed-term contracts (1994) and reduced the cost of open-end ones (1997), but he found no increase in the conversion rate. Maurin and Michaud (2004) focused on France, where a reform in 2002 increased the cost of terminating a temporary contract instead of converting it into a permanent one. Although this should work similarly to a subsidy for a conversion, it also implies a direct cost in hiring individuals with a fixed term contract. Indeed, the authors provided evidence of an increase in the proportion of temporary contracts converted into permanent ones, but they also found that the higher costs induced a decrease in the number of new fixed-term hires and therefore a contraction in the overall employment.

In this paper we provide evidence from Italy about a policy that introduced a subsidy to employers for the conversion from temporary (TD from now on) to open-end (TI) 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, funds were limited and finished in a couple of weeks. This design created the conditions to evaluate its effects through a dif-in-dif strategy over a very short period of time.

Using aggregate time series from the Veneto region, Anastasia et al (2013) showed that for the treated 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, in a regression discontinuity design fashion. Differently from them, here we directly use the micro data built from the administrative archives of the same region. Given that this dataset 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 a dif-in-dif strategy. To the best of our knowledge, this is the first paper providing clean evidence on the causal effect of incentives for fixed-term contract transformations using administrative microdata from Italy.[3]

Our estimates show that the policy increased the probability of transformation by 83% on average 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 announcement and full implementation, nor that they reduced the conversion rate after the funds were terminated. These results are robust to

[3] The only paper we are aware of that focused on conversion rates is Grassi (2009), which studied the effect of employment protection legislation on conversion rates, exploiting the introduction of higher dismissal costs for small businesses.

several checks, including a falsification exercise. We also show that there is no evidence of larger effects for more educated workers, differently from what was found for a different scheme of incentives introduced in 2001 (Cipollone and Guelfi, 2003, 2006).

Section 2 briefly summarizes the literature on fixed-term contracts in Italy and discusses two other Italian policies that introduced different incentives aimed at promoting permanent employment. Section 3 outlines the policy. Section 4 presents the data, while Section 5 describes the identification strategy. Section 6 discusses the results, while the last section concludes.

2. Literature and other policies

The empirical literature on fixed-term contracts in Italy mainly focused on the *stepping stone* effect (Booth et al, 2012), that is on their ability of promoting the access to permanent employment. Papers in this stream of research focused on the probability that individuals with a fixed-term contract at a certain point in time are employed on a permanent basis later on, and compared them with individuals who were instead unemployed. Ichino et al. (2005) found a positive effect of temporary contracts operated through an agency (*lavoro interinale*) in the region of Tuscany and, with some caveats, in Sicily. Picchio (2005) provides evidence that standard fixed-term contracts (*tempo determinato*) increased the probability of being in permanent employment two years later, while Barbieri and Sestito (2008) estimated a significant effect on the likelihood of finding a "satisfactory" job within one year. Still focusing on the comparison with the unemployed, Berton et al. (2011) confirmed previous results, but found stronger effects for training contracts (*formazione-lavoro*), followed by standard fixed-term ones, by apprenticeships contracts and finally by non-standard contracts which are not formally regulated as self-employment ones, although they are signed with a single employer (*para-subordinati*). Bruno et al. (2012) provided additional estimates of the stepping stone effect focusing on a more recent period. Differently from this stream of literature, here we analyse how a subsidy can affect the rate of conversion of temporary contracts into permanent ones.

Other two different schemes of incentives aimed at promoting permanent employment were introduced at different points in time. The evaluation of Decree 5 October 2012 and the comparison with them can thus provide useful insights about how incentives for permanent contracts influence firms' hiring decisions.

Firstly, law 388 in year 2000 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 average reported for the pre-policy year. The scheme also required them 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.[4] In both cases the new hiring could not be simply a substitute for another employee ceasing his/her contract (for retirement, firing, or any other reason), because the overall number of employees had to increase.[5] The tax-credit was quite extended in time, as it was supposed to last until the end of 2003, and it was later extended until 31/12/2006 by law 289/2002. This policy was evaluated by Cipollone and Guelfi (2003, 2006), who estimated its impact on the probability that individuals entered into permanent employment, using longitudinal data from the Labour Force Survey. Although they found no aggregate effect, they provided evidence of a positive effect for more educated workers, in particular university graduates. This is coherent with a model where individual productivity is unknown to the employer, who therefore prefers to hire permanently only individuals with a stronger signal of higher productivity. They also found that the effect of the incentives on the probability of getting a permanent job was higher for those who were employed with a temporary training contract in the previous year (not necessarily with the same employer), and for those who were unemployed but had previous work experience. Differently from them, we evaluate incentives directed only towards the conversion of a contract held with the same employer. It must be added that our data are better suited to analyse this kind of transition, because we can follow temporary contracts across different periods within the same year, while they only observe year to year transitions. Furthermore, we have access to a larger sample, as we have 593,486 observations on temporary contracts over four periods in 2012 (approximately 148,400 period) for which we can observe conversions into permanent employment, while Cipollone and Guelfi (2006) could observe transitions into open-end contract for a total of 76,854 observations over 9 years (nearly 8,500 per year), including not only individuals who were previously in temporary positions but also those who were not employed.

Secondly, 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.[6] The same applied for conversions from fixed term to permanent

[4] To be eligible, individuals hired with (or converted to) the permanent contracts should not had held another open-end contract in the previous 12 months.

[5] The actual calculation of the increase in employment depended also on the length of the contract, because temporary contracts lasting less than 12 months counted also as a fraction of the entire year. Therefore it was formally possible to increase employment by simply transforming a 6 month temporary contract (counting 6/12) into a permanent one (12/12).

[6] Firms were allowed to present demand for the incentives starting from October 2013, but they were allowed to request incentives for contracts signed back to August.

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. This policy has received great attention from the media, but at the time of writing the scheme does not seem to have attracted a strong reaction from employers, as measured from number of applications.

3. The policy

The Decree 5 October 2012 introduced substantial incentives for employers who

- Converted temporary contracts (TD) for eligible workers into permanent ones (TI); the incentive in this case was equal to 12 thousand euro in total.
- Stabilised 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 was valued 12,000.
- 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 depending on the length of the contract.

The incentives were reduced for part-time workers. The scheme also required that the job lasted for at least 6 months after the conversion/hire. Eligible workers were men aged less than 30, and women of whatever age. It should be noted that the key difference with the previous 2000 policy and with the more recent one is that conversions did not require an increase in the total workforce.

The decree made use of a dedicated national fund for the increase in the employment of youth and women. This 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 was approved. We therefore take the latter as the date of announcement, although we will also test whether results depend on assuming that the period before was not affected by the policy. Figure 1 depicts the timeline of the policy.

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The incentive applied only to conversions/hires starting from the official date of publication of the decree, the 17th of October, to March 31. Given that funds were limited, employers were allowed to check online whether the number of requests made until then was already sufficient to exhaust the total sum. At the moment of the formal request, however, they should had already signed and communicated the new contract with the eligible worker, but they could not make completely sure that they would have got the incentive: if on the same day the funds terminated, demands would have been approved on a first come first served basis. Indeed, on the 2nd of November the National Institute for Social Security (INPS) declared that the number of requests that had been presented until then were enough to terminate the funds, and therefore discouraged new requests. Note that, despite of INPS communication, some requests could have arrived even later, because nothing guaranteed that all the demands already presented were actually eligible.

The only publicly available data about the incentives come from the website of the Ministry of Labour: around 44.054 requests were made, of which only 24.581 were accepted.[7] The decision about acceptance was communicated only in June 2013, to comply with the request that the job had lasted at least 6 months. Given that the rules were relatively simple, it is likely that the selection of requests was mostly based on the order of presentation, rather than on their eligibility. There is, therefore, a significant fraction of employers who converted/hired a contract after the 2nd of November and asked for the incentive, given that the application was basically zero, but were excluded from it. We do not have any information on whether they were aware that their request was actually a bet, and whether they correctly predicted the chance of winning it.

Finally, among the few data released by the Ministry of Labour we know that around 90% of all incentives were distributed for conversions or stabilisations of temporary contracts. Considering that the incentive for direct hires with temporary contracts was much smaller (and required an increase in the overall workforce), it is not surprising that few requests were made for that option. It should also be added that stabilisations of *parasubordinati* usually involve a much smaller number of workers. For these reasons in this paper we focus on temporary contract conversions only.

4. Data

Since March 2008 employers who hire new workers or who make alterations to pre-existing contracts are obliged to communicate it to a regional agency through an online system.[8] This administrative archive does not allow creating a database with the complete stock of workers, given that permanent contracts signed before March 2008 do not enter it. However, these communications allow researchers to track the universe of job-relationships that started with a TD after March 2008. Considering that standard temporary contracts can be signed only for up to 36 months, from around March 2011 active TDs should be observable in the archives.[9]

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

[8] Some firms with multiple centres are allowed to communicate directly with the Ministry of Labour, but the information is redistributed to the regional system according to the location at which the worker is hired. Anastasia et al. (2009, 2010) describe the history and quality of these data.

[9] There are few exceptions that involve a small number of workers, in particolar 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 (apart from possible errors).

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 analysing these data is Veneto (Maurizio, 2006), where the dedicated agency (*Veneto Lavoro*) started elaborating a dedicated software since 1996. Moreover, using the flow of these communications, *Veneto Lavoro* organizes a full set of longitudinal microdata that track single individuals through time.

We chose to start from this dataset mainly because of their higher quality. Furthermore, *Veneto Lavoro* makes available to researchers the entire universe of microdata, while at the national level these are available only for a subset (individuals born in 48 different dates) and, more problematic, without any direct information about the event of a contract conversion from TD to TI. It must be mentioned that, according to data from the Labour Force Survey, in 2012 the Veneto region accounted for 9.5% of total employees in Italy, and for the 8.3% of total employees on a temporary contract.[10]

We focused on the universe of job-relationships that started with a standard temporary contract (*tempo determinato*) and were still active as temporary during 2012. We define as job-relationship a job with the same employer that carries through over time without the contract being closed. This includes jobs that have been regulated by a single contract for their entire duration, but also jobs where a temporary contract was later extended or was converted into a permanent one, and jobs that were subject to some alterations, like switching from full to part time. To be clear, two different and subsequent temporary contracts with the same employer, which were not formally connected by these kinds of events, are to be considered as two distinct job-relationships.[11] The main reason to focus on this unit of analysis is that we want to know whether there was a change in the event "temporary contract converted into permanent". However, it is clear that individuals could access permanent jobs even without a formal conversion, and therefore we will provide evidence that the increased rate of conversion was not compensated by a reduction in the number of direct hires with an open-end contract.

We selected only standard temporary contracts, that is we excluded those that are activated to substitute a permanent worker on leave (*per sostituzione*), those who are signed with an agency (*interinale o a scopo di somministrazione*), those that are working under a special contract that allows them to work in their own place (*a domicilio*), and those working under contracts designed for particular sectors or for other particular reasons.[12] Main results carry through to the more

[10] Data extracted from http://dati.istat.it/ (last retrieved: 00028/01/2014).
[11] There are also some legal limits on the possibility of signing repeated temporary contracts with the same employer.
[12] In results available on request we also tried excluding all employees in public sector, with no significant changes.

general case (results available on request), but this selection clears possible distortions generated by the specificity of these contracts.

Using the longitudinal information on each job-relationship $i=1,\dots,N$ we built a panel over different periods $t=1,\dots,T$ in 2012.[13] Each period has a length between 12 and 16 days, but we postpone to the next paragraph the discussion of how we exactly defined these units of time. For each period, we kept only job-relationships that are active with a temporary contract for at least one day. Therefore the panel is unbalanced, for two reasons:

1. New temporary contracts can be signed all during the year, and therefore new job-relationships can enter the panel in any period.
2. Temporary contracts which terminate (either for the natural end of the contract or for other reasons) or that are converted into permanent ones exit the panel after the period in which the event takes place.

The main outcome is a binary variable

$$y_{it} = \mathbf{1}[\text{job-relationship } i \text{ was converted from TD to TI in period } t] \qquad (1)$$

so that all results about the expectation of the outcome have to be interpreted as the probability that a temporary contract active during a single period t is converted into permanent during the same period. It should be noted that our outcome is different from the ones analysed in most of the papers about the "stepping stone" effect: while they focused on the probability that a temporary employee at time t obtains a permanent position sometimes later (generally one year later), here we focus on the rate of conversion within each single period.

For each job-relationship we also observe some time invariant characteristics: educational level of the worker, gender, sector of activity and citizenship.[14] 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. Finally, we can identify the employer: this allows us to cluster the standard errors at this level and to observe how many temporary contracts he holds at a particular moment in time.

From the raw panel, we dropped those observations with missing age or gender (21 in total), and we selected 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 593,486 observations, approximately 150 thousand job-relationships per period.

[13] We did very minor corrections on the raw data, dropping few cases where temporary contracts switched to permanent over time without an explicit communication about the conversion, and correcting the data of conversion for a small number of individuals who experienced repeated conversion of their contract over time.

[14] 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).

5. Identification strategy

We use a dif-in-dif strategy over different periods within 2012 and over different groups defined in terms of demographic characteristics. 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*:

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

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

1. The time trend is parallel across groups.
2. 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

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

Exploiting the timeline of the policy 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 incentives.
- 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 16 days after INPS declared that funds were (presumably) already finished.

We assume that in Period I the policy did not have any effect: only on the 5th of October the scheme of incentives and its details were made public and received full attention from the media. Differently, the 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 the change during that period if employers substituted conversions over time in order to benefit from the incentives.[15] 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 normally taken place much later in time, and for this reason we also analyse the following days (Period IV). In both cases (periods II and IV) we expect that, if

[15] For a discussion of announcement and implementation effects in dif-in-dif analysis, see Blundell et al. (2011).

employers substituted conversions over time in order to benefit from the allowances, the effect of the policy should be negative and may compensate the changes in period III.

The length of periods I and IV was chosen in order to match the period of full validity of the incentives. Clearly, one may want to look at a longer period of time, in particular for the period after they announced the end of funds. We also tried with other periods after November 17th, with similar results.

The different groups are implicitly defined by the policy: men over 30 were not eligible for the policy, while younger men and women of whatever age were. As in Anastasia et al (2013), we split women into those aged 30+ and those aged less than 30, in order to facilitate the comparison with the other two groups, although we also show aggregate results. We allocate individuals to each group according to their age at the begin of the period, but we also replicated the estimates defining it a the end of the period, with no sensible changes.

In order to identify the effect δ, we exploit the structure of the policy. Given that men over 30 were not eligible, we can 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 can 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.

In our case we further complicated the model by assuming that the treatment effect δ varies by period of treatment (II-III-IV) and by treated group. This is equivalent to a series of 2X2 dif-in-dif strategies where the control group is always Men over 30 and the pre-reform period is always Period I. Formally, the effects of interest can be identified from the coefficients on the interaction terms from the regression:

$$\begin{aligned} y_{it} = {} & \beta_0 + \beta_{M<30}\,\mathbf{1}[\text{Men} <30] + \beta_{W<30}\,\mathbf{1}[\text{Women}<30] + \beta_{W\geq 30}\,\mathbf{1}[\text{Women}\geq 30] + \\ & + \beta_{II}\,\mathbf{1}[\text{Period II}] + \beta_{III}\,\mathbf{1}[\text{Period III}] + \beta_{IV}\,\mathbf{1}[\text{Period IV}] + \\ & + \delta_{M<30,II}\,\mathbf{1}[M<30] \times \mathbf{1}[PII] + \delta_{M<30,III}\,\mathbf{1}[M<30] \times \mathbf{1}[PIII] + \delta_{M<30,IV}\,\mathbf{1}[M<30] \times \mathbf{1}[PIV] + \\ & + \delta_{W<30,II}\,\mathbf{1}[W<30] \times \mathbf{1}[PII] + \delta_{W<30,III}\,\mathbf{1}[W<30] \times \mathbf{1}[PIII] + \delta_{W<30,IV}\,\mathbf{1}[W<30] \times \mathbf{1}[PIV] + \\ & + \delta_{W\geq 30,II}\,\mathbf{1}[W\geq 30] \times \mathbf{1}[PII] + \delta_{W\geq 30,III}\,\mathbf{1}[W\geq 30] \times \mathbf{1}[PIII] + \delta_{W\geq 30,IV}\,\mathbf{1}[W\geq 30] \times \mathbf{1}[PIV] + \varepsilon_{it} \end{aligned} \tag{4}$$

There are several threats to this identification strategy. First of all, 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 is not likely to be the case: on the opposite, eligibility was targeted on workers that were more likely to be hit by the ongoing economic crisis. Furthermore, it should be noted that our estimates are based on relatively short periods of time, close 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.

Secondly, there may be seasonal trends that are divergent across groups. We checked whether this is the case by running a falsification on the analogous periods in 2011.

Thirdly, the panel is unbalanced, so that the group composition is not guaranteed to be stable over time.[16] We also run the same regression by adding a set of important 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).

Fourthly, we need to assume that in the first period 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. We tested this assumption using a dif-in-dif 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 problematic is that the incentives could have induced the employers to favour workers from the eligible demographic groups and to reduce the conversions for men over 30. We provide evidence that this is not likely to have happened by looking at the change in their conversion rate between periods in 2012 and in the previous year. Furthermore, while we look at conversion rates, employers were allowed to indirectly subsidize direct hires with permanent contracts by hiring workers with a temporary one and converting it after few days. Moreover, they could have simply favoured conversions with respect to direct hires. We also show that, during the periods of validity and relatively to 2011, there does not seem to be a sensible decrease in the aggregate number of jobs starting with a permanent contract.

[16] Younger men and women may also move across groups, but given the limited period of time this is not likely to be a major concern.

6. Results

6.1 Main results

Figure 2(a) shows how the rate of conversion within each of the four periods differs across groups in 2012. The outcome is pretty similar for all groups before the announcement, with only a slightly smaller probability for older women. Differently, in period III the probability that a temporary contract becomes permanent diverges for the eligible groups, showing an increase. There is no sign of substitution effects over time, because in the other two periods the rate remains quite similar across groups. The dif-in-dif regressions (Table 1, column (1)) confirm the findings. We always use standard errors clustered at the employer level to account for potential common shocks across different job-relationships.[17] For workers under 30 there is only evidence of a positive effect of the policy in period III, larger for men (1.6 percentage points) and smaller for women (1.1), with no statistically significant change with respect to the control group in the other two periods. For older women we still find a positive effect of the policy in period III, (1.2 percentage points). However, there is also evidence of a positive effect in periods II and IV, around 0.2-0.3 percentage points. This seems likely to suggest evidence of a diverging trend for this group, which would violate assumption A1, rather than of an actual policy effect: if nothing, we would expect a decrease in the conversion rate for eligible workers in the time window between announcements and begin of validity. Therefore we are probably overestimating the policy effect for this group. However, if we interpolate this diverging trend, the bias should be around 0.2-0.3 percentage points, bringing the effect for women aged 30 closer to those for younger ones.

-------- Figure 2 ABOUT HERE

-------- Table 1 ABOUT HERE

It seems puzzling that in the time between the announcement (October the 5th) and the start of the incentives (October the 17th) firms did not reduce their conversion activity for eligible workers in order to benefit for incentives later on. Indeed, although there is a decrease in the overall conversion rate in period II, this does not diverge across treated and control groups, and can be explained by the fact that the period is shorter (12 days) and does not include those days in which conversions usually take place (generally the begin of the month).[18] The most likely explanation is

[17] We also tried with standard errors clustered by sector of economic activity, and the main results on statistical significance carry on.

[18] Comparing Figure 2(a) and (b) we also notice that there does not seem to be a sensible decrease in overall conversion rate between the two analogous periods in 2011 and 2012.

that it took time for the information about the incentives to spread across employers, which were probably becoming aware of the set of rules only when their temporary contracts were coming to an end (generally towards the end of the month).

In Table 1, column (2) we present also a dif-in-dif regression which considers all the three eligible groups as a single one. Results are compatible with the main ones. There is still weak evidence of a small effect in period IV. Although this might be sign of a diverging trend (mostly driven by the results for older women), it is not completely impossible that the policy still had some impact on employers' decision. First of all, not all of them might have realised that the funds were (probably) terminated only after having dealt with the worker about the possible conversion, and they may had already signed the contract as well. Secondly, although INPS declared that the number of request arrived by November the 2nd were already enough to exhaust the fund, it was not certain that all the demands were indeed eligible, and therefore employers could still hope to get the benefit, even if with smaller chances.

----------- Table 2 ABOUT HERE

Given the general absence of substitution effects over time, Table 2 summarizes the results by calculating the proportional increase due to the policy as the ratio between the estimated effect in Period III and the counterfactual conversion rate predicted by the model (4). It does not consider the possible presence of effects in period IV as well, both because they can come from a diverging trend for older women, and because it is anyway quite small. As a percentage, the effect for young men and older women is quite similar, while it is smaller for females under 30. The overall proportional effect is 83%, which implies that the actual cost of increasing the number of conversions by one unit is equal to 221% the actual cost of one single incentive, because part of the expenditure goes to contracts that would have anyway become permanent. Given that our data contain the universe of all temporary contracts (started with a standard type) for workers aged 15-65 in Veneto during the different periods, in Table 2 we also show the reform effects in terms of number of contracts, by simply multiplying the number of observations for the estimated probabilities. Our conclusions are consistent with what was found by Anastasia et al (2013) on aggregate data, although our estimated effects are slightly smaller, given that they evaluated that the policy approximately doubled the number of conversions.

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-relationship subject to conversion in Period III (2012) were full-time.

Assuming that all the part-times were at half of the standard working time, the average incentive was 9,695 euro.[19] This implies that the full cost of an actual unit increase in the number of conversions with respect to the counterfactual is 21,389 euro, as it requires an expenditure of 11,693 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 month and in terms of position within the month, the conversion rates across time are likely to be affected by seasonal patterns. In our case this would bias the results only as long as there are group-specific seasonal trends. To check whether this problem affects our estimates we also replicated the same exercise over the analogous periods in 2011, when no similar policy applied. From Figure 2(b) we notice no evidence of diverging trends. The relative regression estimates are reported in Table 1, column (3). For men there is no evidence of differential changes over periods with respect to the control group, neither from the economics perspective, nor from the statistical one. For women there is a weak sign of a potential decrease in period III (with respect to the change in the control group): if, in the absence of the policy this would have applied in 2012 as well, then in our main results we would be underestimating the effect of the policy by around 0.2 percentage points, which would broadly compensate the previously discussed positive bias. 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 small and not statistically significant, we prefer to focus on the dif-in-dif within 2012 in order to avoid introducing noise in our main estimates.

In order to see whether large changes in the group composition are affecting the result, in Table 1 col. (4) we also added to the basic regression 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.[20] The results are basically unchanged.

In the main results 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,

[19] 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).
[20] In case of missing for educational level or sector of activity, we kept the observation but we added a dummy for missing value.

passed in December 2011. To test this, we estimated a dif-in-dif regression with the same control/treatment groups, but considering only two periods: period I in 2012 and period I (the analogous window of the year) in 2011. If eligible groups are affected by the policy in period I of 2012 as well, then we should find evidence of an effect when comparing it with 2011 (using men over 30 as a control):

$$y_{it} = \gamma_0 + \gamma_{M<30}\, \mathbf{1}[\text{Men} <30] + \gamma_{W<30}\, \mathbf{1}[\text{Women}<30] + \gamma_{W>30}\, \mathbf{1}[\text{Women}\geq 30] +$$

$$+ \gamma_{2002}\, \mathbf{1}[\text{year } 2012] + \delta_{M<30,I}\, \mathbf{1}[M<30] \times \mathbf{1}[\text{year } 2012] +$$

$$+ \delta_{W<30,I}\, \mathbf{1}[W<30] \times \mathbf{1}[\text{year } 2012] + \delta_{W\geq 30,I}\, \mathbf{1}[W\geq 30] \times \mathbf{1}[\text{year } 2012] + \eta \qquad (5)$$

None of these δ are statistically (or economically) significant, being all smaller than 0.13 percentage points, with a joint test for them all equal to zero failing to reject the null with p-value 0.3936. Therefore there seem to be no evidence that firms anticipated the policy before its announcement.[21] Full results are available on request.

Besides anticipation/delay effects, employers could have also substituted conversions for the control group in favour 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 tested whether the trend over periods I to IV was different in 2012 with respect to 2011. Results are reported in Table 3. 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 0.9095. Therefore the evidence is not against the assumption that the control group has been (on average) unaffected by the policy.

------------- Table 3 ABOUT HERE

Another possible substitution could take place between conversions from TD to TI and direct hires with a permanent contract. In Figure 3 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 decrease sufficient to reduce the increase in conversions showed in Table 2.

------------- Figure 3 ABOUT HERE

[21] Similar dif-in-dif regressions 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).

One may also criticize the use of treatment and control groups with large differences in terms of average age. Table 1, col. (5), shows that results are robust to selecting only individuals around the age cut-off, although the effect for older women is larger.

In results not shown in the paper, but available on request, we also show that the effect is not driven only 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 a positive impact for the treated groups. In terms of possible anticipation effects, it is clear that negative effects on the conversion rates of eligible workers can be found even later in time, also because our definition of Period IV does not include the begin of a month, when usually conversions take place. For this reason we added two subsequent periods of 16 day length, adding therefore dates until December the 19th, and we checked whether negative effects were in place (figures and results available on request). 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 (-0.18 percentage points, p-value 0.214). Finally, these incentives could be cumulated with others available for hiring workers that have been previously dismissed through a particular procedure, called *mobilità*.[22] As a robustness check, we also replicated these regressions by excluding these employees, again with no significant changes in the main results (estimates available on request).

The magnitude of the financial incentives could have induced firms to convert contracts into permanent only in order to obtain the transfer, and later dismiss the worker as soon as the required six months duration requirement was met. The possibility of this strategic behaviour is constrained by the presence of higher employment protection legislation for permanent workers. Nevertheless, we examined whether the contracts for eligible employees converted during the period of validity of the incentives (period III in 2012) were more likely to be terminated 7 months after. Essentially, we focused on the outcome **1**[job-relationship *i* terminated 7 months after period *t*], conditioning only on those contracts that were converted in period III, and we did a dif-in-dif regression using men over 30 as control group and the same period in 2011 as pre-reform period.[23] There is no evidence of higher chances of termination for those contracts converted during the policy, as the estimated effect is actually negative though not statistically significant (full results available on request). Clearly this result might be conditioned by the six months duration constraint, and one may want to

[22] http://www.inps.it/docallegati/Informazioni/aziendeconsulentieprofessionisti/Documents/FAQ_elenco_esportabile_02_08_2013.pdf (last retrieved: 14/02/2014).

[23] We also tried using period I in 2012 as pre-reform period, with similar conclusions.

focus on the medium and long term. Unfortunately, our data are currently limited to the end of June 2013, and therefore we leave this question to future research.

6.3 Heterogeneity

Given previous results from Cipollone and Guelfi (2003, 2006), who found evidence of a positive effect of the incentives from 2001 for university graduates, but smaller for high school graduates and negligible for those with a lower qualification, we examined the effect is heterogeneous across educational level. In the following we focus only on the effects on period III.

---------- Figure 4 ABOUT HERE

The graphs on the left of Figure 4 show, for each group, the estimated effect in percentage points by educational level.[24] Those who have at most completed primary school show a smaller effect, significantly different from zero only for men under 30. However, this is a relatively small group, accounting for only 11% of the observations for eligible workers in 2012. Differently, starting from middle school (8^{th} grade) there is no evidence of a strong heterogeneity: most of the effects are positive and there is no systematic increase associated with higher qualifications. Furthermore, the effects for the three most relevant qualifications (middle school, high school, university) are all significantly different from zero at the 5% level. The effect for vocational diploma, which can be taken at the 10^{th} or 11^{th} grade, has a similar point estimate but is more dispersed and not significantly different from zero, probably because of a smaller number of observations. Similar conclusions can be taken by looking at the proportional effect (graphs on the right of Figure 4), 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.

One critique is that the reduction in the number of observations, and in particular in the total number of observed conversions, for each cell of group – period – educational qualification makes estimates largely imprecise, hindering the ability to detect heterogeneity. We tried to understand whether our conclusions are driven by this problem in two ways. First, we estimated the average effects aggregating by low qualification (middle school or less) on the one hand, and by high qualification (high school or above) on the other. We excluded vocational diploma because they are

[24] 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 all interaction terms (for all groups and all periods) in all educational group are jointly equal to zero with p-value 0.4412. Only for women the joint test of all interactions across all educational level gives a p-value 0.052, which is driven by a moderate decrease in periods II-III-IV for high school graduates.

a particular case. For men under 30, the 95% confidence interval for the effect in percentage point is [.0102;.0201] for low qualification, very similar to the estimate for the other group, [.0112;.0225]. The same holds for younger women ([.0038;.0145] vs [.0056;.0158]) and for older ones ([.0089;.0167] vs [.0076;.0161]).[25]

We also replicated the regressions aggregating all the eligible workers into a single treatment group (similarly to Table 1, col. (2)). Apart from those who completed at most primary school, who show a small and not statistically significant effect, the other educational qualifications do not show high heterogeneity and are not consistent with effects being greater for more educated workers: point estimates range from 0.0157, for those who completed middle school, to 0.0120 for university graduates.

------------ Figure 5 ABOUT HERE

Differently, in Figure 5 we show evidence that the effect is smaller for workers whose employers hold a larger number of temporary contracts, both in percentage points and as a proportion of the baseline rate of conversion.[26] Indeed, for all treatment groups we reject the null that the effects are all the same at the 5% level, both considering the effects in percentage points and as a proportion of the baseline.[27] There are two main reasons for this. First of all, the policy had a limit of 10 incentives per employer, which constrained the possibilities of conversion for workers with more colleagues holding a temporary contract. This is likely to be an important explanation. Indeed, we also split the sample according to the number of eligible temporary workers, and estimated effects (in percentage points) are positive and statistically significant only for employers with less than 10 eligible workers, while they are smaller and not statistically significant at the 5% level for the other.[28] Secondly, employers with a widespread use of fixed-term contracts may be less interested in signing permanent ones no matter what incentives they could receive, possibly because of the nature or their economic activity.

[25] Given that the baseline rate of conversions is higher for most educated, these results imply a smaller % increase for them, although we always fail to reject the null that the proportional effect is different (with high p-values). Anyway, this evidence is consistent with concluding that we did not find evidence of a larger effect for more educated workers.

[26] As a sideline, results for firms with only one temporary contract are an additional robustness check, given that employers could not substitute between eligible and non eligible workers. It should be added that the falsification over 2011 fails to reject the null that all interaction terms (for all groups and all periods) for all different totals of temporary workers are jointly equal to zero with p-value 0.3276, and the test gives similar results within each single treatment group.

[27] In the latter case the test is based on s.e. calculated using the delta method, using the built-in command *testnl* of Stata™ 13.

[28] There are two exceptions. Men under 30 whose employers fit in the [11,25] group (in terms of # of eligible workers) show an effect around 0.8 percentage points (50% as a proportion of the baseline), though significant only at the 10% level (p-value 0.094). For women over 30 the estimated effect is 0.6 percentage points (179% in proportional terms), with p-value 0.055.

7. Conclusions

We evaluated the impact of a scheme (Ministerial Decree 5 October 2012) that provided monetary incentives to firms willing to transform fixed-term labour contracts into permanent ones. Results show that: (i) the policy significantly increased the likelihood of a contract being transformed from fixed-term to permanent; (ii) this effect does not seem to be compensated by a decrease in the number of transformations for workers who were not eligible for the incentives (older men); (iii) there seems to be no evidence of substitution over time.

The finding of a positive average effect, with no sensible differences according to the educational level of the worker, is different from the main results from Cipollone and Guelfi (2003, 2006) about the scheme of incentives that was available at the begin of last decade. We can advance two possible tentative explanations. First of all, the incentives for contract conversion of Decree 5 October 2012 did not require the employer to increase the workforce, as instead was stated in law 388/2000, and this difference can be particularly crucial during the economic crisis. Clearly, whether a policy maker should or not impose this constraint also depends on the final target of the scheme. Furthermore, caution should be taken about this conclusion, because we could not simultaneously observe the two alternative treatments, with and without the constraint, and therefore further evidence is needed to evaluate whether it played an important role. Secondly, Cipollone and Guelfi (2006) general results refer to the probability of entering permanent employment from any other status, while we analysed the effect only on the conversion rates from fixed-term to open-end contracts. Indeed, our results are consistent with their finding of a positive effect for those previously employed with a temporary training contract. This could also explain the absence of heterogeneity by educational level in our estimates. While they correctly argued that a potential employer is more likely to exploit an incentive in order to directly hire individuals with signals of higher productivity (in particular for more educated workers), this framework is not the best one for discussing the effect of the benefit for contract conversions introduced by decree 5 October 2012. The reason is that in this case all the eligible workers were already known by the employer, who had already had time to screen them during the temporary contract, and therefore their willingness to change their status to permanent is less likely to depend on external productivity signals.

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Tables and Figures

Table 1 Dif-in-dif results (probability of conversion from temporary to permanent contract during each single period)

	(1) Main results	(2) Main results (aggregated)	(3) Falsification over 2011	(4) With covariates	(5) Aged [26,34]
Men < 30	-0.0004		-0.0012	-0.0030**	0.0014
	(0.0011)		(0.0010)	(0.0012)	(0.0019)
Women < 30	-0.0011		-0.0019	-0.0042***	-0.0012
	(0.0011)		(0.0013)	(0.0012)	(0.0019)
Women ≥ 30	-0.0034***		-0.0021**	-0.0019**	-0.0013
	(0.0009)		(0.0010)	(0.0009)	(0.0019)
All treated groups		-0.0021***			
		(0.0007)			
Period II	-0.0124***	-0.0124***	-0.0122***	-0.0128***	-0.0137***
	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0016)
Period III	0.0001	0.0001	-0.0003	-0.0006	-0.0001
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0020)
Period IV	-0.0100***	-0.0100***	-0.0097***	-0.0114***	-0.0104***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0017)
Men < 30 × Period II	0.0004		-0.0006	0.0003	-0.0004
	(0.0012)		(0.0011)	(0.0012)	(0.0022)
Men < 30 × Period III	0.0163***		-0.0008	0.0161***	0.0157***
	(0.0019)		(0.0014)	(0.0019)	(0.0031)
Men < 30 × Period IV	0.0011		-0.0017	0.0008	-0.0012
	(0.0014)		(0.0012)	(0.0014)	(0.0024)
Women < 30 × Period II	0.0001		-0.0007	0.0003	0.0002
	(0.0013)		(0.0014)	(0.0013)	(0.0022)
Women < 30 × Period III	0.0108***		-0.0015	0.0111***	0.0111***
	(0.0019)		(0.0018)	(0.0019)	(0.0031)
Women < 30 × Period IV	0.0014		-0.0014	0.0020	0.0006
	(0.0014)		(0.0014)	(0.0014)	(0.0024)
Women ≥ 30 × Period II	0.0020**		-0.0001	0.0024**	-0.0001
	(0.0010)		(0.0010)	(0.0010)	(0.0022)
Women ≥ 30 × Period III	0.0124***		-0.0023*	0.0129***	0.0166***
	(0.0014)		(0.0013)	(0.0014)	(0.0032)
Women ≥ 30 × Period IV	0.0028***		-0.0008	0.0036***	0.0003
	(0.0010)		(0.0011)	(0.0010)	(0.0024)
All treated × Period II		0.0012			
		(0.0008)			
All treated × Period III		0.0131***			
		(0.0012)			
All treated × Period IV		0.0020**			
		(0.0009)			
Constant	0.0178***	0.0178***	0.0185***	-0.0016	0.0193***
	(0.0007)	(0.0007)	(0.0007)	(0.0018)	(0.0014)
Observations	593486	593486	615975	593486	147345

Note: * p<.10 ** p<.05 *** p<.01. Standard errors clustered for employer in brackets. Covariates in column (4) include 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 (coefficients available on request). All estimates are obtained using Stata^{TM} 13.

Table 2 Summary of the effects

	Men < 30	Women < 30	Women ≥ 30	All treated
Counterfactual conversion rate from temporary to permanent during period III	0.0175	0.0168	0.0145	0.0158
Reform effect in period III	0.0163	0.0108	0.0124	0.0131
Number of conversions during period III without the policy	403	308	687	1400
Reform effect in number of conversions during period III	375	198	588	1159
Reform effect / baseline	93%	64%	86%	83%
% full time on total conversions in period III	84%	56%	50%	62%
Average incentive (euro)	11051	9332	9012	9695
Full cost per increased conversion (euro)	22916	23848	19550	21389

Note: the number of conversion is calculated as the estimated probability times the number of temporary contracts active in Period III. The last column does not precisely sum up the previous three, because the estimate of the effect come from the aggregate model (Table 1, col. (2)). The average cost of a conversion is calculated assuming that all part-time are at half time.

Table 3 Test for a reduction in the control group

	Men ≥ 30
Period II (in 2011)	-0.0122***
	(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 × Period II	-0.0002
	(0.0011)
Year 2012 × Period III	0.0005
	(0.0013)
Year 2012 × Period IV	-0.0003
	(0.0011)
Constant	0.0185***
	(0.0007)
Observations	468844

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

Figure 1 Timeline of the policy

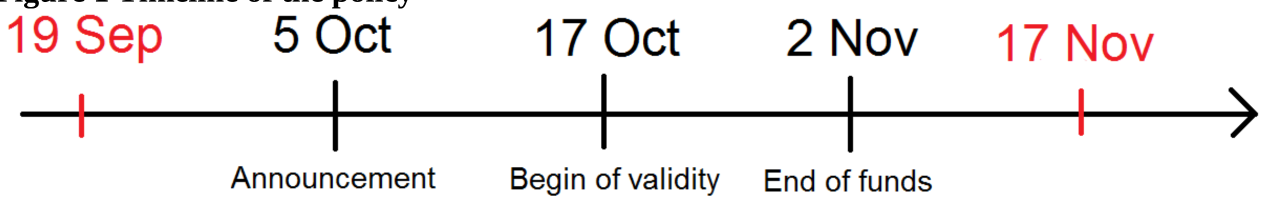
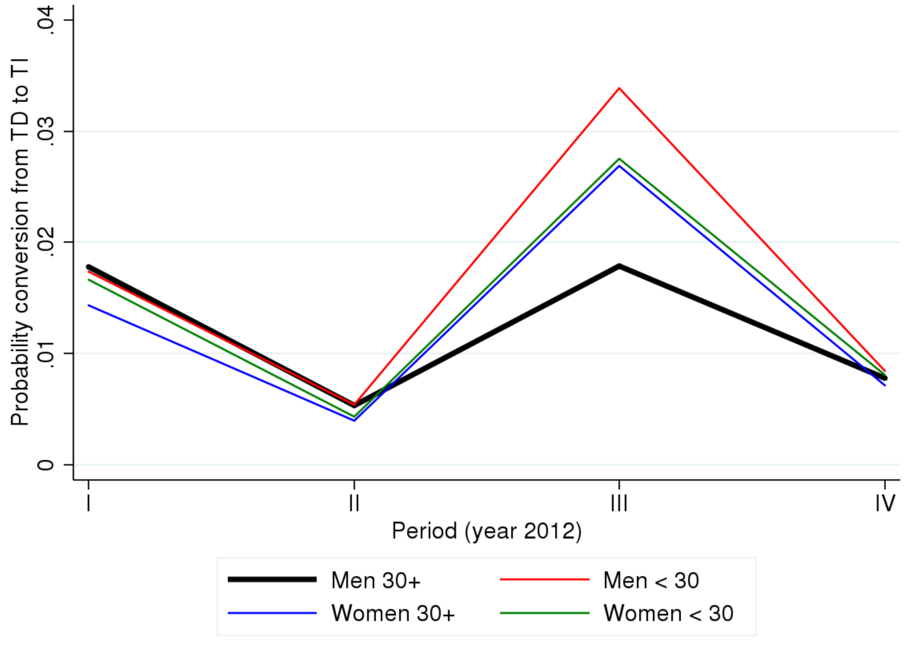
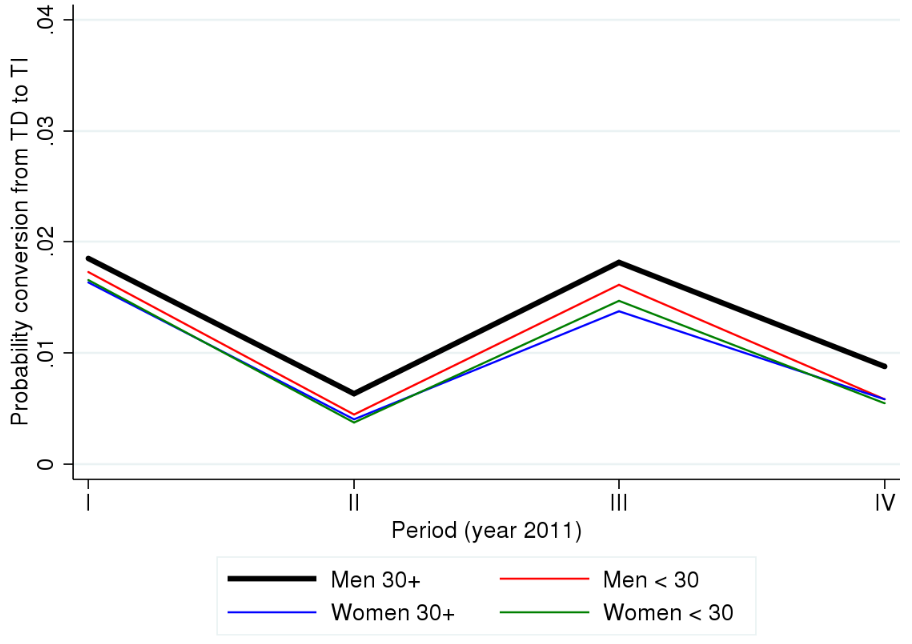


Figure 2 Probability of conversion from temporary to permanent contract during the period, by period and group

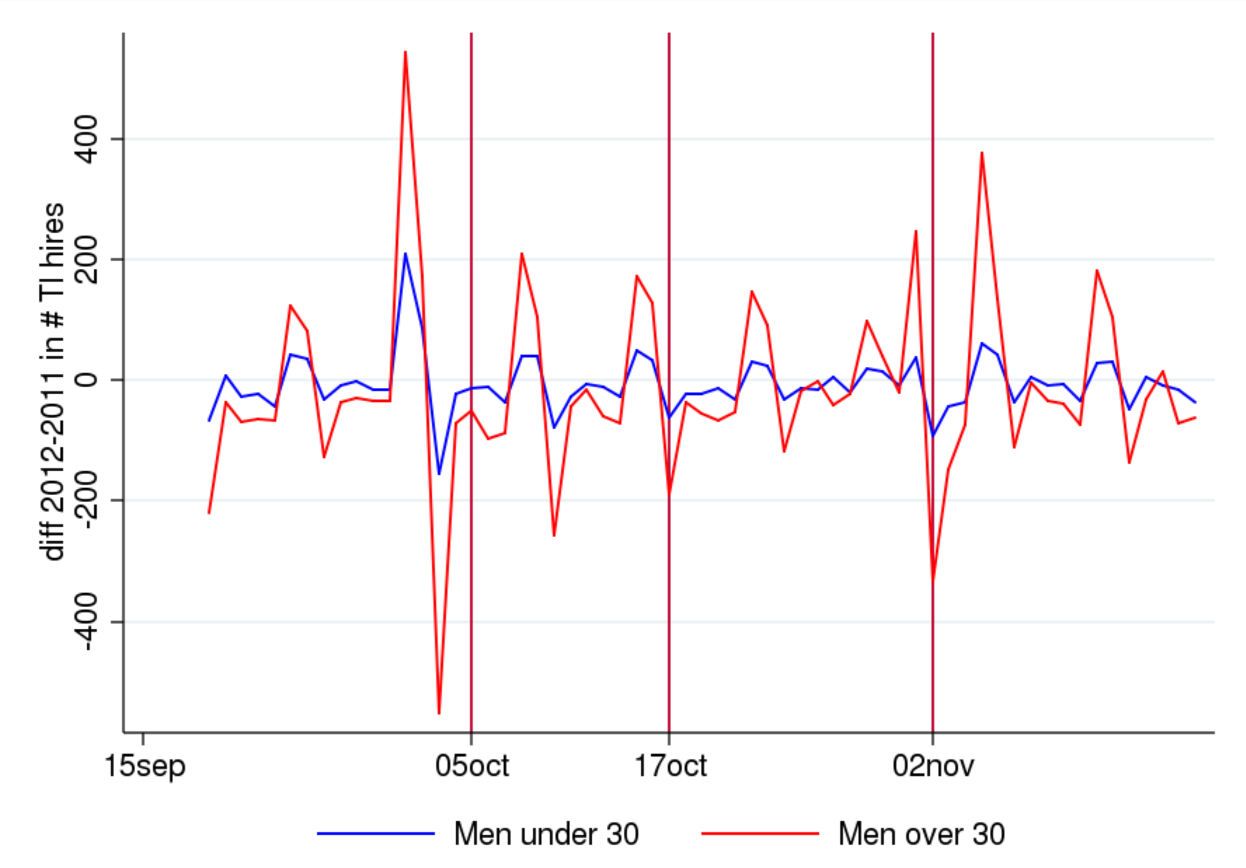


(a) Dif-in-dif on year 2012



(b) Dif-in-dif on year 2011 (falsification)

Figure 3 Difference in the number of direct hires with permanent contracts (TI) between 2012 and 2011, by day and group



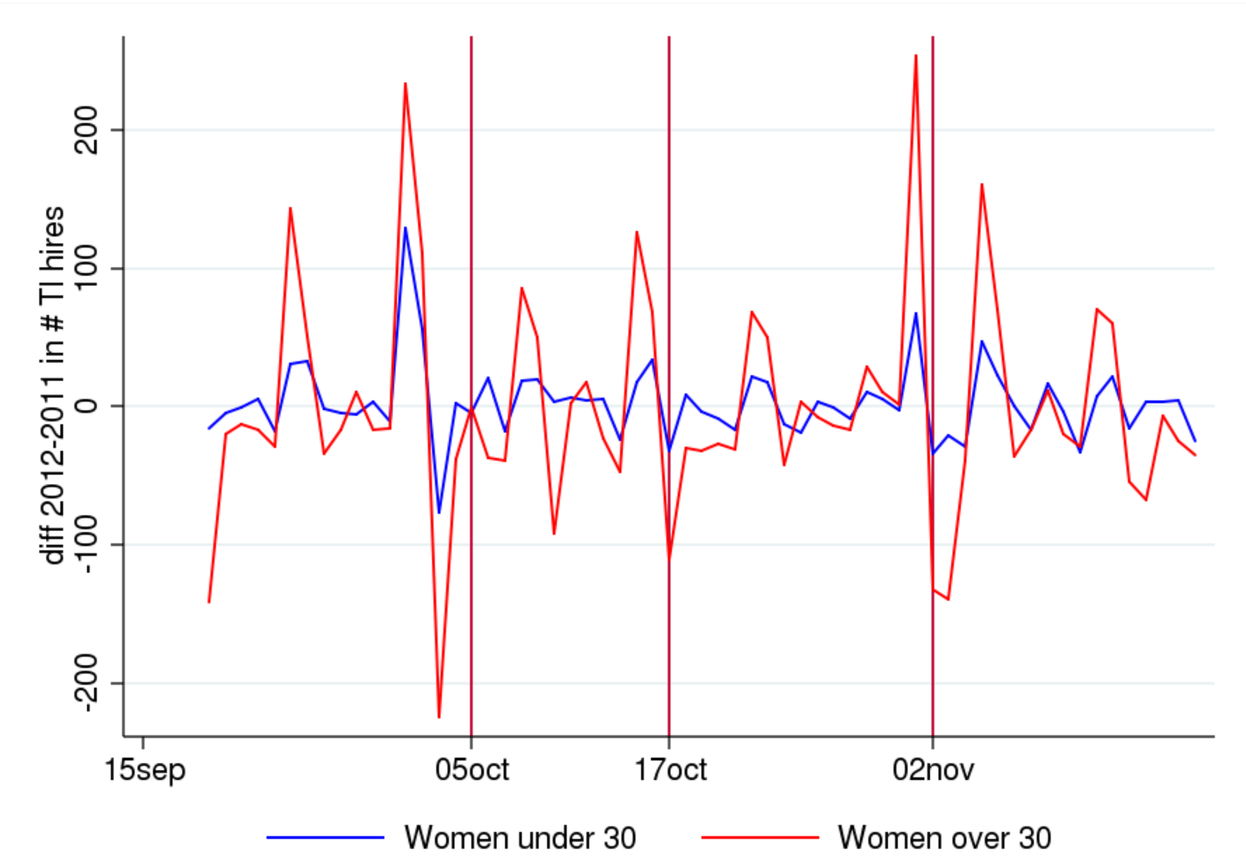
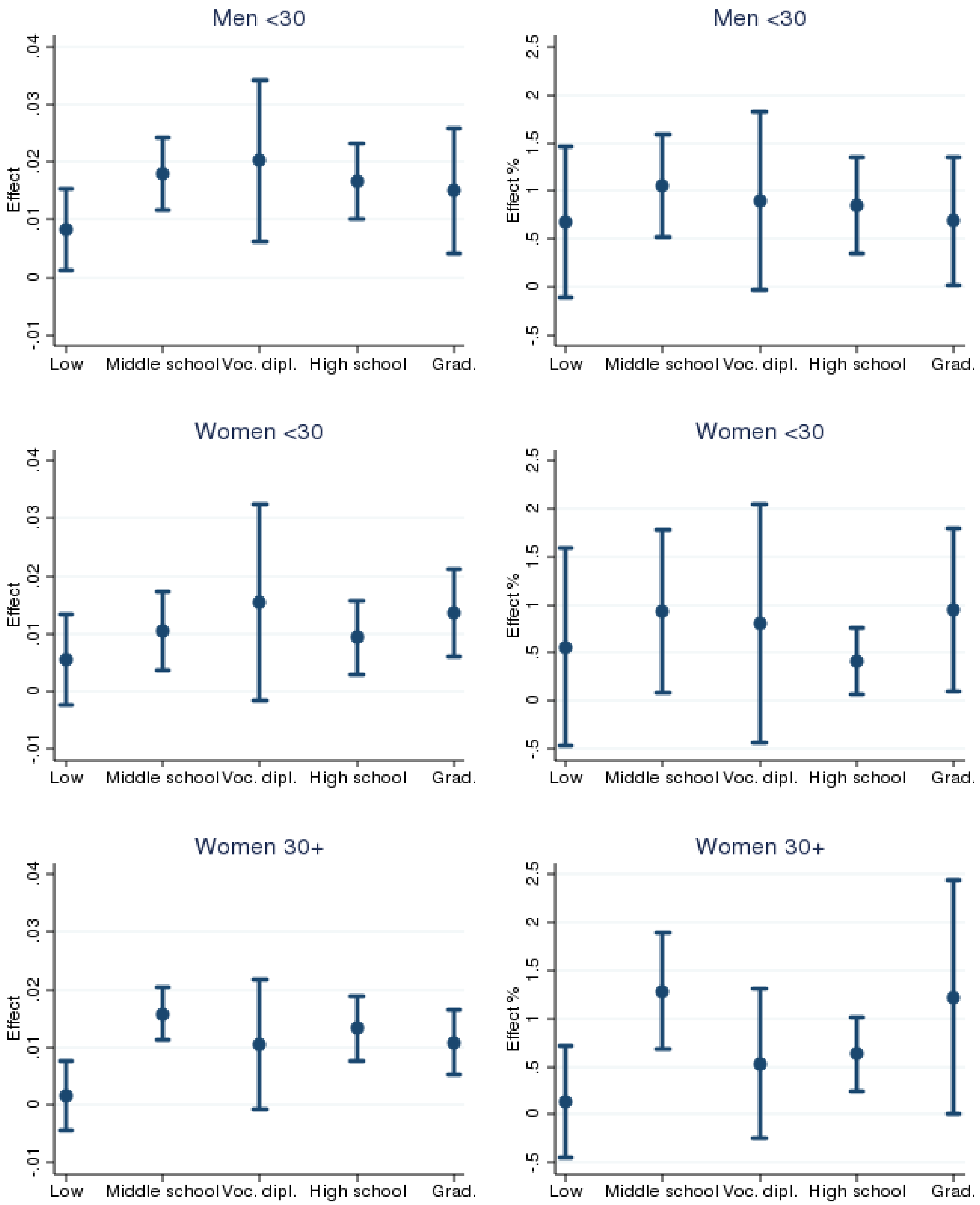
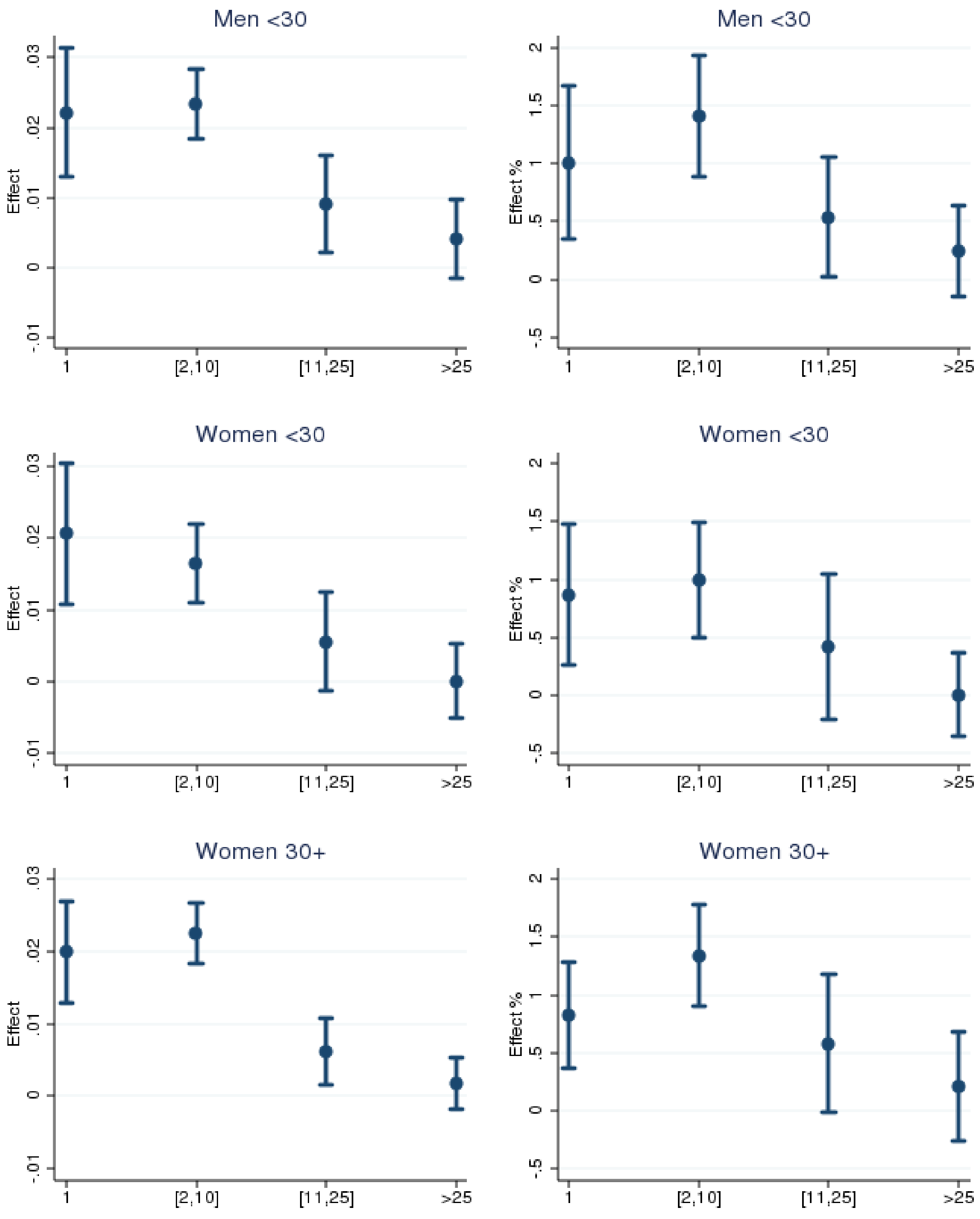


Figure 4 Dif-in-dif by educational level (effect on the rate of conversion during period III, in percentage points on the left and as a proportion of the counterfactual rate on the right)



Note: 95% confidence interval; s.e. clustered by employer. Estimates are obtained using Stata™ 13. “Low” stands for workers who completed at most primary school, “Middle school” is the 8th grade, “Voc. Dipl.” is a vocational diploma that last two or three years after middle school, “Grad.” stands for graduates.

Figure 5 Dif-in-dif by number of temporary contracts held by the employer (effect on the rate of conversion during period III, in percentage points on the left and as a proportion of the counterfactual rate on the right)



Note: 95% confidence interval; s.e. clustered by employer. Estimates are obtained using Stata™ 13.