

**HIRING INCENTIVES AND/OR FIRING COST REDUCTION?
EVALUATING THE IMPACT OF THE 2015 POLICIES ON THE ITALIAN LABOUR
MARKET***

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Abstract

Aiming at reducing labour market dualism and fostering employment, Italy adopted in 2015 two different policies: a generous permanent hiring subsidy (covering also conversions into open-ended contracts) and new regulations lowering firing costs and making them less uncertain. Using microdata for Veneto and exploiting some differences in the design of the policies, we evaluate the impact of each measure. Both contributed to double the monthly rate of conversion of fixed-term jobs into permanent positions. Moreover, around 40 per cent of new gross hires with a permanent job contract occurred because of the incentives, with the new firing regulations adding up, when in conjunction to the hiring incentives, further 5 percentage points. The new firing rules made also firms less reluctant in offering permanent job positions to yet untested workers. The possibility to benefit from the incentive in case of a conversion also boosted temporary hiring, as it allowed firms to test for the quality of a job match.

JEL: J6, J21

Keywords: job creation, firing costs, hiring incentives, labour market reforms.

* We thank Bruno Anastasia and Gianluca Emireni of Veneto Lavoro for providing us not only with the data but also valuable help in interpreting them. We also thank Effrosyni Adamopoulou, Samuel Bentolila, Clemence Berson, Emanuele Ciani, Emanuela Ciapanna, Marta de Philippis, Guido de Blasio, Eugenio Gaiotti, Sebastien Roux and Francesco Zollino for helpful comments. We are solely responsible for all errors. The views expressed herein are ours and do not necessarily reflect those of the Bank of Italy.

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1. Introduction

Italy lost 1 million jobs over the 2008-2014 period as a result of a double deep recession. As a consequence of its dualistic labour market structure, job losses were concentrated among youths and more generally people holding temporary job contracts. More recently, employment, and more specifically open ended contracts, have started to rise (around 1 percent in 2015), albeit the still subdued GDP growth (a bit less than 1%). In this paper we test whether and to what extent this evolution can be imputed to two policy measures adopted in Italy at the end of 2014 and aimed at both reducing labour market dualism and stimulating job creation. The first is a sizable temporary rebate of non-wage labour costs which applies to all new permanent job contracts (hereafter PHI) offered to workers who in the previous semester had not an open-ended position. The incentives are not targeted to specific groups of workers nor conditional to firm-level net job creation and apply also to conversions from a fixed-term to an open-ended position. The second is a reshaping of the regulation of dismissals, aimed at reducing the level and the uncertainty of firing costs for all new permanent contracts in firms with at least 15 employees (the “*contratto a tutele crescenti*”, or “graded security contract” hereafter CTC, part of a wider reform package denominated “Jobs Act”).

We separately identify the effects of the two policies by jointly exploiting the different time dimension (one implemented since January 2015 and the other since March 2015) and the differences in the coverage of the two schemes, as the PHI applies to all firms but only to those workers with no permanent job position over the previous semester, while the new CTC regulation reshaped firing costs for all new permanent contracts, but only for firms beyond the 15 employees threshold. In order to do that, we exploit the administrative data for the Veneto region, which allow us not only to measure labour market flows (in the first half of 2015 and 2 years before), but also to reconstruct the previous labour market status of workers, to identify the firm matched with the workers and the firm’s size.

It has to be stressed that there are relevant aspects of the two policies here not considered. We do not discuss all the pros and cons of the design of the two policies, nor the general equilibrium effects that can derive from their implementation, like, for instance, the effects on labour supply. However, even with these caveats, our results show clearly that both policies fostered net job creation at the firm level, shifted the employment composition towards permanent job contracts and increased the workers’ (permanent) job finding probability.

According to our findings the doubling in the monthly conversion rate from temporary to permanent contracts observed in Veneto in the period January-June 2015 (from about 1 to more

than 2%) is entirely due to the two policies, each policy contributing approximately one half to the total growth. On top of that, around 45% of gross hires with a permanent job contract can be imputed to the two policies. Disentangling the effect of the two policies, we find that hiring incentives (PHI) had the larger impact, contributing by 40 percentage points, with CTC only strengthening the impact of the PHI in the above 15 employees firms, such a combination of the two policies contributing for further 5 points.

An interesting result is in the impact of the two policies on firms' attitude to hire yet untested workers on either a permanent or a temporary basis. First, the reduction of firing costs introduced by the CTC enhanced the willingness of 15+ firms to hire workers who had never before worked for the firm. Second, the fact that PHI also applied to temporary to open ended contract conversions led on the contrary to a rise in temporary hiring¹, as many firms still exploited the possibility to test workers through a temporary job spell eventually converting it into a permanent position later on.

The relatively small effect of the CTC on the level of employment is in line with the literature about the labour demand effects of a certainty equivalent change in firing costs. From a theoretical point of view, in a static model firing costs are just a tax on firing that is economically equivalent to a component of labour costs. If wages cannot adjust, an increase in firing costs implies a downward shift in labour demand and lower employment. Its relevance, however, may be limited by the fact that firing costs may be far away over time and needs to be discounted. In a dynamic context, higher employment protection dampens employment fluctuations as firms do not fully adjust labour input to economic shocks: most of the positive employment effects of a reduction (increase) in firing costs so would materialize in an economic upswing (downswing) and Italy in 2015 was far away from a cyclical upswing. So, most of the impact of firing costs is through its negative effects on the allocation of resources, a general equilibrium channel recently emphasized by Rodano, Rosolia and Scoccianti (2016). As such, our paper adds to (and mostly confirms) the relatively rather scant empirical literature focusing upon the labour demand effects of firing costs. Autor, Kerr and Kugler (2007), using state-level and time variation in employment protection legislation in the US, find that higher employment protection reduces employment flows. Adhvaryu, Chari and Sharma (2013) focus on rural India and relate supply-side shocks (like rainfalls) and different state-level employment protection

¹ While our estimating period includes only the first half of 2015, so as to focus upon a period close to the two policies introduction, such an interpretation is supported by the raw evidence of hiring in December 2015, which was the last date over which the full amount of the PHI could have been cashed in. See INPS, Osservatorio sul precariato, available at <https://www.inps.it/portale/default.aspx?iMenu=1&itemDir=10342>.

legislation to show that employment protection reduces employment responses to shocks. In our paper we instead focus on a single Italian region, Veneto, before and after the inception of the new law on firing costs. As a consequence, our results are not affected by spurious local trends as in the above mentioned empirical papers.

More novel are our results about the effects on hiring of a reduction of both the uncertainty and the average expected level of firing costs. Actually, the CTC impacted mostly upon the high uncertainty stemming from the possibility that in the previous regime judges had to decide in favour of the worker mandating not only monetary reimbursement, but also the worker's reinstatement in the firm. Any effect of the CTC, albeit small, is likely to be driven by the effects of the reduction in the uncertainty about the possible firing costs.

Similarly, several papers have already analysed how firing costs shape firms' propensity to use fixed-term job contracts, not only to facilitate short-term labour adjustment, but also to screen workers and test the goodness of a job match before offering a permanent position (e.g. Faccini, 2013, Guell and Petrongolo, 2007). However, to the best of our knowledge, no paper has so far shown that a reduction in firing costs increases firms' propensity to offer a permanent position to workers not previously screened.

Concerning hiring incentives, to the best of our knowledge, this is the first time that the Italian government introduces non-targeted, non-conditional hiring incentives. Cipollone and Guelfi (2003) analyze selective hiring incentives introduced in 2001 targeted to young workers hired on a permanent basis and do not find relevant effects on labour demand. Ciani and De Blasio (2014) consider instead a very short-term policy intervention for the conversion of fixed-term contracts into permanent jobs, introduced in Italy in 2013 and lasting just few weeks because of severe funding constraints. This incentive was targeted to females and young people and they find a positive effect of the policy on conversion rates. In 2014 the government introduced incentives to firms hiring workers on a permanent basis, but incentives were conditional to firms' net job creation. Given the very weak economic conditions, only very few firms applied for the incentives. Our results, instead, support the hypothesis of the immediate effectiveness of non-targeted non-conditional incentives on gross and net job creation, and in this respect are similar in spirit to the ones analysed by Cahuc et al. (2014) who find a positive and rapid expansionary effect of the non-conditional hiring credits introduced in France during Global Financial crisis. Our results are also in line with Neumark (2013) and Neumark and Grijalva (2013) who argue that non-targeted hiring incentives have a positive effect on

employment during recessions, even if deadweight losses associated to these policies are generally large (e.g. Brown 2011).

The paper is organized as follows. In section 2 we briefly describe the policy measures introduced by the Italian government in 2015 (hiring incentives PHI and the CTC introduced by the Jobs Act). In section 3 we describe our dataset. In section 4 we describe our estimation strategy. Section 5 present the results of the empirical analysis. Section 6 concludes.

2. The Italian labour market and the policies under scrutiny

The Italian labour market is heavily segmented in permanent and fixed-term workers. The dualism arose at the end of the nineties when the government progressively introduced different types of fixed-term contracts, to increase flexibility in the use of labour. Higher flexibility, however, was not accompanied by changes in firing costs for permanent job contracts.²

During the 2000s the share of fixed-term workers in total workers increased rapidly to around 13 percent. More than 60 percent of hires were made up of fixed-term job contract, used not only to face labour demand uncertainty, but also as a cheap screening device before hiring workers under a permanent contract. People employed temporarily suffered the most for the consequences of the Global financial crisis, when firms, facing a sudden drop in their activity, used all the available margins to adjust labour input. For this reason the first steps in tackling firing costs were made in 2012, by the law no. 92/2012, the so-called “Fornero Reform”, followed in 2015 by the Jobs Act.

Before that, Italy was characterised by firing costs whose main feature was their uncertain and (potentially) high amount³. As such, dismissals were costless for firms if taking place on a just cause basis, i.e. workers mis-behaviour or firms’ need to reduce or reorganize its workforce⁴. However, whenever a worker opposed and the court judged the dismissal to be unfair, the costs might become rather high, particularly in the case of firms with at least 15 employees. Firms with less than 15 employees could opt (according to the law no. 108/1990) between reinstating the worker and paying a pre-set severance payment (related to workers

² See Sestito (2002) and Pirrone and Sestito (2006).

³ We here refer to individual dismissals.

⁴ A liquidity cost is present because the firm has to pay to any departing worker (including those quitting and those retiring) a given percentage of each yearly earnings accumulated over time at a subdued interest rate (more precisely it is equal to $\frac{3}{4}$ of the CPI inflation rate plus 1.5%), the so called *Trattamento di Fine Rapporto* (TFR). Disbursing the TFR may imply an additional firing cost at the margins for those firms who are liquidity constrained as such an anticipated disbursement has to be somehow financed through a banking loan or other means. Notice that since 2007 workers may decide to accumulate the yearly contributions to the TFR into a second pillar pension scheme, depriving the firm of the low cost financing provided by TFR: in such a case, firing a worker is totally unaffected by the existence of the TFR.

seniority and varying from 2.5 to 6 monthly pay). For firms with at least 15 employees, the general rule was that of workers' reinstatement. On practical grounds firms could side-step reinstatement by reaching private arrangement with the worker, the cost of it possibly being quite high, too.

The most critical aspect of this regime was the uncertainty in both the timing and contents of judges' deliberations, whose inclinations are deemed to have varied quite a lot across both time and space⁵. The widespread delays in the Italian civil justice are also likely to have compounded the inefficiencies as workers were lacking any income support (and pay) pending the litigation, whereas firms could end up having to pay quite high wage arrears if finally succumbing.

Most of the analysis made in Italy focused on the immediate effect of the regime discontinuity around the 15 employees threshold. There is evidence that the potentially higher costs affecting firms with 15+ employees may have somehow refrained firms' growth when firms happened to approach the threshold (see Schivardi and Torrini, 2008). The effects, while statistically significant, were not economically very relevant, as the discontinuity in the firms' size distribution around the threshold is not as marked as the sharp discontinuity taking place for instance in France around the 50 employees threshold, relevant for the rules concerning the role of unions at the firm level (see e.g. Garicano, Lelarge and Van Reenen, 2013; Gourio and Roys, 2014). Such a limited impact may however be due to measurement error in measuring firms' size, as actually there were ways to circumvent the stringency of the threshold, for instance by employing apprentices or other temporary workers whose presence in firms' workforce did not concur to determine the 15-employees threshold. Moreover, the general equilibrium implications of such a restraint to firms' growth might have been wider than what appears around the relevant threshold. This is the argument of Rodano, Rosolia and Scoccianti (2016) who show that removing uncertainty about expected dismissal costs could entail effects on firms' growth along the entire firms size distribution, whose relevance is likely to be compounded by the presence of other allocative distortions (they focus upon credit access distortions).

Removing these potentially high firing costs has been an hotly debated issue in Italy for over 20 years. Some attempts were made by both the first D'Alema government in 2000 and, more prominently, the second Berlusconi government, in 2002. The latter's policy initiative was

⁵ See Ichino (1996).

stopped by the strong reactions of the biggest union, the CGIL, exploiting the reinstatement principle stated by the art. 18 of law 300/1970 as a successful flagship.

Both the Fornero Reform (law no. 92/2012) and more recently the Jobs Act (decree no. 183/ 2014) were aimed at reducing the uncertainty about firing costs in 15+ firms, while leaving fair dismissals costless. The Fornero law attempted to better fix the existing procedures: litigants were channeled into a conciliatory procedure so as to prevent lengthy judicial litigations; restrained the possibility of reinstatement in the case of unfair dismissals for disciplinary or economic reasons; and provided an upper bound to the monetary firing costs (24 monthly pay) whenever these had to be applied. At the same time, the Fornero reform relaxed some constraints in the use of fixed-term job contracts but introduced limits to the number of their renewals. These new limits were highly criticized also because of their possible negative impact during a deep recession. For this reason in 2013 and then again in 2014 (Decree Laws 76/2013 and 34/2014) the government progressively relaxed them.

Concerning firing costs the Fornero reform still left much room to the judges in determining both the fairness of the dismissal and its consequences for firms. For this reason the new Renzi government made a second attempt to reduce firing costs, with the so-called “Jobs Act”, reinforcing significantly some elements and renouncing to the custom to buy in unions’ consent. The Jobs Act further restrained the possibility of workers’ reinstatement, limiting it to discriminatory dismissals and to a few specific cases of disciplinary dismissals, and dictating, as a general rule, that unfair dismissals have to be compensated by disbursing an amount of money strictly predetermined by the law proportionally to job tenure (from a minimum of 4 times the monthly pay to a maximum of 24 times, i.e. 2 monthly pay every year of seniority). Such a monetary compensation may be halved if the worker accepts to end any pending litigation about the nature of the dismissal, the amount so transacted, for a temporary period (up to 2018), being cashed tax free by the worker.

Differently from the Fornero reform, which covered the stock of permanent employees, the newly established rules apply only to the permanent contracts signed after March, 7th, 2015, when the new law came into force. The new law applies also to all open-ended contracts signed by firms with more than 15 employees (or by those which by the new hires would be overcoming the 15+ threshold). Existing employees (as well as the newly signed permanent

contracts in firms staying below the 15 employees threshold) remain covered by the previous law arrangements.⁶

All in all, the new regime reduces both the expected firing cost and, most significantly, the amount of uncertainty surrounding it for firms over the 15 employees threshold, with no change for those below the threshold⁷. It is not easy, however, to quantify the reduction in uncertainty in monetary terms; even assuming as a benchmark the case of risk neutral firms, it may depend upon the behaviour of local courts in terms of both judgment orientation and length of trials. The quantification of the reduction in firing costs may be further complicated by the presence of the above mentioned temporary regime with the tax free transaction option. This may have two different possible implications: on the one hand it directly reduces the cost for a firm of a dismissal which will be deemed as unfair by the judge; on the other hand it may stimulate the worker to resist (and the judge to deem as unfair) a dismissal which according to the now prevailing judgement criteria would be considered as fair, as its presence may provide for a low cost alternative to the full lack of compensation in case of no resistance (and no unfairness decision).

The government also introduced a very generous non-conditional hiring incentive. The incentive, established by the Financial Stability Law for 2015 (but already announced at the end of October 2014) was covering all new permanent workers hired by any firm from January to December 2015, provided the worker did not have a permanent contract in the previous 6 months. The incentive is a three-year exemption from social security contributions up to a threshold, which is quite high compared with the average contributions typically paid by firms to workers (according to the government's estimates the incentive should fully cover the social security contributions of almost 80 percent of new hires). Notice that also conversions from fixed-term to permanent job contract within a given firm are subsidized (conversions from apprenticeship are instead excluded as they benefit from an ordinary subsidy which is still in place).

⁶ The Jobs Act states that for firms with less than 15 employees firing costs cannot exceed 6 monthly pay with a minimum of 2 monthly pay (one per year of seniority). Before the Jobs Act firing costs varied from 2.5 to 6 monthly pay. Differently from Fana, Guarascio and Cirillo (2015) we believe that this change is negligible.

⁷ To be more precise, for those approaching the 15 employees threshold there is a reduction in the costs implied by overcoming the threshold as they are exempted from the implications of overpassing it. This is why, differently from Schivardi and Torrini (2008) we are considering not only firms close to the threshold in analysing the possible effects of the reform. For firms close to the threshold (say firms with 14 employees) the relevant discontinuity would be that with firms also below but much farther away from the threshold (say firms with 1 to 5 employees), for which no change was implied by the new law. However, focusing upon such a comparison would have implied looking at a much smaller subsample.

The two policy measures undertaken in 2015 and analysed in this paper almost overlap, because both target permanent hires and job contracts conversions from fixed-term into open-ended. There is, however, a small difference in their timing: from January 2015 for the incentive, from March 7th, 2015 for new firing costs. Moreover, there are some differences in the population targeted by the two policies that can be used to separately identify their effects. Incentives are paid to firms of any size, while the CTC applies to firms with at least 15 employees; the incentives PHI apply only to workers with no permanent contract in the previous 6 months, while the previous status of the worker is irrelevant for the application of the CTC. Thus, information about firm size and workers' past work histories, together with the precise date of the new contract, allow for separate identification of the effect of the two policies.

It has to be noticed that the fixed amount of the hiring incentive for all new permanent contracts signed in 2015 leaves a lot of room for firms' "strategic" behavior, since a firm may cash in the same amount for permanent contracts signed in both January 2015 and December 2015 (including conversions from a temporary to a permanent contract). It is well known that in a dualistic labour market structure, many temporary hiring are explained by a screening motivation. Whenever the quality of the match is uncertain – for instance because the worker is unknown to the firm - the latter may opt for a low risk path, as the worker may be hired on a temporary basis, tested and eventually later on transformed into a permanent position. Given the features of the PHI, such a strategy may be still preferable, as offering a temporary contract to an unknown candidate does not destroy her hiring incentive eligibility status. The temporary job contract still allows the firm to test for worker's skills and leaves unchanged the option to convert the fixed-term position into an open-ended one, benefitting from the PHI if the conversion occurs before December 31st, 2015. In contrast, whenever the match with a given worker is positively valued by the firm because she was already known (for instance because she had held a temporary contract position in the past), there is an incentive to immediately offer a permanent contract. In this way the firm cashes in the incentive and prevents the risk that someone else offers the worker a permanent contract, removing her from the pool of workers eligible for the incentive.

The CTC may have an opposite effect, increasing the propensity to hire a worker unknown to the firm, insofar as the CTC has reduced the expected cost of firing her.

This has two consequences. First, the impact of both policies depends upon the presence of a previous knowledge of the worker by the firm⁸. Second, the PHI may boost not only permanent hiring (and conversions from fixed term to open ended contracts) but also temporary contract hiring, especially in those cases where testing workers' skills is relevant. This implies that the effect of the two policy measures introduced in 2015 may not be easily assessed by comparing the changes in the number of newly signed open-ended contracts and new temporary job contract⁹. This issue, however, is more extensively discussed in the next two sections.

3. The data and some evidence

In this paper we use administrative microdata about the so-called *Comunicazioni Obbligatorie*. In Italy all occurrences concerning a job position must be electronically notified to the Regional agencies in charge of active labour market policies (and also made accessible to the Italian social security institute, INPS). Microdata archives, which cover only employees in the private sector and part of the public sector, are collected and organized by each Italian region. Potentially, the database registers when the position is created, destroyed, converted from fixed-term to permanent, or whether the duration of a fixed-term contract is extended. Because of the decentralization of the data-collection process, the quality of the micro data differ quite a lot across the different regions. Our data refer to one Italian region, Veneto, characterized by high quality and high timeliness. Veneto is located in the North-Eastern part of the country. The weight of manufacturing in total economy is among the highest in the country, as well as labour market participation. On average, the weight of this region in total dependent employment is around 8 percent (excluding agriculture and Public Administration).

Our dataset contains information regarding all events (hiring, firing, conversion and fixed-term contract prolonged duration) occurred in Veneto between January 2013 and June 2015. This time span has been chosen because of data availability. Nevertheless, the exclusion

⁸ In a fully developed model, one should take care not only of the presence of a previous knowledge between the firm and the worker - which is a variable we may empirically proxy by looking at the presence of previous job spells of a given worker in a given firm - but also of the local labour market environment and the length of the job applicants pool which is available to each firm. The risk of having a worker transiting into a temporary position is that some other firms might offer her a permanent position removing her from the pool of those eligible to the PHI. The fact that our estimates look at only a few months and a unique region should allow us to neglect these further elements from the analysis.

⁹ The creation of temporary job contracts might be affected insofar as there are fraudulent arrangements between the worker and the hiring firm in order to "build up" the eligibility status of the worker by passing through a temporary contract. While - as shown by Veneto Lavoro (2015) - clearly fraudulent cases are present in the data, we believe that the quantitative amount of the extreme case here described is contained by the risk that the worker would have to bear by renouncing to a permanent contract in order to get the promise to obtain a new permanent contract in a different firm in 6-month time. As a matter of fact (see section 4) the job to job moves flow has remained quite unchanged over time.

of events after June 2015 allows us to avoid the potential bias induced by at least two confounding factors. The first is that in June 2015 the government introduced new limits to the use of collaborators, with potentially indirect effects on the use of other types of job contracts. The second is that in October 2015 the government announced its intention to extend the PHI also to 2016. The amount of the incentive however has been reduced (from 100% to no more than 40% of total social security contributions) and the duration has been shortened (from 3 to 2 years).

For each event recorded in our dataset it is possible to uniquely identify both the firm and the worker involved. On top of the relevant anonymized (firm and worker) identifiers, we know the firm's size¹⁰ (by size class) and sector of the activity and the worker's gender, birth-date, nationality. For each event we know the type of job contract, i.e. whether permanent or fixed-term. The latter group includes: (i) standard fixed-term dependent contracts, (ii) agency workers, (iii) apprentices¹¹, (iv) collaboration workers (so-called *parasubordinati*, i.e. a sort of consultants employed on a temporary basis) and (v) internships (so called *tirocini-formativi*). For each job contract we also know the relevant dates (day, month, year). Moreover, for each worker, we have information also on past work experiences and we may reconstruct whether the worker is eligible for the PHI.

From our dataset we exclude job transitions of domestic workers hired by households, public sector workers and those in the agricultural sector, as they are not subject to the policy measures. We also exclude job transitions in the touristic sector, as the very high seasonality of employment in this sector cannot be fully captured by a 3-year sample as the one we are using in this paper.¹² After this selection our dataset includes around 2,3 million episodes involving 800,000 workers and almost 150,000 firms.

In this paper we use three dimensions of our dataset. First, we use the individual-level panel dimension of our dataset, recording the working status of individuals month by month. We randomly select 50,000 individuals among those with at least one occurrence from January 2013 to June 2015. Thus, for each individual we know whether he/she is not or is working in a given month, the contract type, the size of the firm (in case of hiring, conversion and job separation) and past work experiences.

¹⁰ The size of the firm is not directly communicated at the time of hiring/firing/conversion, but can be recovered by other compulsory reporting required by the law about legally protected workers (Law 68/1999).

¹¹ To be more precise, since 2008 apprenticeship in Italy are considered by the law as open-ended contracts (they were previously considered as fixed-term). However, since no firing cost is associated to them we classify them as fixed-term.

¹² Indeed, past work histories are calculated by considering all the sectors where workers were employed before the current episode, i.e. also job spells in the touristic sector.

Second, we also consider the firm-level panel dimension implicit in our dataset, i.e. the monthly flows (hiring and net job creation) originated by each firm in our dataset. We randomly select a closed panel of 5,000 firms followed from January 2013 to June 2015, among those firms hired at least one worker from 2013 to 2015. In this way we exclude firms entering or exiting the labour market during the period under consideration. For each firm we calculate cell-level flows. Each cell is defined by the intersection of year, month, the 6 types of job contracts and workers' past-working status, i.e. whether employed/non-employed with a permanent job contract in the previous 6 months. In what follows, for simplicity, we label the latter workers as "eligible for the incentive", independently on the year in which we observe them (i.e. even if we observe them in 2013 and 2014, when no incentive was in place for them). In other words, being "eligible" at the time of hiring means that the worker was not employed on a permanent basis in the previous semester. In case of contract conversions the "eligible" group excludes people with an apprenticeship job contract as these conversions were not covered by the PHI.

We calculate both hiring and net job creation at the firm-cell level. Net job creation is defined as the difference between hiring and job separations. We consider all types of job destruction, e.g. firing, workers' voluntary separation, retirement.¹³

Last, for the descriptive analysis, we consider aggregate flows. In Table 1 we report the total number of hires in each semester from 2013 to the first semester of 2015. In Table 2 we report the main socio-demographic characteristics (share of men, average age, share of "eligible") of workers involved in the various labour market flows, which indeed are quite similar across different groups (also when calculating each share separately for each year).

Figure 1 plots hires and aggregate net job creation, from January 2013 to June 2015, for both open-ended and fixed-term job contracts. In January 2015 both the number of new open-ended and fixed-term contracts increased considerably, and then declined, even if open-ended contracts remained at a level remarkably higher than the one registered in the corresponding periods of 2013 and 2014. The total net flow of newly created job contracts was positive for both types of contracts (bottom panel), even if at the end of the period it was positive for fixed-term job contracts only.

In 2015 also conversions from fixed-term to open-ended increased considerably, as shown by Figure 2. They increased both in absolute terms (dashed line) and relative to the number of fixed-term positions (solid line), peaking in April 2015.

¹³ As for hiring, the eligibility condition for people separated from their job -necessary to compute cell-level data- is defined by looking at the labour market status at the time of hiring.

As already mentioned in Section 2, firms have the possibility to hire a worker on a fixed-term basis, test her skills and then convert the contract into an open-ended (still benefitting for the incentives and the CTC). This two-step strategy is in principle always convenient for the firm whenever the worker has not a better alternative (and insofar as the PHI does not decrease or vanish, as after December 2015). The only risk for the firm stems from the fact that the worker may have a better alternative and, if offered a permanent contract by another firm, may disappear from the pool of eligible workers. Given that we are analysing a single region and a relatively short period of time, we are not attempting to take account of these “local labour market” effects. What we may however consider is the fact that, *ceteris paribus*, the propensity to use such a two-step strategy is larger whenever the firm does not know the worker’s characteristics. We proxy such a feature by distinguishing between two types of matches between workers and firms: those involving workers who were already employed in the same firm in the past (we label this group as “known workers”) and those who are matched with a firm for the first time (workers “unknown to the firm”). The upper part of Figure 3 plots the flow of new job contracts, by worker-firm relationship. The solid line refers to contracts involving workers and firms who were never matched before; the dashed line refers to matches involving workers already known to the firm. The flow of new hires of known workers increased considerably in January 2015, when incentives were introduced and then declined. The flow of new hires involving unknown workers peaked instead in March 2015 and increased also after April. The bottom part of Figure 3 plots the share of known workers in total hires of permanent workers and confirms that after the introduction of the PHI in January 2015 firms preferred to hire workers whose skills were already tested in the past.

As a first step, we first check differences in trends before and after the inception of the two policies, by comparing eligible vs. non eligible workers (in Figure 4) and firms of different size (in Figure 5), independently of the job contract type. Even if the comparisons are affected by monthly seasonality, in Figure 4 the difference in the trends of eligible and non-eligible workers is never significant before January 2015 and then differs afterwards. In Figure 5, instead, we do not find clear-cut evidence of a change in trends after the inception of the Jobs Act in March 2015.

Notice however that the presence of the above depicted two-step strategy implies that the number (or the share) of new hiring (or conversion from fixed-term contracts) into permanent positions is not a sufficient statistics to be looked at, as the policy interventions might have boosted also temporary hiring. To identify the effects of the two policies, we need to identify a

“control” or non-treated group of events totally unaffected by them. This means focusing upon events in small firms (as such unaffected by the CTC) by workers who were neither immediately nor prospectively eligible for the PHI. People belonging to such control group are the workers transiting from a permanent position in a small firm to another permanent contract in the same firm class-size. Their job-to-job transitions are driven by the materialisation of a better match, mostly because of idiosyncratic reasons unrelated to any labour demand shifts related to the PHI or the CTC. The evolution of these flows might be due to business cycle features, as a more buoyant economy normally triggers them. But this is precisely what one would like to control for by comparing the evolution of the other flows, potentially affected by the two policies, and the control group evolution.

Thus, in Figure 6 we report the flow of hiring of non-eligible workers in small firms (which, as reported in Table 1, amounts to around 5 percent of total hires in all the years here considered) and the difference between its trend and the trends in some other relevant flows. More in detail, panel (a) of Figure 6 reports the difference in trends of eligible and non-eligible workers in small firms. Panel (b) reports the difference in trends of small and large firms in the group of eligible workers. Panel (c) and (d) respectively report and compares the trend of non-eligible hires in small firms and all other hires (of eligible workers in both small and large firms and of both eligible and non-eligible workers in large firms). In panels (a) and (d) the existence of a difference in trends emerges clearly after January 2015. In panel (b), where the effect of the CTC is calculated by controlling for eligibility, together with some effect probably due to seasonality, there is also significant evidence of a change in trends in the last months of the period, i.e. after March 2015. Last, to check for the existence of difference in trends of non-eligible workers in small firms after January-March 2015 we look at job-to-job transitions of individuals, by controlling for individual fixed effects and we do not find any relevant difference in trends (estimates available upon request).

4. The estimation strategy

The charts in previous sections show that there are discontinuities in the trends of hiring along some of the dimensions affected by the two policy measures here considered, and that the picture is very complex, as the policy measures may have impacted more than one labour market flow. In particular, the two-step strategy of offering a temporary position first and then convert it into a permanent contract – so as to test the worker’s skill and still cashing in the whole amount of the PHI – may have boosted also temporary contracts. Broadly speaking, a

possible measure of the counterfactual business cycle evolution unrelated to the two measures may be obtained by focusing upon those labour market flows unaffected by the two measures, permanent hires of non-eligible workers from January to February 2015, and permanent hires of non-eligible workers in small firms from March 2015 on.

In order to get a more precise and quantitative assessment of the effects of the two policies we estimate a diff-in-diff model at two different levels: individuals¹⁴ and firms.

The first exercise looks just at conversions of fixed term contracts into open-ended. This is a flow directly affected by both policies, although along different lines, depending upon the workers' previous history (defining the eligibility to the PHI) and the firm size (relevant for the CTC). More specifically, we will look at the probability, for a worker who is holding either an apprentice or a standard fixed-term contract, to have her contract converted into an open-ended contract in the same firm¹⁵. More formally we define a dummy variable π equal to 1 if the fixed-term position is converted into an open-ended position and we estimate:

$$[1] \quad \pi_{pgwym} = \gamma_p + \gamma_g + \gamma_w + \gamma_y + \gamma_m + \beta D_{(w=1)(y \geq 2015)} + \delta D_{(g=15+)(y \geq 2015)(m \geq \text{March})} + \epsilon_{pwym}$$

where γ_p are individual fixed effects, aimed at capturing workers' unobserved heterogeneity, γ_g , γ_w , γ_y , γ_m are respectively fixed effects for firm's size-class (g), for worker's not having a permanent job in the previous semester (w), for year (y) and month (m). The variable $D_{(w=1)(y \geq 2015)}$ is a dummy equal to 1 if the observation refers to the period from January 2015 onwards and involves workers eligible for the hiring incentive. The dummy $D_{(g=15+)(y \geq 2015)(m \geq \text{March})}$ is a dummy equal to 1 if the observation refers a 15+ firm in the period following the inception of the CTC. Year and month dummies take account of time trends. In equation [1] the control group includes conversions taking place to non-eligible workers (those employed with a permanent job contract in the previous 6 months or those with an apprenticeship job contract); conversions in small firms after March 2015, represent the control group to identify the effect of the CTC.

In a second exercise we look at workers' probability of obtaining a permanent job. Our sample includes all people "at risk" of finding a permanent position, i.e. both temporary workers and all people currently jobless.¹⁶ Notice that when considering individuals, we may only

¹⁴ We do not observe people who have never get an employment (the ones remaining unemployed or out of the labor force) and those who, employed over a permanent contract, have never changed their status since 2011.

¹⁵ Also at the cell level we consider only the conversions stemming from apprentices and standard fixed-term contracts taking place in a given firm.

¹⁶ But who have had at least 1 hiring or separation since 2012.

recover the effects of the PHI¹⁷ (as firm size is a characteristics of the firm and not of the worker). We then define a variable π_{pwym} which is equal to one if the worker p -th is hired with a permanent job contract in year y and month m and is equal to zero otherwise (in case of no employment or if the job contract is a fixed-term). The other indices are defined as in equation [1]. We then estimate the following model:

$$[1'] \quad \pi_{pwym} = \gamma_p + \gamma_w + \gamma_y + \gamma_m + \beta D_{(w=1)(y \geq 2015)} + \epsilon_{pwym}$$

where we use the same notation as in equation [1]. Moreover, since we have a panel of individuals and the variable π_{pwym} typically does not change after a person has found a permanent job, we drop the worker from the sample after she finds a permanent job (unless she after some time she re-enters the pool of job seekers because separated from her job).¹⁸ Fully exploiting the worker's past history, we can also split the sample among those who are not employed at a given time and search for a job and those who have a fixed-term job.

The third exercise looks at a similar phenomenon, but from the firm's perspective: permanent gross hiring. Also this flow is affected by the two policies along different dimensions, depending upon the workers' previous history (defining the eligibility to the PHI), and the firm size (relevant for the CTC). Again, the evolution of the flow of non-eligible workers and in small firms provides the relevant control group. More formally, for each firm we identify a number of cells defined by the intersection of 30 months, 2 worker's past conditions (depending upon the workers' history during the previous semester and defining eligibility from January 2015 onwards) and 6 contractual types (open-ended contracts, standard fixed-term dependent contracts, agency workers, apprentices, collaboration workers and internships).

We then estimate:

$$[2] \quad n_{igofwym} = \gamma_i + \gamma_g + \gamma_o + \gamma_f + \gamma_w + \gamma_y + \gamma_m + \\ + \beta_1 D_{(o)(w=1)(y \geq 2015)} + \beta_2 D_{(f)(w=1)(y \geq 2015)} \\ + \delta_1 D_{(g=15+)(o)(y \geq 2015)(m \geq \text{March})} + \delta_2 D_{(g=15+)(f)(y \geq 2015)(m \geq \text{March})} + \epsilon_{igofwym}$$

where γ_i indicates firm i -th fixed effects, γ_o is the fixed effect for permanent job contract o , and γ_f is a set of fixed effects, one for each of the 5 types of fixed-term contracts (the other fixed effects are the same as in equation [1]). The variable $D_{(o)(w=1)(y \geq 2015)}$ is a dummy equal

¹⁷ Actually, the job finding rate is the sum of two events, finding a job in a small firm and finding it in a large firm and what one could look at are these two components, one affected only by the PHI and the other one affected also by the CTC.

¹⁸ A more suitable, but rather complex estimation model could be a panel duration model.

to 1 if the cell corresponds to permanent hiring occurred from January 2015 on, and involving eligible workers (and 0 otherwise). The dummy $D_{(g=15+)(o)(y \geq 2015)(m \geq \text{March})}$ is a dummy equal to 1 if the cell refers to permanent hiring occurred after the inception of the Jobs Act, in a 15+ firm (independently on eligibility). $D_{(f)(w=1)(y \geq 2015)}$ and $D_{(g=15+)(f)(y \geq 2015)(m \geq \text{March})}$, refer to fixed-term contracts. The first is equal to 1 if the cell refers to gross hiring of person not previously employed as permanent workers ($w=1$), occurred after January, 1st. The second refers to fixed-term hiring in firms with at least 15 employees, occurred after March, 7th 2015. So, in equation [2] β_1 and δ_1 identify the direct effect of the two policies (i.e. the effect on open-ended contracts), while the terms β_2 and δ_2 (which indeed are further distinct by type of temporary job contract) capture substitution or complementarities induced by the policies on other types of contracts. As before, month and year dummies capture business cycle and seasonality affecting permanent hiring of non-eligible workers in small firms. Notice that in the equation we may also control for firm's fixed effect, γ_i , disaggregating the class size dummy as such used for identifying the CTC effects. Further, in this exercise we may also control for the distinction between workers who are already known to the firm (i.e. already screened by the firm in the past) and those who are not, so as to verify whether the PHI, and the CTC as well, have different effects on the two groups.

The last and final exercise tries to get together the firm's and worker's sides of the market by looking at the changes in employment (net hiring). We estimate equation [2] but the dependent variable is now net hiring at the firm level, equal to hiring *minus* firing.

5. The results

5.1 Temporary to open-ended contract conversions

We start in Table 3 with conversions from fixed-term to open-ended contracts. Notice that the inclusion of worker fixed effects allows us to control for the fact that many people have had several temporary employment spells (and practically all of them have had the chance of being converted to a permanent contract over several months). The effect of hiring incentive PHI is positive and sizeable in column 1. The effect of the CTC is positive and significant in column 2 and also in column 3, when the interaction with the PHI is included. In the last case, however, the interaction is not different from zero, suggesting a larger effect of the CTC for

non-eligible workers, namely apprentices. All the effects are sizable when compared to the average size of the monthly probabilities of conversion, reported in the last row of the table.¹⁹

5.2 Permanent contract gross hiring

We now look at permanent hiring from both the perspective of the worker and the perspective of the firm. Table 4 presents the results for the probability that an unemployed or a fixed-term worker obtains a permanent job position, given her characteristics. The first column of Table 4 refers to all individuals (i.e. non-working or working under a fixed-term job contract), Columns 2 and 3 report the same estimates distinguishing workers by their initial status (out of employment, working with a fixed-term contract); so, column 2 identifies the effect of incentives on the flow from non-employment into permanent employment, while column 3 is the worker's perspective about the conversion rate from temporary to open ended contracts already examined in table 3 (however and differently from that table, here also passages into permanent employment implying a change from one firm to another one are included in the count). The results confirm the positive effect of incentives, which is extremely large when compared with the average probabilities observed in 2013/14 (reported in the last row).

We then estimate equation [2], which refers to gross hiring made at the firm level. The inclusion of firm fixed effects allows us to control for average firm-level hiring, which can depend on unobservable firm-specific characteristics. In some specifications we also split the dummies $D_{(o)(w=1)(y \geq 2015)}$ and $D_{(g=15+)(o)(y \geq 2015)(m \geq \text{March})}$ into two groups to identify the effect of the two policies on workers already "known" to the firm, i.e. those for which there is lower uncertainty about the goodness of the job match. The results of this exercise are reported in Table 5. Notice that the effect of the two policies is obtained by using hiring of non-eligible workers in small firms as a control group (firm size matters from March 2015 onwards).

Both policies were legislated before being actually implemented. So, our identification strategy based on the timing of their implementation might fail whenever firms have strategically moved back and forth already planned hiring in order to exploit the announced

¹⁹ As a robustness check in Table A1 we report the same estimates as in equation [2], but estimated at the cell-level. The dependent variable refers to the number of conversion from fixed-term and apprenticeship to open-ended contract. The first column refers to conversion of non-eligible workers (so the cells under exam are 60 out of the total 120) and it is aimed at identifying the effect of the CTC by comparing cells pertaining to firms above and below the 15 employees threshold. Non-eligible workers are those with an apprenticeship job contract or were employed with a permanent job contract in the previous semester. The effect of CTC, mostly identified by transitions from apprenticeship, appears to be positive and statistically significant. The second column includes also the cells measuring the conversions involving workers who did not have a permanent contract in the previous semester (whose conversion does allow to cash in the PHI). It allows to identify separately the effect of the CTC and PHI and their interaction. While positive, the CTC still positive and close to be significant (p-value equal to 13 percent), while both the PHI and the interaction terms are positive and statistically significant at standard levels.

policies. In order to take account of such a possibility we exploit the fact that the PHI was known since October 2014 and the CTC since December 2014. So the possible anticipation effect of the PHI is captured by a dummy equal for the observations related to eligible workers in the time window from October 2014 to January 2015 (and for the CTC for the time window December 2014 to March 2015). Since the two policies can affect hiring strategies not only with respect to open-ended contracts but also with respect to all the other types of contract, these dummies are further interacted with the dummies capturing the type of job contract.²⁰

The first column reports the effect of incentives only, which, is positive and highly significant. As expected the dummy capturing possible anticipating behaviour of firms is negative and significant, but smaller in absolute terms, suggesting that hiring incentives indeed had a total positive impact on hiring. The second column adds the dummy capturing the effect of the CTC which is positive, significant but remarkably smaller than the effect of hiring incentives. The own effect of CTC vanishes when we consider the interaction between the two policies, as in column 3, while the interaction term is quite large. So, most of the CTC effect appears to have acted by strengthening the effect of the PHI in the large firms segment.

Column 4 and 5 split the effect of incentives PHI (column 4) and CTC (column 5) by type of worker, i.e. whether “known” or “unknown” to the hiring firm. PHI boosted the chance for both workers, but, as expected on the basis of our discussion of the two steps strategy for exploiting the PHI, the increase is larger for the “known” ones²¹. As expected, lower firing costs boosted permanent hiring of those workers who were unknown by the firm as the CTC reduced the costs of breaking a bad job match and made the 15+ firms less selective and less reluctant in their permanent hiring. The last column of the Table considers instead total hiring net of job-to-job flows. Job-to-job flows are identified as those flows of workers with a time span smaller than 7 days from a job separation and a new hire. The column confirm the expansionary effect of both policies.

On the basis of these estimates, the last rows of Table 5 report the size of the estimated impact of the two policies. According to our estimates around 40% of total permanent hires occurred in the first semester of 2015 are due to hiring incentive. These hires correspond to around 20% of total hires in the period. The effect of the CTC, while sizable, is quantitatively smaller and equal to 5% of total hires of permanent contracts and 1% of total hires (including

²⁰ For instance, at the time of the announcement of the policies a firm could hire temporary workers and then convert their contract into an open-ended position after the inception of the law.

²¹ The coefficients of the interaction term between “known workers” and the fixed term contract types are reported in Table A2 in the Appendix.

also other types of contracts). When excluding job-to-job flows the impact of the two policies on newly created jobs is even higher.

5.3 The employment dynamics

We now look at net job creation at the firm level, as defined by gross hires minus job separations²². Following equation [2], n_{gofwym} now represents the number of jobs created in each cell net of the number of jobs destructed in the same cell. The estimated coefficients, reported in Table 6, represent the increase in the average size of the cell, due to the policies.

The results of the first column indicate a positive impact of hiring incentives (PHI), even when controlling for possible effects of anticipation of the policy measure, which reduced net job creation in the last quarter of 2014, as firms strategically postponed hiring in the first months of 2015 to get the incentives.²³ The second column of the Table includes also the direct effect of the CTC (i.e. the coefficient of the dummy $D_{(g=15+)(o)(y \geq 2015)(m \geq \text{March})}$), which is positive. The term capturing possible anticipation of the CTC is instead not statistically different from zero. The third column includes both the direct effect of the two policies and the interaction between the two. The results are similar to what already presented in Table 5 for the hires: the own effect of the CTC (i.e. what would have happened in the absence of hiring incentives) becomes statistically significant (and negative) as most of the actions appear as a strengthening of the incentive in the 15+ firms segment thanks to the CTC (the interaction term is largely positive and statistically significant).

As already mentioned, our main firm-level sample is a closed panel of firms with at least a labour market episode in each of the years from 2013 to 2015. As a robustness check, in the last column we carry out the same exercise as in column 3 but on a unbalanced panel of 50,000 randomly selected firms, which includes firms which register at least one change in their workforce during the period here analysed.²⁴ In this way we are able to capture the potential bias induced by firms entering (or exiting) the market because of the policies. In fact, PHI could have induced many small firms without employees to hire employees for the first time, an effect

²² Notice that contract conversions do not contribute to net job creation as the additional open ended contracts are counterbalanced by the reduction in temporary contracts, which are converted into open ended contracts. The terminations of temporary contracts, insofar as they are not converted into open ended positions in the same firm, are taken into account as a negative component of net job creation.

²³ The sample size of the first two columns is half of the sample size of the third column as we do not distinguish cells by size of firms, as we do not estimate the effect of the Jobs Act.

²⁴ In this case not the firm fixed effect is not identified for all firms, but contributes to the estimate of the average flow.

that cannot be captured by a closed panel as the one used in columns 1-3. The results of this exercise are fully consistent with the ones presented in the first columns.

To assess the quantitative relevance of these results, as in Table 5, in the bottom part of the Table we report some back-of-the-envelope calculation of the impact of the two policies on net job creation. According to our estimates, workers hired because of PHI account for more than 40% of total net flow of permanent workers, while the effect the Jobs Act is smaller (around 5%). They also account for 35% and 4% of the net flow of newly created dependent employment positions, in all the specifications (also when the unbalanced panel is considered). Moreover, if we look at the difference between the net flow in 2015 and the average net flow in 2013-14, we find that this difference is totally explained by the two policies.

It is not possible to extrapolate these results to Italy as a whole, as both the underlying trends and composition effects may differ by geographical area. Focusing on Veneto, our estimates imply that in the first semester of 2015 both policies increased the number of people employed with a permanent job contract by 0.7 percent.²⁵

We have carried out several robustness checks. First, since also the apprenticeship job contract has been subject to several legislative changes during the period under analysis, we have carried out the same regressions presented in Table 5 and Table 6, but excluding apprentices. Results, available upon request, are unaffected. As an additional check, in some estimates we exclude from the sample those firms around the 15-employees threshold, as their behaviour might be impacted by the CTC through additional channels. Also in this case the results are qualitatively similar.

6. Conclusions

In this paper, using a diff-in-diff strategy, we analyse the reaction of firms to two policies introduced in the first part of 2015 by the Italian government, aimed at both reducing labour market dualism and favouring job creation. The first is a generous hiring incentive to firms offering open-ended job contracts, not conditioned to firms' net job creation. The second is the reduction of firing costs for firms with at least 15 employees (not only a monetary reduction, but also a decrease in uncertainty about the consequences of unfair dismissals). We find that the two policies were successful in both reducing dualism and stimulating labour demand, even during a recession period characterised by very high macroeconomic uncertainty.

²⁵ Estimates calculated by applying the net flow of permanent job positions and contract conversions to the average stock of permanent employees in Veneto in 2014. The overall employment effect is smaller (0.5 percent) as part of the above mentioned effect is due to a shift towards permanent employment.

As already said, our estimates do not consider all the relevant aspects relevant in judging the appropriateness of the measures undertaken (see Brown et al., 2011, for a wider theoretical discussion on hiring subsidies). In particular, we do not discuss the pros and cons of the current temporary and rather unselective hiring incentives vis-à-vis the host of often cumbersome but permanent and more selective subsidies targeting specific groups of workers supposed to be weaker and less employable (mostly youths and long term job seekers). Furthermore, we do not deal with the merits and pitfalls of having incentives for gross hiring so that also the normal turnover taking place in a firm is incentivized insofar as people are hired through a permanent contract. The combination of the two elements implies that the policy favours the conversion of temporary contract into permanent ones and possibly the poaching of suitable temporary workers from one firm to another. So, an indirect effect of the hiring incentives might be that of lifting up the temporary hiring of people whose contract is later on transformed into a permanent one. At the moment we can only testify both an increase of the probability to find a permanent position for both non-employed and temporary workers in other firms, and an increase in the temporary-to-permanent contract conversions within the same firm.

Our estimates also fall short of an overall evaluation of the new firing rules introduced by the Jobs Act. As a matter of fact, the CTC has left unchanged the general principle that only dismissals opposed by the worker and considered unfair by a judge have to be compensated. Differently from the previous regime, the reinstatement of the worker, while still possible, applies only to few and better specified cases of unfair dismissals (the ones deemed to be discriminatory). Also the uncertainty concerning the amount of the financial compensation possibly stemming from a judiciary intervention has been considerably lowered as its amount has been capped and pre-specified by the law as an increasing function of worker seniority. Furthermore, the compensation cost for firms has been halved if the worker accepts a transaction, so ending any pending litigation, whose acceptance is favoured as the compensation so obtained is cashed tax free. The new regime is likely to provide more certainty to both the worker and the firm. At least for the time being, i.e. the three years over which the above mentioned tax exemption has been granted, it may induce an higher share of dismissals to end up with some compensation (the risks of high compensation costs has been reduced, but the firm may still find more convenient to avoid any risk at all by offering the tax exempt transaction). Our estimates do not allow to consider all these specific shifts, whose relevance will increase over time as the stock of employees will be increasingly made up by people hired

according to the new rules. Neither we are able to consider to what extent the tax exemption is relevant and the possible effects of its removal in the future.

Furthermore, our estimates do not consider the overall general equilibrium implications of the reduced uncertainty and average amount of the dismissal costs for the firm. Albeit small, the effects of the firing costs reduction might have significant general equilibrium effects, for instance if their allocative effects may cumulate to other market imperfections (e.g. capital market imperfections) in shifting the whole firms' size distribution.

All in all, our estimates are therefore only one of the elements necessary to decide what to do in the future, concerning the possible presence of selective work incentives (i.e. incentives targeted to population groups considered less easily employable) and the tradeoffs between marginal tax wedge reductions (i.e. tax cuts applied only to either gross or net hiring) and tax wedge reductions applied across the board (i.e. inward shifts in the tax schedule facing the whole stock of workers). We however believe that our empirical exercise is a step forward for the comprehension of the reaction of firms to changes in employment protection and in its interaction with changes in other labour cost components.

We show that both measures were effective in both shifting employment towards permanent contract and raising overall employment levels. The predominant component has to be attributed to the sizable incentive provided by the law, with a strengthening of such an impact in the 15+ employees firms thanks to the new CTC which reduced the average cost and the uncertainty concerning possible future dismissals. A relevant longer run effect of the CTC comes from the fact that it made firms less reluctant in hiring on a permanent basis a yet untested worker.

All schemes of incentives provide money for events which economic agents might have decided anyway to put in place, so that their unitary budgetary costs are higher than what formally provided for each individual event. The PHI made no exception to such a rule. There is evidence that firms of shifting back and forth planned hires in order to exploit the timing of the temporary incentive provided by the law and; furthermore, firms acted "strategically" also along other lines as, given the fact that workers eligibility to the PHI was related to the absence of a permanent job in the previous semester, the firms continued to offer only a temporary position to all the yet unknown job applicants, postponing the chance of cashing the full amount of the subsidy to the eventual case of conversion to an open ended contract. Such a behaviour has led to an increase in temporary hires as well, with subsequent conversions to permanent positions (taking place within the December 2015 window). Still, our estimate of the contribution of the

PHI to the total flow of gross permanent hiring allows us to assess that the deadweight loss of the PHI was of about 1 out of two cases. Our estimates of the deadweight loss, however, are only just one element to determine its full monetary value, as other factors may affect the calculation (for instance, labour income taxes paid for each additional permanent job position and savings in unemployment benefits not paid to temporary workers in case of their conversion).

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Tables and Figures

Figure 1: Hiring and net job creation by type of job contract (thousands). (1)



(1) Net job creation is the difference between job created and destroyed.

Figure 2: Number of conversions from fixed-term or apprenticeship job contracts to permanent job contract, and ratio to the total number of hiring with fixed-term or apprenticeship job contracts.

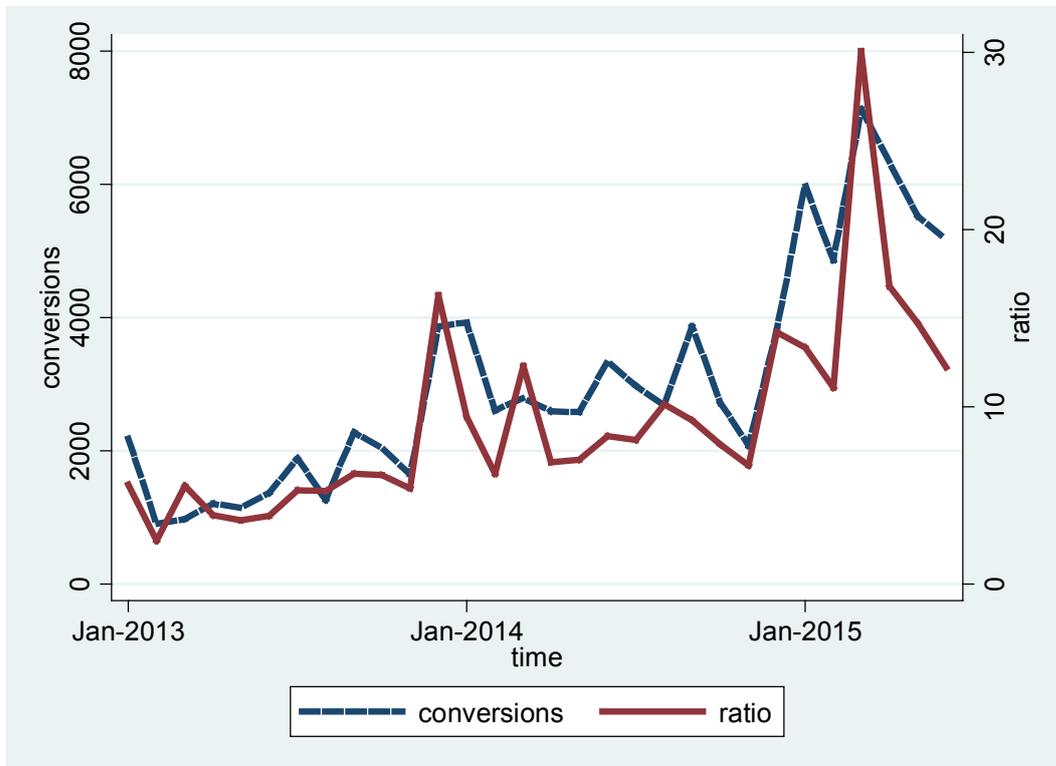


Figure 3 Hiring (all types of contracts) by previous relationship with the worker: known to the firm (i.e. employed in the past in the same firm, in the left-hand scale) and unknown to the firm (i.e. never employed with the firm, in the right-hand scale) in the upper part; share of known workers in total permanent hires in the bottom panel.

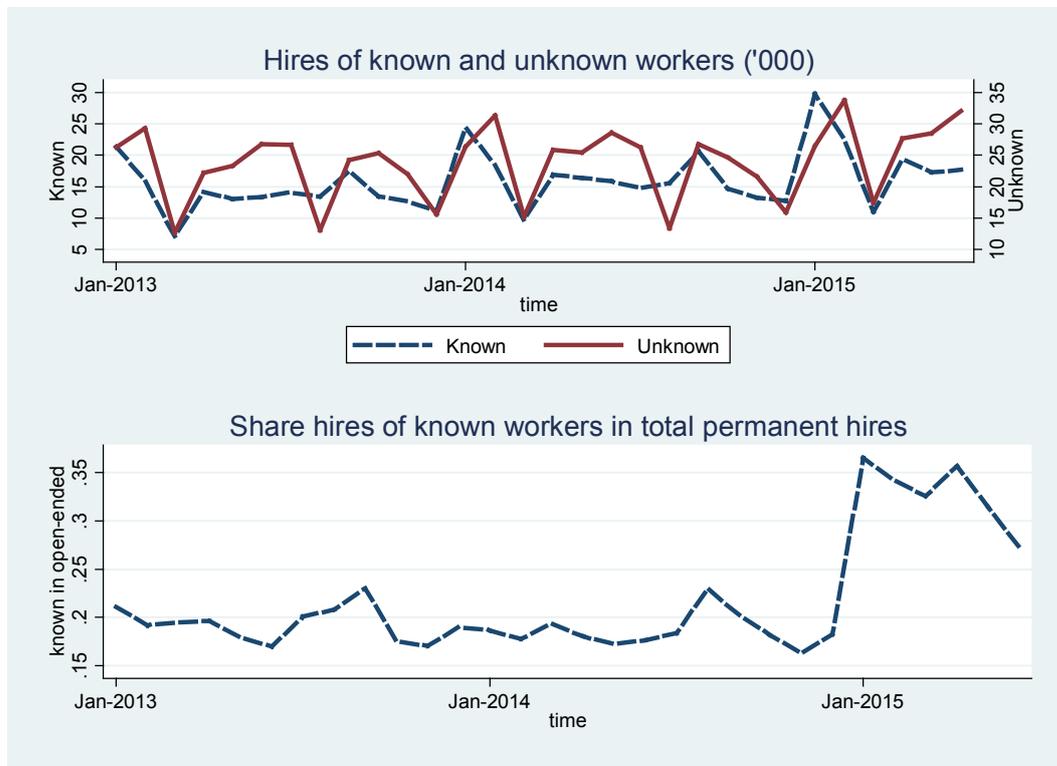
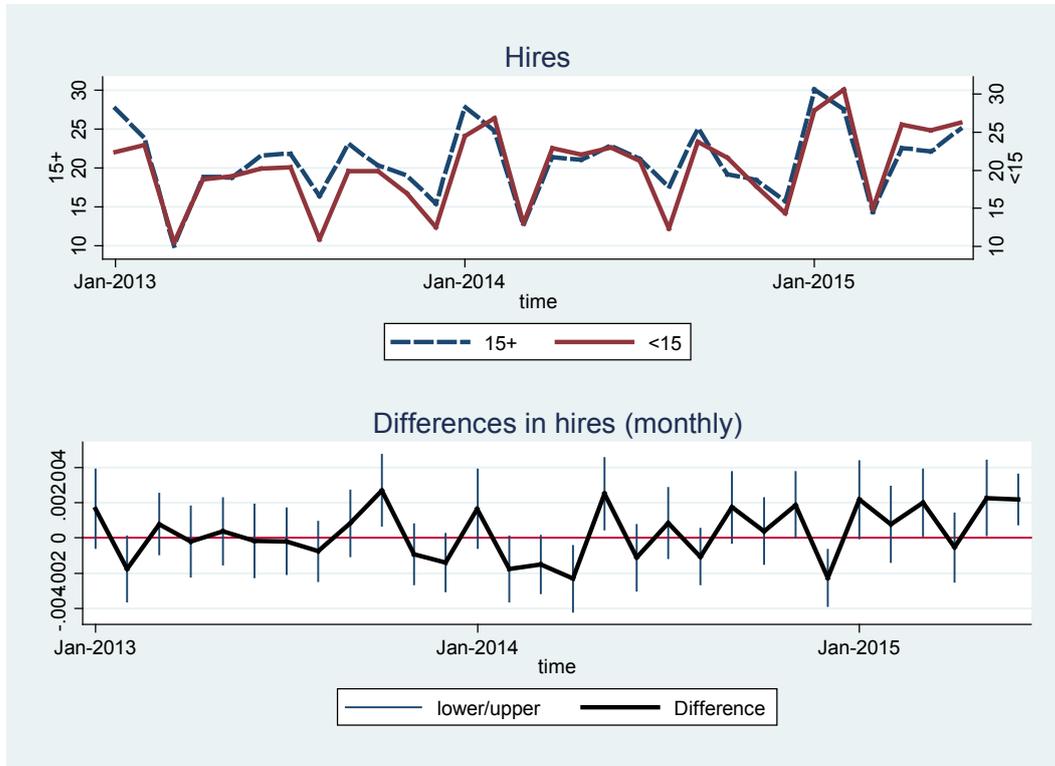


Figure 4: Hires (all types of contracts) by previous employment condition: employed with an open-ended contract in the previous 6 months (i.e. non-eligible for PHI in 2015) and unemployed or employed with a fixed-term job contract in the previous 6 months (eligible for PHI in 2015). Thousands in the upper panel and differences in monthly hires between the two groups in the bottom panel. The small vertical lines represent the confidence intervals. Differences are normalized by using the pre-treatment average. (1)



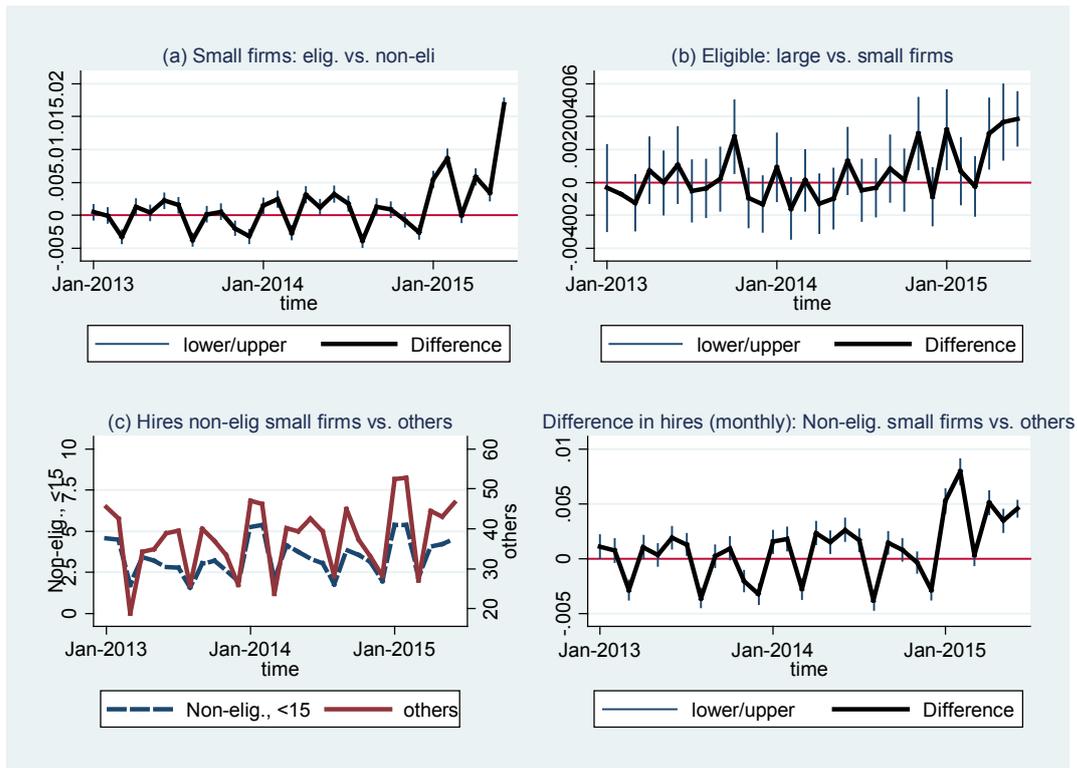
(1) Differences in trends are derived by an OLS estimate of hires on a set of separate monthly dummies for eligible and non-eligible workers. Robust standard errors.

Figure 5: Hires (all types of contracts) by size of the firm: less than 15 (not subject to the Jobs Act) and 15+ (subject to the Jobs Act). Thousands in the upper panel and differences in monthly hires between the two groups in the bottom panel. The small vertical lines represent the confidence intervals. Differences are normalized by using the pre-treatment average. (1)



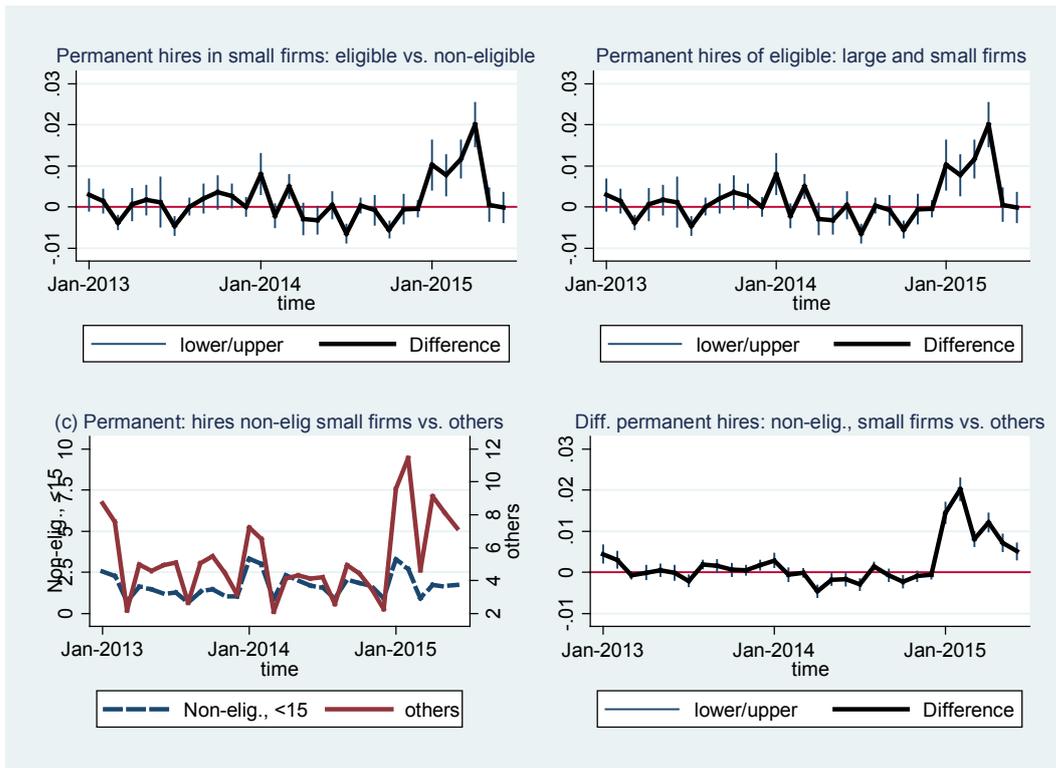
(1) Differences in trends are derived by an OLS estimate of hires on a set of separate monthly dummies for firms with less than 15 employees and 15+ firms. Robust standard errors.

Figure 6: Difference in monthly hires of eligible and non-eligible workers in small firms in panel (a); difference in monthly hires in small and in large firms within the group of eligible in panel (b); in panel (c) we report the number of hires of non-eligible workers in small firms (left-hand panel) and the number of all the other hires (right-hand panel); difference in monthly hires of non-eligible workers in small firms and other hires in panel (d). The small vertical lines represent the confidence intervals. Differences are normalized by using the pre-treatment average. (1)



- (1) Differences in monthly hires are derived by an OLS estimate of hires on a set of separate monthly dummies for the two groups compared in each panel. Robust standard errors.

Figure 7: All panels refer to hires with a permanent job contract. Difference in monthly hires of eligible and non-eligible workers in small firms in panel (a); difference in monthly hires in small and in large firms within the group of eligible in panel (b); in panel (c) we report the number of hires of non-eligible workers in small firms (left-hand panel) and the number of all the other hires (right-hand panel); difference in monthly hires of non-eligible workers in small firms and other hires in panel (d). The small vertical lines represent the confidence intervals. Differences are normalized by using the pre-treatment average. (1)



(1) Differences in monthly hires are derived by an OLS estimate of hires on a set of separate monthly dummies for the two groups compared in each panel. Robust standard errors.

Table 1: Number of hires in Veneto in each semester, by type of firm (less than 15 employees, 15+) and workers past work experience (without a permanent job in the previous semester = eligible for the incentive after January 2015).

	Total	Small firm	Large firms	Non-eligible	Eligible	Non-eligible in small firms
2013.1	235,093	114,430	120,663	41,447	193,646	9,928
2013.2	216,798	100,566	116,232	32,956	183,842	7,774
2014.1	263,033	132,310	130,723	42,101	220,932	13,322
2014.2	228,438	111,195	117,243	31,630	196,808	9,378
2015.1	292,443	150,773	141,670	43,813	248,630	15,007

Table 2: Characteristics of workers hired in Veneto in each semester from January 2013 to June 2015. Total, workers hired in small firms and workers not previously hired with a permanent job contract (non-eligible for the 2015 incentive) hired in the semester by a small firm.

	All hires			Small firms			J-t-j flow , small firm, non-eligible	
	Share eligible	Share men	Average age	Share eligible	Share men	Average age	Share men	Average age
2013.1	96.4	52.4	36	95.6	53.1	37	59.1	36
2013.2	96.3	51.7	35	95.2	53.7	35	53.3	36
2014.1	95.5	54.7	36	94.1	55.1	37	55.5	38
2014.2	95.8	51.6	36	94.0	54.4	36	61.0	36
2015.1	94.8	55.7	37	93.5	56.2	37	60.3	38

Table 3: Probability of conversion from fixed-term to permanent job contract: individual-level estimates of the effect of the policies

	(1)	(2)	(3)
Incentive (PHI)	0.008 [0.086]*	0.007 [0.095]*	0.009 [0.075]*
Jobs Act (CTC)		0.005 [0.000]***	0.013 [0.047]**
Incentive (PHI)*Jobs Act (CTC)			-0.009 [0.209]
Individual fixed-effects	yes	yes	yes
Year and month dummies	yes	yes	yes
Observations	625,306	625,306	625,306
R-squared	0.164	0.164	0.164
Monthly probability of conversion in 2013-14	.009	.009	.009

Linear probability model. Robust p values in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is a dummy equal to 1 if the fixed-term job contract is converted into an open-ended contract.

Table 4: Probability to find a permanent job: individual-level estimates of the effect of the policies

	All	Non-working at time t-1	Fixed-term at time t-1
	(1)	(2)	(3)
Incentives (PHI)	0.008 (0.000)***	0.008 (0.000)***	0.011 (0.001)***
Individual fixed-effects	yes	yes	yes
Year and month dummies	yes	yes	yes
Observations	1,289,075	860,185	428,890
Monthly probability to find an open-ended job position in 2013-14	.008	.008	.005

Linear probability model. Robust p values in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is a dummy equal to 1 if the person finds a permanent job position and 0 otherwise. The model in column (1) includes also a dummy for the condition at time t-1 (whether employed temporarily or not employed). The second column refers to people moving from unemployment into a permanent employment; column (3) refers to transitions from fixed-term to open-ended job contracts in another firm within the same month.

Table 5: Hires (gross) with permanent contracts: firm-level estimates of the effect of the policies

	(1) total hires	(2) total hires	(3) total hires	(4) total hires	(5) total hires	(6) total hires- job-to-job flows
Incentive (PHI), Open-ended	0.040 [0.000]***	0.039 [0.000]***	0.036 [0.000]***	0.008 [0.000]***	0.008 [0.000]***	0.039 [0.000]***
Jobs Act (CTC), Open-ended		0.03 [0.041]**	-0.006 [0.335]		0.008 [0.000]***	0.044 [0.099]*
Incentive (PHI), Jobs Act (CTC), Open-ended			0.074 [0.013]**			-0.013 [0.000]***
Anticipating incentive (PHI), Open- ended	-0.014 [0.000]***	-0.014 [0.000]***	-0.015 [0.000]***	-0.004 [0.000]***	-0.004 [0.000]***	-0.004 [0.583]
Anticipating Jobs Act (CTC), Open-ended		0.019 [0.203]	0.019 [0.198]		0.003 [0.107]	
Known, Inc. (PHI), Open-ended				0.007 [0.000]***	0.008 [0.000]***	
Known, Jobs Act (CTC), Open- ended					-0.008 [0.000]***	
Main effects		yes	yes	yes	yes	
Time dummies		yes	yes	yes	yes	
Observations	1,865,880	1,865,880	1,865,880	6,214,320	6,214,320	1,865,880
R-squared	0.015	0.015	0.015	0.009	0.009	0.015
Share of flow of permanent contracts due to the policies						
Effect of incentives (PHI)	39	38	30	39	38	51
Effect of Jobs Act (CTC)		5			5	12
Effect of both policies			5			
Share of total flow due to the policies						
Effect of incentives (PHI)	8	8	6	8	8	14
Effect of Jobs Act (CTC)		1			1	3
Effect of both policies			1			

Linear probability model. Robust p values in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the number of hires made by each firm. For each firm we identify cells measuring the number of hires of workers with given characteristics made by the firm. Each cell is defined by the intersection of month (from January 2013 to June 2015), type of contract (6 categories), eligibility to PHI (as defined in the text), size of the firm (smaller than 15, 15+), and past relationship with the firm (worker known, unknown) in columns 4 and 5, only. Difference between total hires and job-to-job flows in column 6.

Table 6: Permanent contract net job creation: firm-level estimates of the effect of the policies
(1)

	(1)	(2)	(3)	(4)
	Closed panel (5,000 firms)			Unbalanced panel (50,000 firms)
Incentive (PHI), Open-ended	0.04 [0.000]***	0.038 [0.000]***	0.036 [0.000]***	0.005 [0.000]***
Jobs Act (CTC), Open-ended		0.03 [0.047]**	-0.006 [0.447]	-0.009 [0.123]
Incentive (PHI), Jobs Act (CTC), Open-ended			0.075 [0.016]**	0.014 [0.071]*
Anticipating incentive (PHI), Open-ended	-0.006 [0.028]**	-0.007 [0.023]**	-0.007 [0.019]**	-0.003 [0.000]***
Anticipating Jobs Act (CTC), Open-ended		0.018 [0.282]	0.018 [0.272]	0 [0.991]
Main effects	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Observations	1,865,880	1,865,880	1,865,880	10,400,000
R-squared	0.004	0.004	0.004	0.004
Share of flow of permanent contracts due to the policies				
Effect of incentives (PHI)	51	47	41	47
Effect of Jobs Act (CTC)		5		
Effect of both policies			5	5
Share of total flow due to the policies				
Effect of incentives (PHI)	42	38	35	38
Effect of Jobs Act (CTC)		4		
Effect of both policies			4	4

Linear probability model. Robust p values in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the difference between the number of hires made by each firm and the number of job separation in the same cell. For each firm we identify cells measuring the number of hires and job separations of workers with given characteristics made by the firm. Each cell is defined by the intersection of month (from January 2013 to June 2015), type of contract (6 categories), eligibility to PHI (as defined in the text), and size of the firm (smaller than 15, 15+). Unbalanced panel of 50,000 firms in column 4.

Appendix

Table A1: Aggregate estimates of the effect of the policies on conversions from fixed-term to permanent job contracts

	Non eligible	All
Incentive (PHI), Open-ended	76 (0.065)*	136 (0.127)
Jobs Act (CTC), Open-ended		749 (0.000)***
Incentive (PHI), Jobs Act (CTC), Open-ended		993 (0.000)***
Observations	60	120
R-squared	0.853	0.882
Increase in total flow (post/pre)	103%	63%
Estimated effect (contribution in percentage points):		
Jobs Act	34pp	
Incentives		35pp
Incentives*Jobs Act		26pp
R-squared	0.853	0.882

OLS estimates. Robust p values in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the number of fixed term contract converted into open ended contracts in each cell level. Each cell is defined by the intersection of month (from January 2013 to June 2015), firm size (below/above 15 employees) and worker's eligibility to PHI. In the first column only the cells pertaining to non- eligible workers are considered.

Table A2: Hires (gross) with a permanent contract: firm-level estimates of the effect of the policies. Other coefficients not reported in Table 5, columns 1-3

	(1)	(2)	(3)
Eligible, Apprentices	0.003 [0.031]**	0.005 [0.003]***	0.004 [0.007]***
Jobs Act, Apprentices		-0.019 [0.000]***	-0.016 [0.000]***
Eligible, Jobs Act, Apprentices			-0.007 [0.396]
Anticipating incentive, Apprentices	-0.007 [0.000]***	-0.007 [0.000]***	-0.006 [0.000]***
Anticipating Jobs Act, Apprentices		-0.01 [0.141]	-0.01 [0.134]
Eligible, St. Fixed Term	0.044 [0.000]***	0.042 [0.000]***	0.041 [0.000]***
Jobs Act, St. Fixed Term		0.027 [0.022]**	0.021 [0.164]
Eligible, Jobs Act, St. Fixed Term			0.013 [0.583]
Anticipating incentive, St. Fixed Term	0.001 [0.802]	0 [0.993]	0 [0.982]
Anticipating Jobs Act, St. Fixed Term		0.044 [0.133]	0.044 [0.133]
Eligible, Agency	-0.006 [0.000]***	-0.006 [0.000]***	-0.008 [0.000]***
Jobs Act, Agency		0.009 [0.180]	-0.013 [0.000]***
Eligible, Jobs Act, Agency			0.045 [0.001]***
Anticipating incentive, Agency	-0.01 [0.000]***	-0.01 [0.000]***	-0.01 [0.000]***
Anticipating Jobs Act, Agency		-0.001 [0.820]	-0.001 [0.830]
Eligible, Collaborator	-0.007 [0.000]***	-0.006 [0.000]***	-0.006 [0.000]***
Jobs Act, Collaborator		-0.017 [0.000]***	-0.016 [0.000]***
Eligible, Jobs Act, Collaborator			-0.003 [0.745]
Anticipating incentive, Collaborator	-0.01 [0.000]***	-0.01 [0.000]***	-0.01 [0.000]***
Anticipating Jobs Act, Collaborator		-0.012 [0.014]**	-0.012 [0.013]**
Eligible, Intern	0.012 [0.000]***	0.012 [0.000]***	0.01 [0.000]***
Jobs Act, Intern		0.005 [0.580]	-0.019 [0.000]***
Eligible, Jobs Act, Intern			0.049 [0.009]***
Anticipating incentive, Intern	0.005 [0.011]**	0.005 [0.010]**	0.005 [0.011]**
Anticipating Jobs Act, Intern		-0.001 [0.941]	-0.001 [0.950]
Constant	0.008 [0.000]***	0.008 [0.000]***	0.009 [0.000]***
Observations	1,865,880	1,865,880	1,865,880
R-squared	0.015	0.015	0.015

Table A3: Hires (gross) with a permanent contract: firm-level estimates of the effect of the policies: Other coefficients not reported in Table 3, columns 4-5

	(1)	(2)
Eligible, Apprentices	-0.002 [0.000]***	-0.001 [0.008]***
Eligible, St. Fixed Term	0.011 [0.000]***	0.011 [0.000]***
Eligible, Agency	-0.001 [0.004]***	-0.002 [0.000]***
Eligible, Collaborator	-0.005 [0.000]***	-0.005 [0.000]***
Eligible, Intern	0.012 [0.000]***	0.011 [0.000]***
Known, eligible, Open-ended	0.007 [0.000]***	0.008 [0.000]***
Known, eligible, Apprentices	0.005 [0.000]***	0.005 [0.000]***
Known, eligible, St. Fixed Term	0.004 [0.001]***	0.005 [0.000]***
Known, eligible, Agency	-0.001 [0.008]***	-0.001 [0.132]
Known, eligible, Collaborator	0.006 [0.000]***	0.006 [0.000]***
Known, eligible, Intern	-0.016 [0.000]***	-0.016 [0.000]***
Anticipating incentive, Open-ended	-0.004 [0.000]***	-0.004 [0.000]***
Anticipating incentive, Apprentices	-0.002 [0.000]***	-0.002 [0.000]***
Anticipating incentive, St. Fixed Term	0 [0.698]	0 [0.876]
Anticipating incentive, Agency	-0.003 [0.000]***	-0.003 [0.000]***
Anticipating incentive, Collaborator	-0.003 [0.000]***	-0.003 [0.000]***
Anticipating incentive, Intern	0.002 [0.000]***	0.002 [0.000]***
Jobs Act, Apprentices		-0.004 [0.000]***
Jobs Act, St. Fixed Term		0.009 [0.000]***
Jobs Act, Agency		0.007 [0.000]***
Jobs Act, Collaborator		-0.002 [0.007]***
Jobs Act, Intern		0.002 [0.210]
Known, Jobs Act, Open-ended		-0.008 [0.000]***
Known, Jobs Act, Apprentices		0.002 [0.074]*
Known, Jobs Act, St. Fixed Term		-0.018 [0.000]***
Known, Jobs Act, Agency		-0.008 [0.000]***
Known, Jobs Act, Collaborator		-0.001 [0.278]
Known, Jobs Act, Intern		-0.002 [0.261]
Anticipating Jobs Act, Open-ended		0.003 [0.017]**
Anticipating Jobs Act, Apprentices		-0.001 [0.172]
Anticipating Jobs Act, St. Fixed Term		0.005 [0.015]**
Anticipating Jobs Act, Agency		0.001 [0.053]*
Anticipating Jobs Act, Collaborator		-0.001 [0.151]
Anticipating Jobs Act, Intern		0 [0.756]
Constant	0.004 [0.000]***	0.004 [0.000]***
R-squared	0.009	0.009

Table A4: Permanent contract net job creation: firm-level estimates of the effect of the policies:
Other coefficients not reported in Table 6

	(1)	(2)	(3)	(4)
Eligible, Apprentices	0.005 [0.004]***	0.005 [0.002]***	0.006 [0.001]***	-0.002 [0.000]***
Jobs Act, Apprentices		-0.016 [0.000]***	-0.011 [0.002]***	-0.004
Eligible, Jobs Act, Apprentices			-0.01 [0.243]	0.003 [0.270]
Anticipating incentive, Apprentices	0 [0.971]	0 [0.996]	0 [0.988]	0.002 [0.001]***
Anticipating Jobs Act, Apprentices		-0.002 [0.818]	-0.002 [0.812]	0.01 [0.456]
Eligible, St. Fixed Term	0.027 [0.000]***	0.027 [0.000]***	0.027 [0.000]***	-0.001 [0.184]
Jobs Act, St. Fixed Term		0.006 [0.650]	0.021 [0.151]	-0.016 [0.492]
Eligible, Jobs Act, St. Fixed Term			-0.031 [0.231]	0.011 [0.661]
Anticipating incentive, St. Fixed Term	-0.059 [0.000]***	-0.059 [0.000]***	-0.059 [0.000]***	-0.002 [0.097]*
Anticipating Jobs Act, St. Fixed Term		-0.015 [0.655]	-0.015 [0.652]	0.007 [0.598]
Eligible, Agency	-0.007 [0.089]*	-0.005 [0.075]*	-0.004 [0.008]***	-0.001 [0.008]***
Jobs Act, Agency		-0.042 [0.230]	-0.024 [0.100]	-0.005
Eligible, Jobs Act, Agency			-0.038 [0.596]	0.009
Anticipating incentive, Agency	-0.005 [0.010]***	-0.005 [0.008]***	-0.005 [0.009]***	0.001 [0.001]***
Anticipating Jobs Act, Agency		-0.012 [0.269]	-0.012 [0.260]	-0.002 [0.359]
Eligible, Collaborator	-0.004 [0.014]**	-0.004 [0.036]**	-0.003 [0.040]**	-0.001 [0.000]***
Jobs Act, Collaborator		-0.014 [0.008]***	-0.012 [0.000]***	-0.003
Eligible, Jobs Act, Collaborator			-0.005 [0.641]	0 [0.992]
Anticipating incentive, Collaborator	-0.014 [0.000]***	-0.013 [0.000]***	-0.013 [0.000]***	0 [0.554]
Anticipating Jobs Act, Collaborator		-0.028 [0.095]*	-0.028 [0.094]*	-0.007 [0.122]
Eligible, Intern	0 [0.876]	-0.001 [0.796]	-0.001 [0.570]	0.002 [0.000]***
Jobs Act, Intern		0 [0.966]	-0.011 [0.000]***	-0.004
Eligible, Jobs Act, Intern			0.021 [0.339]	0.004
Anticipating incentive, Intern	0.001 [0.786]	0.001 [0.726]	0.001 [0.741]	0 [0.741]
Anticipating Jobs Act, Intern		-0.011 [0.382]	-0.011 [0.386]	-0.002 [0.412]
Constant	0.01 [0.000]***	0.01 [0.000]***	0.01 [0.000]***	0.006 [0.000]***
Observations	1,865,880	1,865,880	1,865,880	10,400,00
R-squared	0.004	0.004	0.004	0.004