Hiring Subsidies and/or Firing Cost Reduction? Evaulating the Impact of the 2015 Policies on the Italian Labour Market

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

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Abstract

In 2015 the Italian government adopted two different policies aimed at reducing labour market dualism and fostering employment: a generous subsidy for firms hiring workers with an open-ended contract and new regulations lowering firing costs for new hires. Using both aggregate data for Italy and microdata for Veneto we exploit some differences in the design of the two policies and evaluate their impact on the labour market. We find that around 25 per cent of gross permanent hires occurred because of the subsidies, whereas 6 per cent can be attributed to the new firing regulations. The policies contributed also to double the monthly rate of conversion of fixed-term jobs into permanent positions. In many cases hiring subsidies were used by firms to hire workers with previous fixed-term work experiences within the firm. The new firing rules made firms slightly less reluctant to offer permanent job positions to yet untested workers.

JEL: J6, J21
Keywords: job creation, firing costs, hiring subsidies, labour market reforms.

1. Introduction

Italy lost 1 million jobs over the 2008-14 period as a result of the double dip recession stemming from the Global financial crisis. Because of the dualistic structure of the Italian labour market, job losses were concentrated among younger workers and, more generally, among people holding fixed-term job contracts. In 2015 the number of open-ended contracts has started to rise again (by around 1 per cent on a yearly basis), despite the still subdued GDP growth (a bit less than 1 per cent). In this paper we test whether and to what extent this evolution can be attributed to two policies introduced in Italy at the beginning of 2015 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 applied to all new permanent job contracts (hereafter hiring subsidy, HS) offered to workers who did not hold an open-ended position in the previous semester. This subsidy was not targeted to specific groups of workers and it was not contingent upon firm-level net job creation. Rather, it applied also to conversions within the same firm from a fixed-term to an open-ended position.

The second policy is a reshaping of the regulation about dismissals, aimed at reducing the level and the uncertainty of firing costs for all new permanent contracts in firms with at least 15 employees. This policy was part of a wider reform package known as the "Jobs Act" and in what follow we label it as FC (for firing costs).

We separately identify the effects of the two policies by jointly exploiting their different commencement dates (one came into effect in January 2015 and the other in March 2015) and, more importantly, the differences in the applicability of the two schemes, as the HS applies to all firms but only to workers without a permanent job contract in the previous semester, while the new FC regulation reshapes firing costs for all new permanent contracts, but only for firms beyond the 15-employee threshold. We provide some evidence of the relevance of the two policies for Italy, based on aggregate data, drawn from the Italian National Security Institute (INPS). Our main estimates are however based on administrative microdata for the Veneto region, which allow us not only to measure labour market flows (in the first half of 2015 and the 2 years before), but also to reconstruct the previous status of workers in the labour market, to match firms to workers, to identify firms' size and, ultimately, to control for unobserved heterogeneity.

It has to be stressed that there are relevant aspects of the two policies that are not considered here. We do not discuss all the pros and cons 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 clearly show that both policies fostered net job creation at the firm level, shifted the employment composition towards permanent job contracts and increased the probability of workers securing a permanent position.

According to our findings around 25 per cent of gross permanent hires occurred because of the subsidies, whereas 6 per cent can be attributed to the new firing regulations.. Additionally, the doubling in the monthly conversion rate from temporary to permanent contracts observed in Veneto in the first six months of 2015 (from about 1 to more than 2 per cent) is entirely due to the two policies, the HS policy again contributing the most.

We also shed some light on the impact of the two policies on firms' willingness to hire yet untested workers on a permanent basis. First, the reduction of firing costs introduced by the Jobs Act slightly enhanced the willingness of firms with more than 15 employees to hire workers who never worked for the firm before. Second, the fact that the HS also applied to permanent contract conversions paradoxically led to an increase in temporary hiring too, as many firms still exploited the possibility of testing workers through a temporary position, eventually converting it later on into a permanent position (and cashing the full amount of the subsidy).

The relatively small effect of the FC on the level of employment is in line with the literature about the effects on labour demand of a 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. For a given wage level, 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 occur several years after the time of hiring and need 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 would materialize in an economic upswing (downswing) and in 2015 Italy was far from a cyclical upswing. As such, our paper adds to (and mostly confirms) the empirical literature focusing on the labour demand effects of firing costs. Autor, Kerr and Kugler (2007), using state-level data and time variations 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 rely on supply-side shocks (like rainfalls) and different state-level employment protection legislation to show that employment protection reduces employment responses to shocks. In our paper we

mainly focus on a single Italian region, Veneto. As a consequence, our results are not affected by spurious local trends as in the above mentioned empirical papers.

Our results are also a contribution to the literature about the effects of a reduction in both the uncertainty and the expected level of firing costs. Actually, the FC reform mostly tempered the high uncertainty stemming from the possibility that in the previous regime judges could decide in favour of the dismissed worker, mandating not only monetary reimbursement, but also the worker's reinstatement in the firm. The employment effects of FC we uncover in the data are likely to be driven by their uncertainty component.

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 suitability 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 subsidies, to the best of our knowledge, this is the first time that the Italian government introduces non-targeted, non-conditional subsidies. Cipollone and Guelfi (2003) analyze selective hiring subsidies introduced in 2001 targeted to young workers hired on a permanent basis and do not find notable effects on labour demand. Ciani and De Blasio (2014) consider a very short-term policy aimed at stimulating conversions of fixed-term into permanent contracts. This policy was introduced in Italy in 2013 and lasted just few weeks because of severe funding constraints. This subsidy was targeted at females and young people and, according to Ciani and De Blasio (2014) had a positive effect on conversion rates. Also in 2014 the government introduced subsidies to firms hiring workers on a permanent basis, but the subsidies were conditional on firms' net job creation. Given the very weak macroeconomic conditions, only very few firms took advantage of the subsidies. Our results, instead, support the hypothesis that non-targeted, non-conditional subsidies are immediately effective in gross and net job creation, and for this aspect are similar to the ones by Cahuc et al. (2014) who find a positive and rapid expansionary effect of the non-conditional hiring credits introduced in France during the Global Financial Crisis. Our results are also in line with Neumark (2013) and Neumark and Grijalva (2013), who argue that non-targeted hiring subsidies have a positive effect on employment during recessions, even if the deadweight losses associated with 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. In section 3 we describe our datasets. In section 4 we examine the aggregate data for the whole country, while section 5 deals with our estimation strategies and present the results of the empirical analysis, based on microdata for the Veneto region. Section 6 concludes.

2. **The Italian labour market and the policies under scrutiny**

The Italian labour market was heavily segmented in permanent and fixed-term workers. The dualism arose since 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.[1]

During the 2000s the share of fixed-term workers increased rapidly to around 13 per cent. More than 60 per cent of new hires were fixed-term job contracts. These contracts were 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 law no. 92/2012, the so-called 'Fornero Reform', followed in 2015 by the Jobs Act.

Before 2015, Italy was characterised by firing costs whose main feature was their uncertain and (potentially) high amount.[2] Dismissals were costless for firms if they were motivated by a just cause, i.e. worker misbehaviour or firms' need to reduce or reorganize its workforce. However, whenever a worker objected to the dismissal and the courts deemed the dismissal to be unfair, the costs could be rather high, particularly in the case of firms with at least 15 employees. Firms with less than 15 employees could (according to law no. 108/1990) choose between reinstating the worker or paying a pre-set severance payment (tied to the worker's seniority and varying from 2.5 to 6 times their monthly pay). For firms with at least 15 employees, the general rule was that the worker had to be reinstated. Firms could side-step reinstatement by reaching a private arrangement with the worker, again at a potentially high cost.

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[1] See Sestito (2002) and Pirrone and Sestito (2006).
[2] We here refer to individual dismissals.

The most critical aspect of this regime was the uncertainty in both the timing and contents of the judges' decisions, which have varied greatly.[3] The widespread delays in the Italian civil justice system are also likely to have compounded these inefficiencies as workers were lacking any income support (and pay) pending the litigation, whereas firms could end up having to pay high wage arrears if finally the court found in favour of the worker.

Most of the analysis made in Italy focused on the local effect of the regime's discontinuity around the 15-employee threshold. There is evidence that the potentially higher costs affecting firms with 15 or more employees may have somehow limited the growth of smaller firms as they approached the 15+ threshold (see Schivardi and Torrini, 2008). The effects, while statistically significant, were however not very economically significant, as the discontinuity in the size distribution of firms around the threshold is not as marked as the sharp discontinuity taking place for instance in France around the 50-employee 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 errors in measuring firm size, as there were ways to circumvent the threshold (for instance by employing apprentices whose presence in the firm's workforce, by law, does not concur to determine the 15-employee threshold).

The reduction of (potentially high) firing costs has been a 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 stopped however by strong opposition from the biggest union, the CGIL.

Both the 'Fornero Reform' (law no. 92/2012) and more recently the Jobs Act (decree no. 183/ 2014)[4] were aimed at reducing the uncertainty about around firing costs. The 'Fornero Reform', enacted by the technocratic Monti government, attempted to fix the existing procedures: litigants were channeled into a conciliatory procedure so as to prevent lengthy judicial litigations; it limited the possibility of reinstatement in the case of unfair dismissals for disciplinary or economic reasons; and provided an upper limit to the monetary firing costs (24-month pay) whenever these had to be applied.

The 'Fornero Reform', however, left judges with a lot of leeway in determining both the fairness of the dismissal and its consequences. For this reason the Renzi government made a

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[3] See Ichino (1996).

[4] The Jobs Act is a wider reform dealing with the strengthening of unemployment benefits and the launch of a national Agency in charge of active labour market policies. In this paper we refer only to the new rules on firing costs, issued in the final days of 2014 and in force since March 2015.

second attempt at reducing firing costs with the 'Jobs Act', significantly reinforcing some existing elements. The Jobs Act further limited the possibility of reinstatement, allowing it for discriminatory dismissals and for a few specific cases of disciplinary dismissals. As a general rule, it established that unfair dismissals must be compensated by an amount of money strictly predetermined by law and proportional to job tenure (from a minimum of 4 times the monthly pay to a maximum of 24 times, i.e. 2-month pay for every year of seniority). This monetary compensation may be reduced if the worker agrees to end any pending litigation about the nature of the dismissal, and the worker is exempt from paying taxes on the compensation received.

Unlike the 'Fornero Reform', which covered all permanent employees, the new rules only apply to permanent contracts signed after March, 7th, 2015, when the new law came into force, by firms with more than 15 employees (or by those firms which will reach the 15+ threshold with their new hires). Existing employees, as well as the newly signed permanent contracts in firms staying below the 15-employee threshold, remain covered by the previous legal framework.[5]

All in all, the new regime reduces both the expected firing costs and, most significantly, the uncertainty surrounding it for firms over the 15-employee threshold, with no change for those below the threshold.[6] It is not easy, however, to quantify the reduction in uncertainty in monetary terms; even using risk-neutral firms as a benchmark, it may depend on the judgments issued by local courts and the length of trials. The quantification of the reduction in firing costs may be further complicated by the presence of the above mentioned regime offering tax-free treatment of severance pay.[7]

The government also introduced a very generous non-conditional hiring subsidy. The subsidy, established by the Financial Stability Law for 2015 (but already announced at the end of October 2014) covered 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 subsidy was a three-year exemption from social security contributions up to a

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

[6] For those approaching the 15-employee threshold there is a reduction in the costs of overcoming the threshold as they are exempted from the consequences of overpassing it. For this reason, differently from Schivardi and Torrini (2008), in some estimates we do not look to firms close to the threshold (see also Section 5.2).

[7] This may have two different possible implications: on the one hand it directly reduces the cost for a firm of a dismissal deemed unfair by a judge; on the other hand it may stimulate the worker to challenge a dismissal that is fair under the now prevailing criteria, because the challenge might lead to an agreement somehow more acceptable for the firm given its reduced costs.

threshold, which was quite high compared with the average contributions typically paid by firms for workers (according to the government's estimates the subsidy should fully cover the social security contributions of almost 80 percent of new hires). Conversions from fixed-term to permanent job contracts within a given firm were also subsidized (conversions from apprenticeships were instead excluded as they benefit from an ordinary subsidy which was not modified by the HS policy).

Last, in October 2015 the government announced a reduction in hiring subsidies for permanent workers hired from January to December 2016, and equal to a 2-year exemption from no more than 40 per cent of social security contributions.

The two policy measures undertaken in 2015 and analysed in this paper almost overlap, because both target permanent hires and job contract conversions from fixed-term into open-ended contracts. There is, however, a small difference in their timing: from January 2015 for the subsidy, from 7 March 2015 for the 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. Subsidies were paid to firms of any size, while the new FC regulation applies to firms with at least 15 employees; the subsidy applied only to workers without permanent contracts in the previous 6 months, while the previous status of the worker is irrelevant for the application of the new FC. Thus, information about firm size and workers' past work histories, together with the precise date of the new contract, and data covering both the period before and the one after the implementation of the policies allow for the separate identification of their effects.

Many important issues, however, must be taken into account. First, firms' hiring strategies were probably influenced by the announcement of HS in October 2014 (3 months before its implementation) and the announcement of its reduction in October 2015 (for hires taking place in 2016). Similarly, also the reform of FC was announced at the end of December 2014, with the policy being actually implemented only in March 2015. Second, it is important to notice that changes in the evolution over time of permanent vs temporary hires (across firms' size classes and workers types) may not help to identify the effects of the two policies as these may have affected also temporary job contracts. Firms often hire fixed-term workers, test their skills and then convert the contract into an open-ended one. Such a two-step strategy is convenient for firms because it allows them to verify the goodness of the job match; such a convenience is increasing in firing costs and in the uncertainty about the job candidate's quality, the only restraining factor coming from the risk that the worker, when hired on a temporary basis, is more likely to search for an alternative job offer. The FC reform might have changed

the propensity to adopt such a two-step strategy, while the HS might have paradoxically increased its use as firms might be willing to hire-and-test a larger pool of unscreened workers in order to eventually convert their job contract into a permanent (and subsidized) one. [8] The opposite is likely to apply to workers who have been already screened in the past, for having been previously employed within the firm.

As better explained in sections 4 and 5 our estimates take into account the possible reactions of firms to the announcement of the policies. Moreover, our identification strategy is not based on a simple comparison between temporary and permanent hires. Further, we distinguish 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 the firm for the first time (workers "unknown to the firm"). Both HS and FC might have affected the preference of firms concerning known and unknown workers. The FC policy could have reduced the reluctance of 15+ firms to offer open-ended positions to unknown workers. The effects of the HS policy are instead more complex: the subsidy provides for an incentive both to hire "known" workers on a permanent basis – by converting a larger share of the existing fixed term contracts and by recalling workers they tested in the past – and to hire unknown people on a temporary basis, in order to test them and gain from the eventual conversion of their contract (cashing the full amount of the HS).

3. General features of the data

In this paper we use two sources of administrative data. The first, available only for aggregate analysis, is drawn from the Social Security database, managed by the Italian National Social Security Institute (INPS). The second is composed of microdata about the so-called Comunicazioni Obbligatorie (CO), collected in the Veneto region.

In Italy all private and some public sector firms have to electronically notify to the local public labour market agency all events concerning hiring, firing, conversion from fixed-term to open-ended contracts and fixed-term contract time extension. The same data, only for the private sector, are also made accessible to the Italian Social Security Institute, INPS, which manages social security payments and receives by firms monthly information about their workforce. The INPS dataset includes then the date of starting and end of each job contract, the

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[8] Insofar as the subsidy also covers conversions from temporary to permanent contracts, which is the case here under examination.

contract type and, basically, all the information needed for determining social security payments.

The CO database is managed by the Italian regions, under the coordination of the Ministry of Labour. Because of the decentralization of the data-collection process, the quality of the microdata (especially information concerning contract conversions) differs across Italian regions. Data issued by the Veneto region are characterized by high quality. Veneto is located in the north-eastern part of the country. The weight of the manufacturing industry in the region's economy is among the highest in the country. It represents slightly less than 10 per cent of the overall Italian labour market (in terms of employees).

From both datasets we exclude firms in the agricultural sector and domestic workers hired by households (excluded from the HS) and focus on industry, construction and private services.[9]

The INPS data cover the period between January 2006 and December 2015. For each month we know how many workers have been hired or fired (as well as contract conversions), whether the workers had/had not a permanent job position in the previous 6 months (which determines the eligibility condition relevant for the HS since January 2015) and firm size (15+ or less). For simplicity, we label all workers with no permanent job contract in the previous 6 months as "eligible", even if observations refer to the years before 2015, when the HS did not apply. We measure firm size at the end of the previous year, to avoid the potential endogeneity of size due to firms' response to the FC reduction. Automatically, from the original sample, we exclude newly born firms (i.e. those which are in the sample at time t, but have no record before t). This selection strategy is particularly important for firms entering in the market (and in the dataset) in 2015, as it ensures that the composition of our sample does not depend on the policy itself.

We use INPS data to analyse overall trends in monthly hiring and firing. For each month between January 2006 and December 2015 we have aggregate flows for 4 groups, identified by the intersection of the following characteristics: (1) whether the workers is eligible for the HS or not; (2) whether the firm was 15+ or smaller.

Second, we identify the effect of the two policies by the use of microdata about job contracts signed in Veneto. Albeit limited to a single region, the use of the microdata will allow us, in the next sections, to take account of compositional factors and to identify more precisely the effect of the two different policies.

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[9] The CO exclude most of the public sector but a few contractual categories referring to temporary hires.

For each event recorded in our dataset we can identify both the firm and the worker involved. On top of the relevant anonymized identifiers (firm and worker), we know the firm's size[10] (by size class), sector of activity and the worker's gender, birth-date, and nationality. For each event we know the type of the underlying job contract, i.e. whether permanent or fixed-term. The latter group includes: (i) standard fixed-term dependent contracts, (ii) agency workers, (iii) apprentices,[11] (iv) consultants (so-called *parasubordinati*, i.e. a group of workers with no firing protection and whose contractual arrangements blend elements of both dependent and independent employment, not included in the INPS dataset) and (v) intern (so called *tirocinanti*).[12] Since we have microdata on both firms and workers, we construct two datasets, one for firms and one for workers.

Because of limits in data availability the dataset for Veneto spans from January 2013 to June 2015. Even if this time span is shorter than the one of INPS data, it allows us to avoid two possible confounding events. The first is the introduction, since July 2015, of new limits for the use of consultants, which might have indirect effects on the use of other types of job contracts. The second is the government's announcement of its intention to extend HS to 2016 -albeit for a reduced amount and duration (up to 40% of total social security for 2 years), affecting flows in the last part of the year.

The dataset on firms is subject to the same selection rules used for INPS data. The final dataset is composed of monthly data for around 75,000 firms which roughly corresponds to the universe of firms[13] whose main branch is located in Veneto.[14]

Concerning individuals we exclude individuals entering into the labour force in 2015 as their entrance is affected by the policies (as made for firms). We then focus only on individuals

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[10] The size of the firm is not directly communicated at the time of hiring/firing/conversion, but can be ascertained via other compulsory reporting requirements such as those under Law 68/1999 on legally protected workers.

[11] Legally, apprenticeships are classified as open-ended contracts. As there is no firing cost in case the contract is terminated before or at the end of the apprenticeship period we classify them as fixed-term.

[12] We also exclude job transitions in the tourism sector, as the very high seasonality of employment in this sector cannot be fully captured by the 3-year sample used in this paper.

[13] In Italy firms undertaking collective layoffs (which occur in case of at least 5 layoffs within 12 months) are forbidden to hire new workers within a certain time period from the layoff. In this paper we do not have information on the causes of job separation. As firms involved in collective layoff cannot hire, their inclusion in our sample would bias our estimates downward. Our dataset lacks information to precisely identify collective layoffs. To be conservative we exclude from our sample all firms having in a given month at least 5 separations involving open-ended contracts.

14 To draw this conclusion we have analysed the INPS database which records all firms with employees by region, unfortunately available for 2012 and 2013 only. After the same sample selection used for the *Comunicazioni Obbligatorie*, we end up with 75,634 firms in two years, a discrepancy of just 0,5% with respect to our sample probably due to the use, in defining the sectoral boundaries, of different sectoral classification rules.

with at least one fixed-term job contract from January 2013 to October 2014, the time of the announcement of the HS policy. We also focus on workers aged between 18 and 55 at the time of their first fixed term job contract, to exclude those who retire. After this sample selection our sample includes roughly 330,000 individuals per year. Notice that this group compares, in Veneto, to an average stock of 190,000 unemployed and 200,000 fixed term workers, at risk to become unemployed at the expiration of the contract. So our sample is quite representative of active job seekers, including both those currently unemployed and fixed-term workers "at risk" to become unemployed.

In this paper we mainly look at two outcomes: (i) gross hiring (and the related individual probability to find a job) and (ii) net job creation. Net job creation is defined as the difference between hires and job separations, the latter including not only fires, but also voluntary job separations and retirement. In the case of job separations, used to calculate net flows, at any time the eligibility status is set equal to the one observed when the worker was hired.

Table 1 refers to Italy and reports the composition of flows by eligibility status of workers and firm size. On average each month 13 per cent of gross hiring involves non-eligible workers in firms with less than 15 employees. The larger flow is instead generated by small firms, hiring eligible workers (48 per cent of gross hiring). On average firms 15+ generate 40 per cent of the total flows.

Table 2 refers to Veneto and mimics Table 1 and confirms that the composition of flows in Veneto is rather similar to the one of Table 1, the only difference being the relatively large share of flows in firms 15+ (depending on the higher incidence of manufacturing firms in Veneto, whose size is on average larger than firms in services).

Table 3 reports the composition of the samples of firms and workers used in this paper. The share of firms 15+ amounts to 10 per cent. Eligible workers amounts to 72 per cent (the HS policy was designed indeed to have a high coverage). Given our selection rules, those who are unemployed amount to 50 per cent of the sample. Looking at those who find a permanent position during the period January 2013-June 2015, 40 per cent of them recorded a past work experience within the same firm. We label these workers as already "known" by the firm which is hiring them.

Figures 1-3 allow us to appreciate the evolution of hires over time. Figure 1 reports net job creation in Italy from January 2006 to December 2015 and the composition of newly created jobs during this period, measured by the share of new hires with a permanent job contract in total new (gross) hires. Two main facts emerge clearly. First, the share of open-ended contracts

in total contracts declines from 2009 to 2014, and starts increasing again at the beginning of 2015. It peaks at the end of 2015, as firms have probably anticipated hiring to benefit for the generous HS for permanent workers. Second, in 2015 the total number of people employed with any type of contract expands considerably.

The peak observed at the end of 2015, the last days to benefit of the 100% HS before its reduction in January 2016, suggests that in our exercises we must take into account the strategic behaviour of firms, as they probably changed their hiring schedule to benefit from higher subsidies. This applies not only to the HS increase at the end of 2015, which is extremely evident, but also to the possible reduction of hiring in the months between the first announcement of the policies at the end of 2014 and their implementation at the beginning of 2015 (determining at that time an increase in the hiring rate). Later on, we will show, on a case-by-case basis, how we take account of these timing issues.

Figures 2-3 refer to Veneto. Figure 2-panel (a) plots aggregate net job creation, from January 2013 to June 2015, for both open-ended and fixed-term job contracts. Consistently with Figure 1, Figure 2 shows that also in Veneto after January 2015 both the number of new fixed-term and open-ended contracts increased considerably, and the latter remained at a level remarkably higher than the one registered in the corresponding periods of 2013 and 2014. Panel (b) reports net job creation, by size class (less than 15 employees and 15+), and shows that the dynamics of net job creation is quite similar across firm-size classes. In the bottom panel we look at flows according to the eligibility status of workers at the time of hiring. We label individuals as "eligible" if they had no permanent job position in the 6 months before the time we observe them. The net flows of eligible and non-eligible workers were similar before the inception of the 2015 HS; afterwards the former increased significantly more than the latter.

We have already mentioned that firms often hire fixed-term workers, test their skills and then convert the contract into an open-ended one. Such a two-step strategy is convenient for firms because it allows them to verify the goodness of the job match; such a convenience is increasing in firing costs and in the uncertainty about the job candidate's quality, the only restraining factor coming from the risk that the worker, when hired on a temporary basis, is more likely to search for an alternative job offer. Similarly, firms could offer directly an open-ended job contract to workers who have been already screened in the past, for having been previously employed within the firm. We then distinguish 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 the firm for the first time (workers "unknown to the firm").

Both HS and FC might have affected the preference of firms concerning known and unknown workers. The FC policy could have reduced the reluctance of 15+ firms to offer open-ended positions to unknown workers. The effects of the HS policy are instead more complex: the subsidy provides for an incentive both to hire "known" workers on a permanent basis – by converting a larger share of the existing fixed term contracts and by recalling workers they tested in the past – and to hire unknown people on a temporary basis, in order to test them and gain from the eventual conversion of their contract (cashing the full amount of the HS).

As a first look at these issues, the upper part of Figure 3 plots the flow of new open-ended job matches involving workers already known to the firm as a ratio to total open-ended contracts. The bottom part reports the number of conversions and the conversion rate, defined as the ratio between the number of conversions and the number of fixed term contracts. Consistently with the hypothesis that firms used the two-step strategy described above, after January 2015 both contract conversions and hires of known workers increased considerably.

All in all, the charts presented in this section show that there are discontinuities in the hiring trends along some of the dimensions affected by the two policies here considered.

4. Evidence from aggregate data: Italy 2006-2015.

As already mentioned, the possibility to identify the effect of the two policies stems mainly on the differences in their design (HS apply to workers not employed with a permanent job contract in the previous 6 months; FC apply to 15+ firms) and the timing of their implementation. However, we cannot simply compare the trends of hires by type of contract because of the presence of the two-step strategy "hiring as temporary and then convert" described in Section 2 (which implies that the HS might have boosted temporary hiring as well). In order to identify a flow not affected by the two policies, to be used as a sort of "control" group, in this section we focus on events in small firms (as such unaffected by changes in FC) involving workers who were not eligible for the HS, i.e. workers with a permanent position in the previous semester.

As a first step we do not attempt to identify separately the effect of the two policies. We deal with this issue in the next sections by the use of microdata. Here we consider only two types of flows: (1) the one composed of non-eligible workers in small firms (non-affected by the policies, labelled as NA); and (2) all the other flows (affected (A), i.e. composed of eligible

workers and/or flows generated by 15+ firms). We label the two groups as $g = NA, A$ respectively and we look at differences in trends before and after the implementation of the two policies. Within this simple diff-in-diff framework, the identification strategy is based on the assumption of the lack of differences in trends before the inception of the policies. We test this hypothesis using INPS data which are particularly suitable as the time series is longer than CO (monthly data from 2006 to 2015).

The first column of Table 4 reports the coefficients corresponding to a dummy equal to 0 when the net flow corresponds to the control group and 1 otherwise. This dummy is further interacted with year dummies from 2011 onwards, corresponding to the bust of the sovereign debt crisis. The results are clear-cut: no significant difference in trends can be found before 2015, being the difference very small compared to the average size of the monthly flow (last row of the Table). Instead, in 2015 the flow affected by the policies increases considerably.

The results of the second column refer only to May and June of each year to avoid that the results are driven by (i) the large spike observed in December 2015 and/or (ii) the implementation of other parts of the Jobs Act reform (since July 2015) (iii) the possible anticipation effects of the two policies in October 2014 and December 2014 (and hiring postponement at the date of the implementation of the two policies in January and March 2015).[15] The results are fully consistent with the ones of the first column. Before 2015 the difference was small and not significantly different from zero, while the opposite holds true in 2015. The combination of the two policies increased net job creation by 50 per cent. INPS data are also used to check whether flows are autocorrelated (with negative effects on the estimated standard errors). The results of this test are described in the Appendix (Table A1).

We replicate these exercises for Veneto only by aggregating the microdata into monthly flows. In Figure 4 we report the flow of (net) newly-hired non-eligible workers in small firms from the one hand, and all the other flows on the other. Indeed, the Figure shows that the trends of the two groups are quite similar until January 2015, diverging significantly from then onwards. The results of a formal test, similar to the one presented in Table 4 is reported in the Appendix (Table A2).

5. Evidence from microdata: Veneto, from January 2013 to June 2015.

In this section we carry out several exercises based on microdata for Veneto. We look at the effects of each of the two policies from the perspective of both workers and firms. Our first

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[15] Other time intervals give similar results; they are available upon request.

exercise is a simple decomposition in the spirit of the aggregate estimates presented in the previous section. It allow us to get a rough estimate of the impact of the two policies on flows, in a setting which controls for firms' unobserved heterogeneity. Second, we focus on permanent contract hires and we identify the effect of FC by comparing the change in monthly hiring in small vs large firms after the FC reform. Then, we consider HS. First, we look at the change in the monthly hiring flow of eligible vs non-eligible workers in the subset of small firms unaffected by the FC policy. Then we look at the policies from workers' perspective. A clean identification of HS is obtained by estimating the monthly probability of finding an open-ended contract before and after HS (irrespective of firms' size). Last, contract conversions from fixed-term to open-ended allow us to estimate the separate effects of both the HS and FC, by comparing the conversion rate of eligible vs non-eligible workers in firms of different size classes.

Since we use microdata from January 2013 to June 2015, our exercises must take into account the anticipation effects described in Section 4. To solve this problem in all the exercises the time of the relevant policy change (the so-called "post" period) is the announcement of the policy. In this way the estimated effects are net of any anticipation or postponement of firms' hiring behavior, because they are the average between the plausible reduction of flows from the time of the announcement to the implementation of the policies (when firms were waiting for the implementation of the policies), and the increase in the immediate aftermath of their implementation (due to firms' postponement). A separate analysis at the end of this section will provide an assessment of these temporary substitution effects.

5.1. Decomposing aggregate flows.

We decompose the aggregate flows examined in the previous section into firm-level flows, using roughly the same approach and relying upon the comparison of affected flows and job-to-job transitions of non-affected workers in small firms. We use this flow as a proxy of what would happen to labour market flows as a consequence of business cycle, as more buoyant economic conditions may favour workers attempting to pursue their idiosyncratic preferences with respect to the firm where they work. We further split firm flows into two parts: the flow of those finding a permanent job position and the flow of those who find a fixed-term job. Thus, for each firm in our sample we can have 4 cells, given by the intersection of (i) the type of contract *c* (open-ended vs. fixed term) (ii) whether the workers involved are "eligible" (in the

sense of not having held a permanent contract over the 6 months period before the hire). For each firm we also know whether it is 15+ or smaller.

Since we have observations for 30 months and around 75,000 firms per month, we end-up with almost 9,000,000 cells. We then carry out the following decomposition:

$$[1] \qquad n_{iecym} = \gamma_i + \gamma_e + \gamma_c + \gamma_y + \gamma_m +$$
$$+\beta_1 D^{HS}_{(e=1)(c=p)(y,m \geq Oct14)} + \beta_2 D^{HS}_{(e=1)(c=f)(y,m \geq Oct14)}$$
$$+ \delta_1 D^{FC}_{(size\ i=15+)(c=p)(y,m \geq Dec14)} + \delta_2 D^{FC}_{(size\ i=15+)(c=f)(y,m \geq Dec14)} + \epsilon_{iecym}$$

where n_{iecym} is the monthly flow, the indices *i*, *e*, *c*, *y*, and *m* indicate respectively firm *i*, whether the flow is composed of eligible workers (*e=1*), *c* is the type of contract (*p* for permanent, *f* for fixed term) and *y* and *m* are year and month. Together with firm fixed effects γ_i the terms γ_e and γ_c are a fixed effects respectively for eligibility, and the type of contract; γ_y, γ_m are time dummies.

The variable $D^{HS}_{(e=1)(c=p)(y,m \geq PostOct14)}$ is a dummy equal to 1 if the cell corresponds to permanent hires of eligible workers occurred from October 2014 on (and 0 otherwise). The dummy $D^{FC}_{(size\ i=15+)(c=p)(y,m \geq Dec14)}$ is a dummy equal to 1 if the cell refers to permanent hiring in a 15+ firm occurred after the announcement of the Jobs Act (independently of eligibility). $D^{HS}_{(e=1)(c=f)(y,m \geq Oct14)}$ and $D^{FC}_{(size\ i=15+)(c=f)(y,m \geq Dec14)}$ differ from $D^{HS}_{(e=1)(c=p)(y,m \geq PostOct14)}$ and $D^{FC}_{(size\ i=15+)(c=p)(y,m \geq Dec14)}$ as they refer to fixed-term contracts. So, in equation [1] β_1 and δ_1 identify the direct potential impact of HS and FC respectively on open-ended contracts, while the terms β_2 and δ_2 capture substitution or complementarities induced by the policies on other types of contracts. Note that with this specification the total effect of the two policies is the sum of both βs and δs. To get aggregate estimates comparable with the ones presented in Table 4 for Italy or Table A1 in the Appendix one has to multiply each βs and δs by the number of cells corresponding to each coefficient and then sum up the results.

Last, even if not reported in equation [1], in all the specifications we include all the interactions between firm size, type of contract, eligibility, and also the local unemployment rate (i.e. the unemployment rate in the province where the firm is located) interacted with the type of contract, so as to capture further business cycle effects differentiated by contract type (to control for the fact that the business cycle has typically different effects on contracts characterised by

different degree of flexibility). Since flows within each firm are very likely to be correlated, standard errors are clustered at the level of firm.

The results of the estimation of equation [1] are in Table 5. (The Table reports only the direct impact of the policies on flows of permanent workers. The other coefficients are available upon request). P-values of clustered standard errors are within brackets). Columns 1-2 deal with gross hires, columns 3-4 with net hires.

The first column reports the basic specification and the coefficients of the dummies $D^{HS}_{(e=1)(c=p)(y \geq PostDec14)}$ and $D^{FC}_{(size\ i=15+)(c=p)(y \geq PostDec14)}$. the effect of both HS and FC are positive and highly significant. The second column includes the interaction term of HS and FC. In this specification the effect of HS remains positive and large while the effect of FC is entirely driven by the interaction term. This suggests that FC mainly strengthened the effect of the HS in the segment of large firms.

Columns 3 and 4 show that the cells subject to the effects of HS and FC registered an increase in the flow of open-ended contracts after the inception of the policies. The interaction term confirms that in large firms net job creation of open-ended positions was mainly driven by the combination of lower firing costs and hiring subsidies.

To determine the size of the estimated coefficients it is important to notice that, since we are dealing with a cell-level decomposition of monthly flows, the coefficients reported in the Table represent the (monthly) increase in cells affected by HS and/or FC. It is then useful to calculate the predicted monthly flow in 2015 and determine how much of this flow is due to either FC or HS. According to our model, in 2015 on average in Veneto there were around 7,000 hires with an open ended contract per month. Since we have 75,000 firms and the coefficient corresponding to HS is equal to 0.022, each month on average there were 0.022 X 75,000 = 1,649 open ended hires caused by HS. They correspond to 28 per cent of the flow of permanent contract hires occurred on average each month of 2015. The contribution of the FC policy on open-ended hires was much smaller as it accounted for just 4 per cent of total open ended hires. If we relate the estimated flows to total hires (i.e. including hires with fixed-term job contracts, which amount to roughly 65 per cent of total hires), HS accounted for 7 per cent and FC for 1 per cent.

Notice that while net job flows due to the policies are smaller than gross flows (compare the estimated flows of col. 1 to those in col. 3 and those in col. 2 to those in col. 4), the weight as a contribution to overall net job creation is larger (compare the same cols but looking at the estimated ratios). This is because the two policies contributed to create stable jobs which are

destroyed at a remarkably lower rate than temporary job contracts. Thus, about 1 out of four total net hires were due to the impact of the two policies on permanent employment. 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.[16]

Last, from the weight of HS in the flow of gross hires one can assess the deadweight loss of the policy itself, i.e. the amount of open ended gross hires which were financed by the HS but would have happened even without the subsidy. Taking into account that approximately 70 per cent of permanent hires involve workers eligible for the HS, 3 out of 5 of the hires of eligible workers would have happened even in the absence of the HS and so, the effective per capita cost of subsidized open ended contracts has to be multiplied by approximately 2.5 (each subsidy was worth around 6,500 euro per year according to INPS data).

5.2 Firms' hiring after the reform of FC

A more precise identification of the FC policy may be obtained by comparing open-ended hires and net job creation of small and large firms before and after the FC reform. Independently on the effect of HS, the reform of firing costs should increase the propensity of firms with at least 15 employees to offer open-ended positions to workers more than in firms below the threshold. We then look at the monthly gross and net flows of open-ended workers within each firm.

As already mentioned, we measure firms' size at the end of the previous year. In such a way we avoid the problem related to the possible manipulation of the treatment threshold induced by the policy itself. For observations in 2015 firms' size is measured at the end of 2014 which corresponds to the time of the announcement of the FC reform. We cannot carry out analysis at the 15-employee threshold because in our dataset firms' size is collected by size classes. Nevertheless, in some specifications we check whether the effect of FC differs across different size classes. Importantly, we exclude firms between 10 and 14 employees as they were also potentially affected by the Jobs Act, which allowed then to maintain lower firing costs even in case they reached the 15- employee threshold.

Using the same notation as in equation [1] we select the flow of open-ended contracts and we estimate:

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[16] 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.
[17] A more suitable, but rather complex estimation model could be a panel duration model.

$$[2] \qquad n_{iym} = \gamma_i + \gamma_y + \gamma_m + \gamma_1\, X_{i(y-1)} + \delta_1 D^{FC}_{(\text{size } i=15+, ym \geq Dec14)} + \epsilon_{iym}$$

where n_{iym} is the flow of firm i, in year y and month m, $X_{i(y-1)}$ is firms' size at the end of year y-1, γ_i, γ_y, γ_m , are respectively firm fixed effects and year and month dummies, $D^{FC}_{(\text{size } i=15+, ym \geq Dec14)}$ is a dummy for observations in firms 15+ after the announcement of the reform of firing costs (December 2014) and ϵ_{iym} is an error term. The coefficient δ_1 measures the effect of the reform of FC on permanent employment, identified with respect to firms with less than 15 employees. As in equation [1] we include also the local unemployment rate, here interacted with firm size to capture differences in business cycle among different types of firms (in any).

The first two columns of Table 6 refer to gross and net open-ended hires respectively. To avoid the fact that larger firms have naturally larger flows, in columns 3-6 we consider the probability of hiring (col. 3) and the probability to increase the size (positive net hire). In these regressions we also split the FC effect by detailed firms' size class. Moreover, since we look at probabilities independently on the size of flows, columns 3 and 4 report estimates with no fixed effects, whereas columns 5 and 6 include them. In this way it is possible to check whether the exclusion of fixed effects modifies the results (and indirectly check whether our sample is affected by selection bias).

Within this setting, which allow for a more direct and clean identification of the effects of FC, we find that the FC reform has positive, and highly significant effect for both gross and net hires. The estimates about the probability of hiring confirm that all sizes benefitted for lower firing costs. Instead if we look at the probability of a positive net flow (column 4) we find that net job creation increased only for firms relatively closer to the threshold. No relevant differences are found if we include fixed effects.

To check for consistency of these results with the decomposition presented in Table 5, we estimate the monthly flow of open-ended contracts that can be imputed to FC (columns 1 and 2, bottom part). We find that FC contributed by 7% to total gross and net hires of permanent workers in 2015. These numbers are roughly comparable with those presented in Table 5.

Last in column 7 we carry out a different exercise. In section 3 we noticed that after the implementation of the two policies the share of known workers in total inflow increased considerably. However, lower FC should increase the propensity for firms to hire directly

unknown workers, because, after the FC reform, it could be easier for firms to dismiss them in case of a bad match. We compute the share of known workers in the open-ended gross hires (including conversions) taking place each month in an active firm (where active refers to the occurrence of open–ended hires). The effect of the FC policy is captured by a term interacting a dummy for the introduction of the new FC and the firm class size. As expected we find a negative sign. While statistically significant, the effect is however rather small. On average the share of known workers in total open-ended hires decreased by around 3 p.p., having been on average equal to 53 per cent.

5.3 Hiring subsidies: evidence for small firms.

It is not possible to get good estimates of the effect of hiring subsidies for open-ended job positions by the use of firm-level data, as the eligibility/non-eligibility condition applies to workers. Therefore, to evaluate whether the HS had an effect on labour market flows we will rely mainly on the individual dimension of our dataset (Section 5.4). Nevertheless, preliminarily, using the same strategy as in Section 5.1 we look at cell-level flows, and focus on flows in firms with less than 10 employees, i.e. far below the 15-employee threshold, as these flows are not likely to be affected by FC changes. Using the same notation as in Equation [1], we then carry out the following decomposition:

[1']

$$n_{iecym} = \gamma_i + \gamma_e + \gamma_c + \gamma_y + \gamma_m +$$
$$+\beta_1 D^{HS}_{(c=p)(e=1)(y,m\geq Oct14)} + \beta_2 D^{HS}_{(c=f)(e=1)(y,m\geq Oct14)} + \epsilon_{iecym}$$

The first column of Table 7 reports the coefficient corresponding to the cells affected by HS (β_1). Since we are again considering quite homogenous size classes, the results in column 1 do not include fixed-effects. The opposite occurs in column 2. In both cases the effect of HS is almost identical, positive, highly significant and comparable with the one reported in Table 5 (based on the overall sample of firms).

In the third column flows are further split according to the known/unknown status of worker. The results suggest that the open-ended gross flows favoured by HS were exclusively those involving known workers. To some extent such a result may be a bit too extreme and due to the fact that we are considering only the first months after HS inception, a period when firms had a backload of already known workers among whom to pick up possible candidates for permanent positions. We would expect that, after having exhausted such a backload of already

known workers, firms may have started to hire "unknown" job candidates. Another possible reason of this extreme result is that, as argued in section 2, an unconstrained firm would always prefer to hire on a temporary basis and later on decide whether to convert the contract into an open-ended arrangement. The HS policy has likely increased the search for unknown workers to be temporarily hired and tested before December 2015, period until which the full amount of the subsidy could have been cashed in. Only to the extent that a more buoyant labour market strengthens workers' bargaining power, allowing them to find alternatives instead of accepting such a testing period, what we called the "two-step strategy" becomes riskier for the firm, inducing it to hire directly on an open-ended basis an unknown worker. Unfortunately the local market conditions did not change very much over a single semester and within a single region in order to further explore such an explanation.

In columns 4 and 5 we look more directly at the composition of gross open-ended hires, in the firms with such a flow in the given month. In the first case we simply look at the share of eligible workers in total open-ended gross hires occurred in the firm. The share increased by 17 p.p. after the inception of the HS policy, a remarkable effect when taking account that it was about 67% in the two previous years. In the second (column 5) we select the firms with positive open ended hires of eligible workers and consider as dependent variable the share of known workers in those total hires. Once again we find that after October 2014 the share of known workers increased substantially.

5.4 Workers' job finding probability

A direct identification of the effect of hiring subsidies can be obtained by the use the individual-level panel dimension.

We analyse the individual probability to find an open ended job (conditional on being in the labour market in 2013 or in 2014) and the probability that the job contract of a fixed-term worker is converted into an open-ended, according to the eligibility status of workers (and in some regressions, according also to firm size).

We first 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. Notice that when considering these individuals, we may only observe the effects of the HS, as firm size is a characteristic of the firm and not of the worker. We then define a variable π_{weym} which is equal to one if the worker w-*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 fixed-term). As before the subscript *e* denotes the eligibility status of workers.

We then estimate the following model:

$$[3] \qquad \pi_{weym} = \gamma_w + \gamma_e + \gamma_y + \gamma_m + \beta_1 \, D^{HS}_{(e=1)(y,m\geq Oct2014)} + \epsilon_{weym}$$

where γ_w are individual fixed effects, aimed at capturing workers' unobserved heterogeneity, $\gamma_e, \gamma_y, \gamma_m$ are fixed effects for workers not having a permanent job in the previous semester (*e*), for year *(y)* and month *(m)*. The variable $D^{HS}_{(e=1)(y,m\geq Oct2014)}$ is a dummy equal to 1 if the observation refers to the period from October 2014 onwards and involves workers eligible for the hiring subsidy. We also include the local-level unemployment rate interacted with eligibility to capture group-specific business cycle effects. Moreover, since we have a panel of individuals and the variable π_{weym} typically does not change after a person has found a permanent job, we drop the worker from the sample after (s)he finds a permanent job (unless after some time she re-enters the pool of job seekers because of a job destruction).[17] The comparison group here is composed of workers who had a permanent job positions in the previous 6 months and workers with an apprenticeship job contract, whose contract conversion cannot receive the HS.

The second exercise looks at conversions of fixed-term contracts into open-ended contracts within a given firm (a subset of the transitions described above). This flow is directly affected by both policies, although along different lines, depending on the employee's work history (relevant for the HS) and the firm size (relevant for FC). More specifically, we look at the probability of a worker with either an apprentice or a standard fixed-term contract to have her contract converted into an open-ended contract by the same firm.[18] 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:

[4]

$$\pi_{weym} =$$

$$\gamma_w + \gamma_e + \gamma_y + \gamma_m + \beta_1 D^{HS}_{(e=1)(y,m\geq Oct2014)} + \delta_1 D^{FC}_{(size\ w\ works=15+)(y,m\geq Dec14)} + \epsilon_{weym}$$

The dummy $D^{FC}_{(size\ w\ works=15+)(y,m\geq Dec2014)}$ is equal to 1 if workers *w* is in a 15+ firm and observations refer to the period following the announcement of the FC reform. Year and month

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[17] A more suitable, but rather complex estimation model could be a panel duration model.
[18] Also at the cell level we only consider the conversions stemming from apprentices and standard fixed-term contracts taking place in a given firm.

dummies account for time trends. Conversions in small firms after December 2014 represent the control group used to identify the effect of the FC.

Last, to strengthen identification and eliminate possible source of remaining heterogeneities, we focus upon those who had a fixed term job contract in two single months, October 2013 or October 2014 and we look at the monthly probability that their contract is converted in each single month during the period November-June, according to their eligibility status and the size of firms where they work. The comparison group is then composed of fixed term workers in October 2013.

The first and the second columns of Table 8 display the results for the probability that a person without a permanent job at time t obtains it at time $t+1$ (with no fixed-effects and with fixed-effects, respectively). The impact of HS (column 1) is positive, significant and large as it implies an increase in the monthly probability to find a job by 0.1 p.p. out of an average of 0.8 (reported in the last row of the Table).

Columns 3-7 refer only to fixed-term workers at time t. They are a subset of the sample used for the estimates reported in column 1. We focus upon the probability of a conversion from a temporary position in a given firm at time t to an open-ended position in that same firm at time t+1. This exercise allows us to separately estimate the effects of HS and FC as we observe the size of the firm where individuals work. The results confirm the general message of previous Tables. Both policies had a positive impact (col. 2), but the effect of FC was mostly in reinforcing the effect of HS in the largest firms (in col. 3, the own impact of FC is slightly negative even if its statistical significance is only marginal). Besides being positive, these effects are quite sizable, when compared to the corresponding average monthly probability.

Workers active in the labour market in the period January 2013-October 2014 do not include new entrants, but their labour market status might still depend upon unobservable factors interacting with the two policies and the time in which they are enacted. Thus, we further select our sample and we limit our analysis to a subset of workers holding a fixed term contract in October 2014, i.e. at the time of the announcement of the HS policy. The pre-policy sample is composed of workers who had a fixed term contract in October 2013. A test for difference in trends before the inception of the reforms supports the hypothesis that conversion probabilities of eligible and non-eligible workers were not statistically different from zero in the pre-period (results available upon request). As before, we look at the probability of conversion to an open-ended position in the period November- June 2014 for the "pre"-sample and November-June 2015, for the treated samples.

As before, in column 6 the HS effect is here identified by the eligibility status of workers, whereas the FC effect derives from the firm class size. The effect of HS is sizable and comparable with the previous findings: it implies that the average monthly conversion probability more than double with respect to its pre-policy average. A positive but smaller and statistically not significant effect is found for the FC.

Column 7 is equivalent to column 6 but it includes only workers in firms with less than 10 employees. Here only the HS effect is identified, with the potential advantage that there is no confounding effect stemming from the enactment of the FC policy (which does not affect this segment of firms). The effect of HS are unaffected by this sample selection.

5.5 Timing effects

In the previous sections we looked at changes in flows occurred after the announcement of the two polices. Our estimates are average effects, net of the temporary substitution effects related to firms' strategic choice of postponing some hires to benefit from the policies. In the Appendix we quantify the relevance of these timing effects. For the estimation of the anticipation effect of FC we use data until February 2015 and, as in the previous estimates, we use the announcement as the time of the treatment; that is, dates before December 2014 are the "pre" period, and dates between December 2014 and February 2015 are the "post". The coefficient measures the reduction of flows due to the postponement of hiring after March 2015.

For HS we end the sample in December 2014. The "post" period spans from October to December 2014. We then replicate the estimates in Table 5, 6 and 7. Our results (see Tables A3 and A4) suggest that the anticipation effect is negligible for the FC reform, more sizable for HS.

5.6. Robustness checks

We have carried out several robustness checks. First, it is still possible that flows of firms operating in the same sector are correlated because of unobserved sector-level shocks. To check for this possibility we have carried out some estimates with standard errors clustered by firm and sector (45 sectors). The results remain unchanged (estimates available upon request). Second, our sample is composed of firms whose main branch is located in Veneto. We have excluded firms having just a local unit in Veneto because these firms could be potentially affected by shocks occurring in the region or foreign country where the main branch is located. In some unreported estimates (available upon request) we have replicated our analysis including also local units. The final sample is composed of more than 135,000 local units (mostly

corresponding to the population of local units, according to INPS data). Also in this case the results are unaffected.

6. Conclusions

In this paper, using a diff-in-diff approach, we analyse the reaction of firms to two policies introduced in 2015 by the Italian government, aimed at both reducing labour market dualism and favouring job creation. The first is a generous hiring subsidy 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 the 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.

Our estimates do not consider all the aspects relevant in judging the appropriateness of the two measures (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 subsidies vis-à-vis permanent subsidies targeting specific groups of workers supposed to be weaker and less employable (e.g. youths and long term job seekers). Furthermore, we do not deal with the merits and pitfalls of subsidies for gross hiring which subsidizes also part of the normal turnover taking place in a firm. The policy also favours the conversion of temporary contract into permanent ones and the poaching of suitable temporary workers from one firm to another. So, hiring subsidies could have lifted up temporary hiring as well.

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 *contratto a tutele crescenti* does not modify 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, 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's seniority. Furthermore, the monetary cost for firms has been reduced 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. It may also induce a 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 effects, 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 for firms' and workers' decisions.

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 subsidies (i.e. subsidies targeted to population groups considered less easily employable) and the trade-off 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 subsidy provided by the law, with a strengthening of such an impact in the 15+ employees firms thanks to the new FC. A relevant longer run effect of the new FC comes from the fact that it made firms slightly less reluctant in hiring on a permanent basis a yet untested worker.

All types of subsidies 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 HS was not an exception to this rule. There is evidence that firms postponed planned hires in order to exploit the timing of the temporary subsidy provided by the law. Furthermore, firms acted "strategically" also along other lines. Given the fact that workers eligibility to the HS was related to the absence of a permanent job in the previous semester, 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).

Last, our estimate allow us to assess that the deadweight loss of the HS was of about 3 out of 5. 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: Italy; total net job creation of open-ended contracts (left-hand axis, thousand) and share of open-ended contracts in total new contracts (right-hand axis).
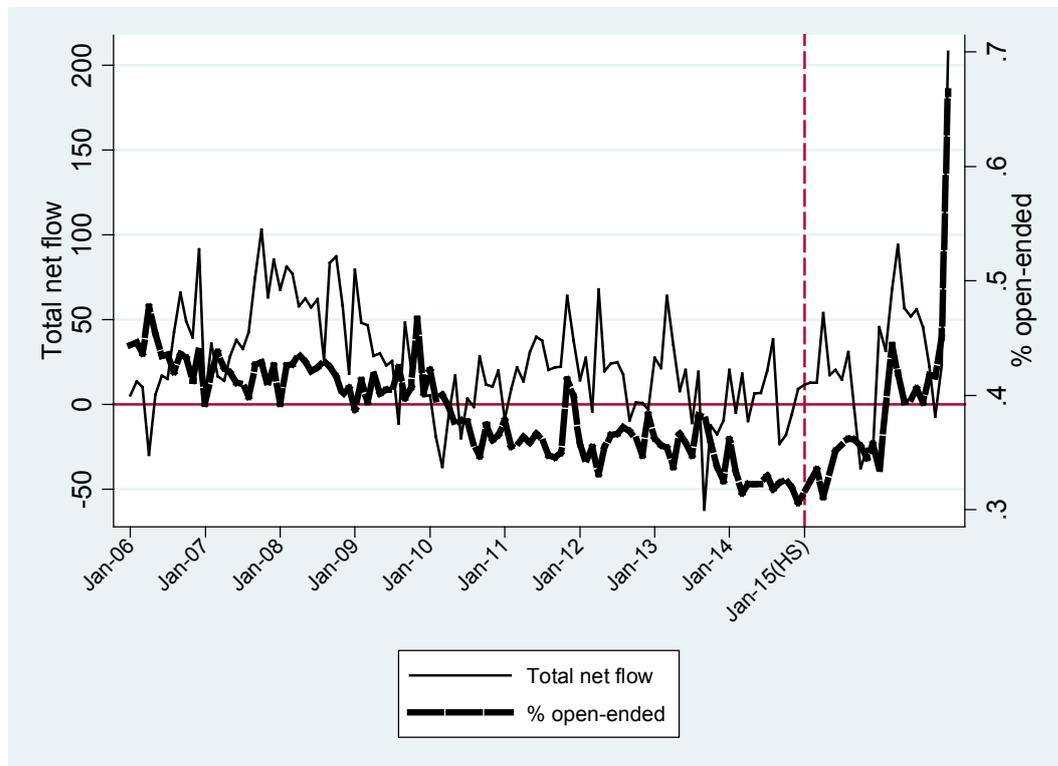
Source: authors' calculations on INPS data, Private sector excluding agriculture. Seasonally adjusted.

Figure 2: Veneto; net job creation by type of job contract, eligibility status of workers and firm size.

a- By type of contract
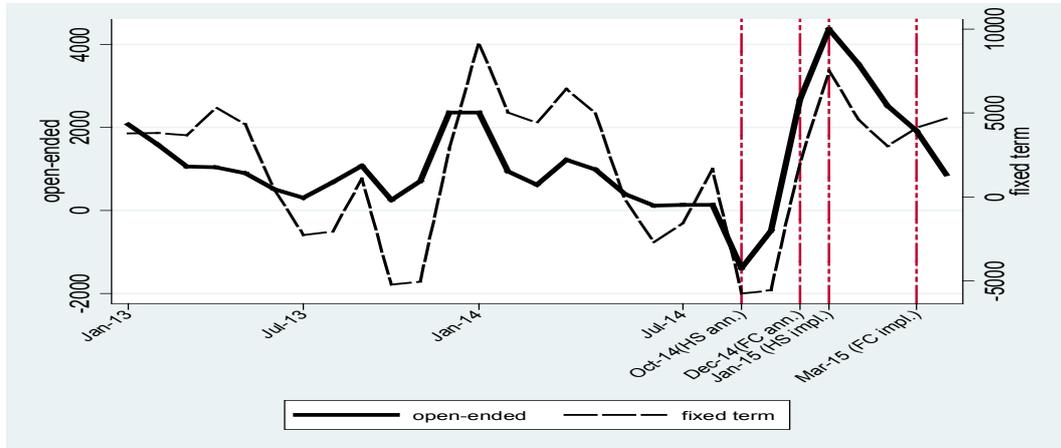
b- By size
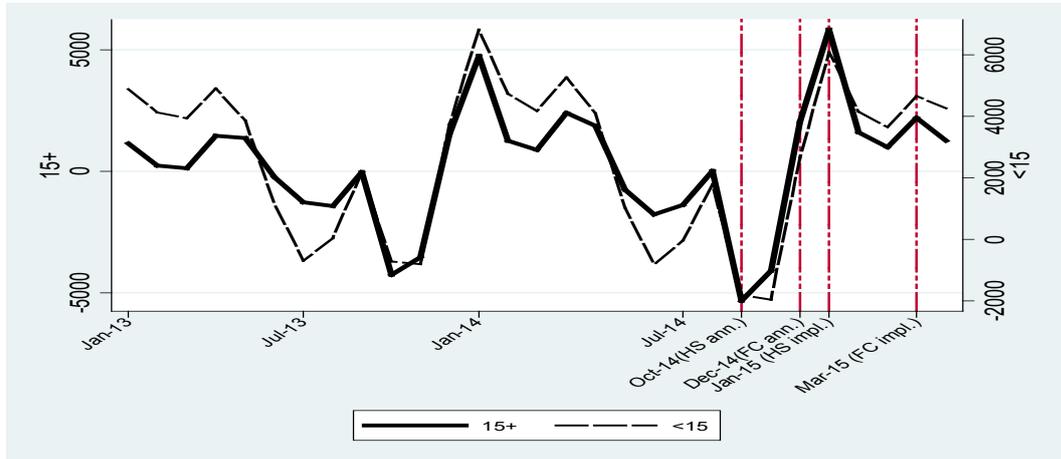
c- By eligibility
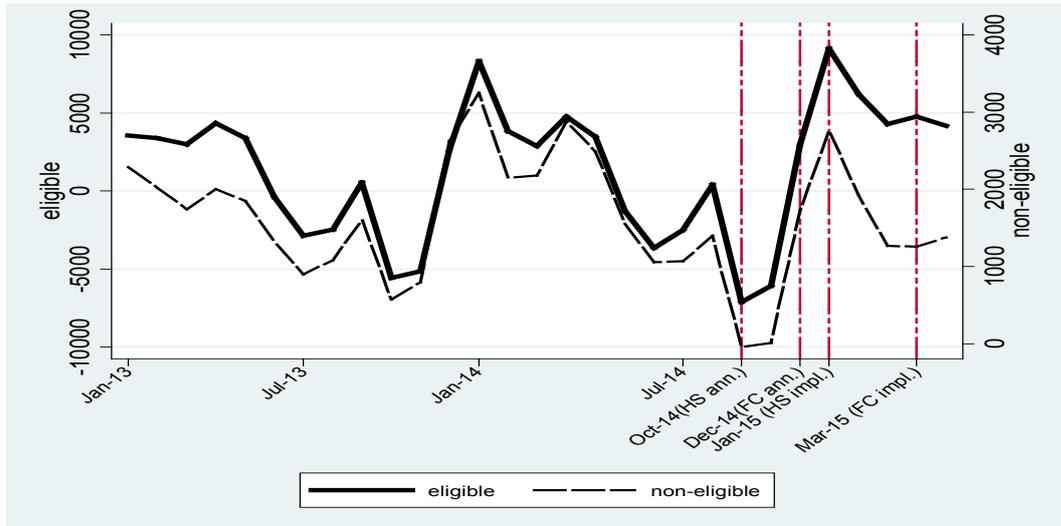
Source: authors' calculations on Veneto CO data. Sample of firms already operating in 2013 and 2014, i.e. before the implementation of the policies. Net job creation is the difference between jobs created and destructed. Open ended job creation includes contract conversions. Data smoothed to attenuate the effect of seasonality.

Figure 3: Veneto; hires (all types of contracts) by previous relationship with the worker and conversions

a- Workers known to the firm (i.e. employed in the past in the same firm) . Ratio to the number of workers hired with an open-ended contract in the same month.
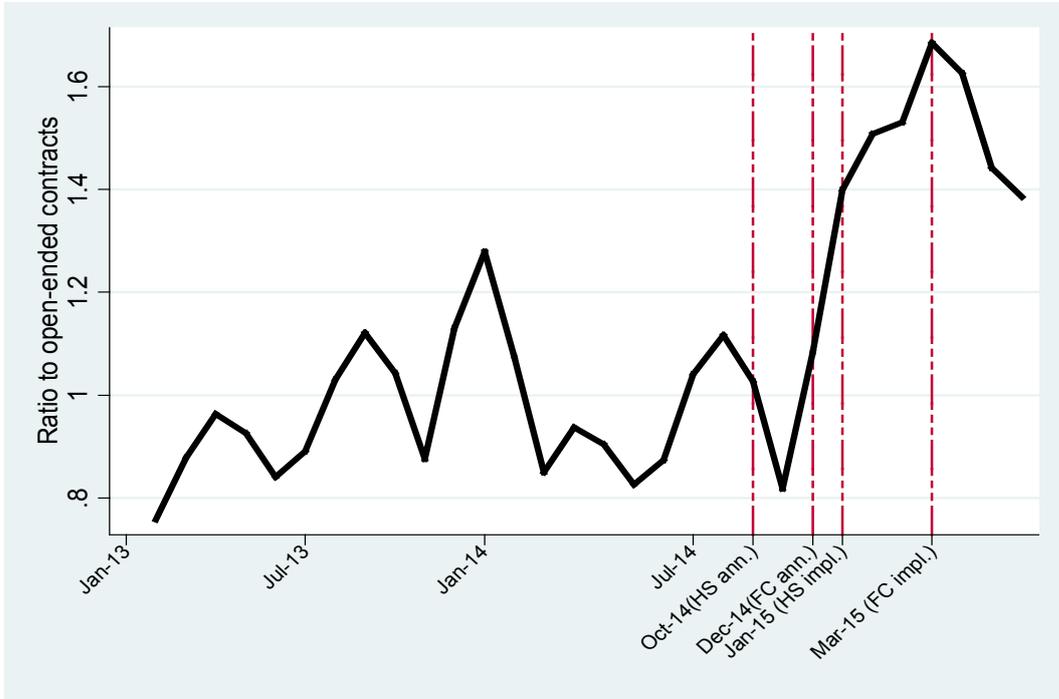
b- Conversions as ratio to total fixed-term job contracts and values.

Figure 4: Veneto; net job creation by treatment status: flow of non-eligible workers in small firms (non-treated) vs. all the other flows.

Table 1: Italy; Composition of flows (gross and net hiring) by eligibility and firm size.

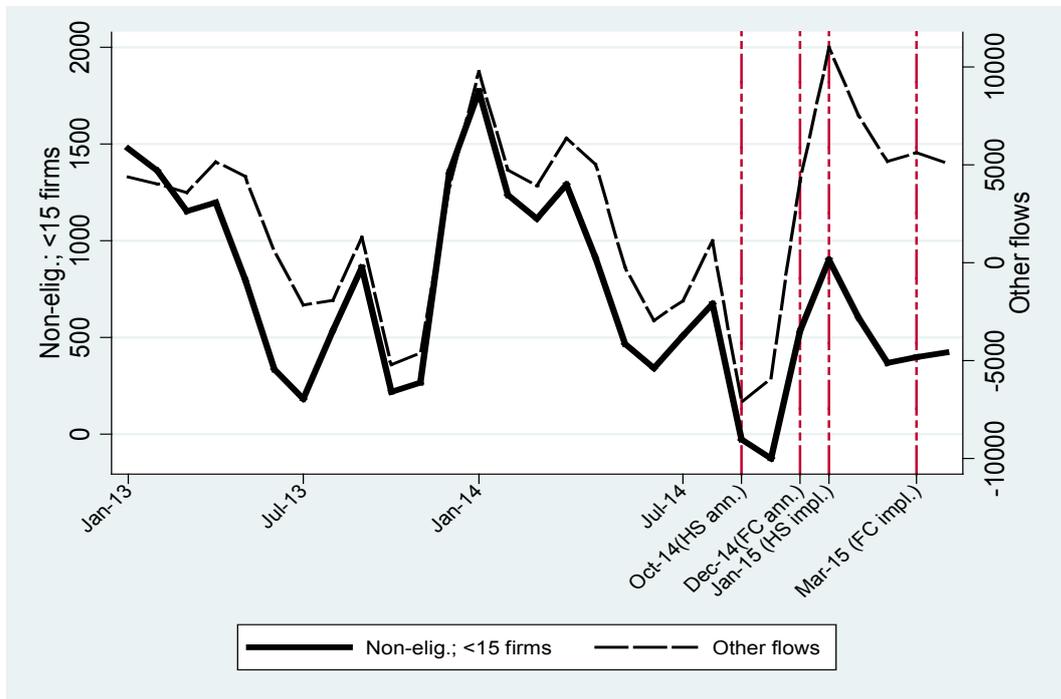|	Gross			Net		
	Less than 15	15+	Total	Less than 15	15+	Total
Non eligible	13.3	7.4	20.7	16.6	4.6	21.2
Eligible	47.5	31.8	79.3	50.9	27.9	78.8
Total	60.8	39.2	100.0	67.5	32.5	100.0

Source: Authors' calculations on INPS data. Firms with at least one employee excluding firms hiring their first employee in 2015. Firm size measured at the end of the previous year.

Table 2: Veneto; Composition of flows (gross and net hiring) by eligibility and firm size.

	Gross			Net		
	Less than 15	15+	Total	Less than 15	15+	Total
Non eligible	14.5	9.4	23.9	20.4	9.7	30.1
Eligible	45.2	30.9	76.1	43.8	26.1	69.9
Total	59.7	40.3	100.0	64.1	35.9	100.0

Source: Authors' calculations on Veneto CO data. Firms with at least one employee excluding firms hiring their first employee in 2015. Firm size measured at the end of the previous year.

Table 3: Veneto; Sample composition. Shares.

	Firms		Workers					
	Size		Eligibility		Past working condition		Past relationship with firm	
	Firms <15	Firms 15+	Non-eligible	Eligible	Unemployed	Working fixed term	Known	Unknown
Shares (%)	90	10	28	72	50	50	40	60

Source: Authors' calculations on Veneto CO data. Firms with at least one employee excluding firms hiring their first employee in 2015. Firm size measured at the end of the previous year.

Table 4: Italy: Net job creation for treated and controls. Difference in trends before and after the inception of the two 2015 policies.

VARIABLES	(1) Whole period	(2) Period May-June of each year
Affected*2011	5,197	9,251
	(0.419)	(0.496)
Affected *2012	-1,640	5,680
	(0.860)	(0.781)
Affected *2013	-919	4,990
	(0.874)	(0.748)
Affected *2014	172	9,940
	(0.984)	(0.258)
Affected *2015	48,461***	42,424***
	(0.001)	(0.001)
Observations	240	40
Average monthly flow	11,127	75,309

Source: Authors' calculations on INPS data. Robust p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Veneto; Firm-level decomposition of flows and the effects of the policies: gross and net hiring.

	(1)	(2)	(3)	(4)
	Gross		Net	
β_1 (effect of HS)	0.022	0.015	0.015	0.014
	[0.000]***	[0.000]***	[0.000]***	[0.000]***
δ_1 (effect of FC)	0.039	-0.02	0.011	0.003
	[0.000]***	[0.000]***	[0.000]***	[0.255]
Interaction (HS * FC)		0.12		0.018
		[0.000]***		[0.001]***
Fixed effects	Yes	Yes	Yes	Yes
Observations	8,992,200	8,992,200	8,992,200	8,992,200
Number of firms	74,935	74,935	74,935	74,935
Number of cells HS	74,935	74,935	74,935	74,935
Number of cells FC	7,666	7,666	7,666	7,666
Number of cells HS*FC		7,666		7,666
Estimated flows	1,948	1,891	1,208	1,210
Of which				
HS (β_1 * no. cells HS)	1,649	1,124	1,124	1,049
FC (δ_1 * no. cells FC)	299	-153	84	23
HS*FC (Interaction + no. cells HS*FC)		920		138
Monthly flow in 2015 (open ended)	7,052	7,052	2,347	2,347
Est. flow as ratio of total flow (open ended; %) Of which:	28	27	51	52
HS	23	16	48	45
FC	4	-2	4	1
HS*FC		13		6
Monthly flow in 2015 (total)	24,944	24,944	5,256	5,256
Est. flow as ratio of total flow (all contracts ; %)	8	8	23	23
HS	7	5	21	20
FC	1	-1	2	0
HS*FC		4		3
For memo: average monthly flow (whole sample) Open ended	2,312	2,312	839	839
Total	10,028	10,028	3,471	3,471

Source: authors' calculations on Veneto CO data. Clustered standard errors (at the firm level); p values in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All the estimates include firm fixed effects, year and month dummies, an interaction term for flows of fixed-term contracts of eligible workers in the post period, an interaction term for flows of fixed-term contracts in large firms in the post period the province-level unemployment rate interacted with the type of contract, all the direct and joint effects of variables measuring eligibility, firm class size and contract type.

Table 6: Veneto; firms 15+ vs. others: gross and net hiring of permanent workers.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Gross hires	Net hires	Prob. Hiring	Prob. positive net flow	Prob. Hiring	Prob. positive net flow	Ratio known workers in gross hires (cond. on hiring)
δ_1 (effect of FC)	0.048	0.013					-0.039
	[0.000]***	[0.091]*					[0.000]***
Interaction (FC*Firm15-49)			0.011	0.010	0.011	0.01	
			[0.000]***	[0.000]***	[0.000]***	[0.000]***	
Interaction (FC*Firm50-99)			0.020	0.004	0.020	0.004	
			[0.000]***	[0.391]	[0.000]***	[0.391]	
Interaction (FC*Firm100+)			0.027	-0.002	0.027	-0.002	
			[0.001]***	[0.769]	[0.001]***	[0.769]	
Fixed effects	Yes	Yes	No	No	Yes	Yes	Yes
Observations	2,128,803	2,128,803	2,128,803	2,128,803	2,128,803	2,128,803	100,407
Average	0.081	0.024	0.052	0.046	0.052	0.046	0.576
Monthly flow in 2015 (open ended); ratio of known workers in column 5	6,268	2,116	-	-	-	-	-
Est. flow as ratio of total flow (open ended; %)	6%	5%	-	-	-	-	-

Source: authors' calculations on Veneto CO data. Robust standard errors; p values in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Linear probability models in columns 3-6. Firms in the size class 10-14 employees are excluded. All models include firm fixed effects, year and month dummies interacted with firm class-size, and the province-level unemployment rate interacted with firm class-size.

Table 7: Veneto; Flows of eligible workers in small firms: gross hiring.

	(1)	(2)	(3)	(4)	(5)
	Flows	Flows	Flows	Share eligible (conditional on being hired with a permanent job contract)	Share known workers (conditional on being hired among eligible workers and with permanent job contract)
β_1 (Effect of HS)	0.015	0.014	-0.003	0.166	
	[0.000]***	[0.000]***	[0.000]***	[0.000]***	
Interact.(HS*Known)			0.019		0.029
			[0.000]***		[0.000]***
Fixed effects	No	Yes	Yes	Yes	Yes
Observations	7,524,072	7,524,072	15,048,144	74,068	51,426
Average	-	-	-	0.675	0.457
Number of firms	62,701	62,701	62,701	-	-
Number of cells HS	62,701	62,701	62,701	-	-
Monthly flow in 2015 (open ended)	4,881	4,881	4,881	-	-
Est. flow	941	941	941	-	-
Est. flow as ratio of total flow (open ended; %)	19%	19%	19%	-	-
Monthly flow in 2015 (total)	13,154	13,154	13,154	-	-
Est. flow as ratio of total flow (all contracts; %)	7%	7%	7%	-	-

Source: authors' calculations on Veneto CO data. Clustered standard errors (at the firm level) in columns 1-3; robust standard errors in columns 4 and 5; p values in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All the estimates include firm fixed effects, year and month dummies, the province-level unemployment rate interacted with the type of contract and with eligibility (and in model 1 and 2 only also the direct effect of eligibility and its interaction with contract type). Equation in column 2 includes also the variable measuring the known status, also interacted with eligibility and contract type.

Table 8: Veneto; Individual probability to find an open-ended job.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Prob. hired open ended	Prob. hired open ended	Prob. conversion	Prob. conversion	Prob. conversion	Prob. conversion	Prob. Conversion
						Being Fixed-term in October	
							In small firms
β_1 (effect of HS)	0.014	0.001	0.010	0.011	0.011	0.013	0.013
	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***
δ_1 (effect of FC)				0.002	-0.001	0.0007	
				[0.001]***	[0.065]*	[0.536]	
Interaction (HS * FC)					0.004		
					[0.000]***		
Fixed effect	No	Yes	No	Yes	Yes	Yes	Yes
Observations	11,200,000	11,200,000	972,757	972,757	972,757	577,105	503,899
Average prob. per month (%)	0.009	0.009	0.010	0.010	0.010	0.0116	0.0132

Source: authors' calculations on Veneto CO data. Linear probability models. Robust p values in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All the estimates include individual fixed effects, year and month dummies, the province-level unemployment rate interacted with eligibility (and firm size in columns 2-4). Equation in columns 2-4 include also firm size and the interaction between firm size and eligibility.

Appendix:

(a)   Aggregate data for Italy, for which a longer time series is available, can be used also for another exercise, which allows us to control for possible autocorrelation in diff-in-diff estimates. We calculate, for each month, the difference between the flows of treated and non-treated workers and we check whether it significantly increased because of the two policies. To ensure that our results are not driven by the very high spike observed in December 2015, we limit our analysis to the period January 2006-June 2015. Further, to control for possible anticipation effects since the announcement of the first policy in October 2014, we look at changes in the difference since that date. The estimated monthly variation is then the average effect of the policies in the period October 2014-September 2015. In other words these estimates correspond to the average effect between the possible drop of flows in October-December 2014 and possible spikes immediately after the implementation, due to postponement strategies within the period. In doing so, we apply also Newey-West estimator, to adjust estimates for possible auto-correlation in residuals.

The results of this exercise are in Table A1. The first column report the OLS estimate. The second is based on the Newey-West estimator. The results confirm the existence of differences in trends during the period under scrutiny and, more importantly, that autocorrelation has a small impact on the precision of our estimates.

Table A1: Italy: Difference between the flows of treated and non-treated individuals (respectively eligible workers and workers in firms 15+, and non-eligible workers in small firms). The effect of autocorrelation. Data from January 2006 to June 2015; difference in trends estimated since the announcement of the first policy, i.e. from October 2014.

	(1)	(2)	(3)	(4)
	Gross flows		Net flows	
	OLS	Newey-West	OLS	Newey-West
Affected	49,787	49,787	29,962	29,962
	[0.000]***	[0.001]***	[0.001] ***	[0.092] *
Observations	114	114	114	114
Average difference in flows (1)	296,240	296,240	83,921	83,921

Source: authors' calculations on INPS data. P-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. National security institute, INPS. The models include month dummies. (1) First semester of each year.

(b)   We test also for Veneto that the lack of differences in trends before 2015.

In Table A2 we simply replicate Table 4, which refers to Italy as a whole. We look a pre-trend and at the effect of the policies on the two types of aggregate monthly flows. Since data are aggregated we have just 30 monthly observations for each of the two groups. Even with this limited time span, the difference between the size of gross and net hiring increases considerably since January 2015, and no relevant pre-trend emerges for the previous year.

Table A2: Veneto: Gross and net job flows of affected and non-affected workers and firms (grouped data). Difference in trends before and after the inception of the two 2015 policies.

	(1) Gross flows	(2) Net flows
Affected*year 2014	1,809	121
	[0.207]	[0.962]
Affected *year 2015	5,532	4,748
	[0.005]***	[0.057]*
Observations	60	60
Average monthly flow	10,694	2,947

Source: authors' calculations on Veneto CO data. P values in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%.

(c)   We estimate the impact of anticipation due to firms' strategies after the announcement of the policy changes. In particular, we focus on Veneto and the anticipation effect of the first announcement of HS in October 2014 and of the announcement of the FC reform at the end of December 2014. For the estimation of the anticipation effect of FC we use data until February, 2015 and we use the announcement of the FC reform as the treatment; that is, dates before December 2014 are the "pre" period, and dates between December 2014 and February 2015 are the "post". At the second stage we use all of the data. As mentioned in the main text this identifies the effect of anticipation.

We follow the same strategy for HS. We use data until December 31st, 2014, and we define observations until October 2014 as "pre"; those after October 2014 are instead in the "post" period.

Table A3 can be compared with the estimates presented in Tables 6, for gross and net flows respectively. This exercise shows that for FC the effect of anticipation was indeed quite small.

Table A4 looks at the anticipation effect of HS, using a specification which is fully comparable with the one used in Table 5 in the first column, with the one in Table 7 in the second column. While the overall message remains unchanged, the impact of anticipation for HS is slightly more sizable than for FC.

Table A3: Firms 15+ vs. others: the effect of anticipating the FC reform in gross and net hiring of permanent workers.

	(1)	(2)
	Prob. hiring (data until Feb 2015)	Prob. positive net flow (data until Feb 2015)
δ_1 (effect of FC)	-0.002	-0.004
	[0.449]	[0.113]
Observations	1,845,676	1,845,676
Average monthly probability	0.050	0.045

Source: authors' calculations. Robust p values in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Linear probability models. Firms in the size class 10-14 employees are excluded. All the estimates include firm fixed effects, year and month dummies interacted with firm class-size, the province-level unemployment rate interacted with firm class size.

Table A4: Flows of eligible workers in small firms: the effect of anticipating the HS subsidy in gross flows.

	(1)	(2)
	Flow (data until December 2014)	Flow in small firms (data until December 2014)
δ_1 (effect of FC)	-0.003	-0.001
	[0.036]**	[0.345]
Observations	7,193,760	6,039,984

Source: authors' calculations. Clustered standard errors (at the firm level); p values in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All the estimates include firm fixed effects, year and month dummies, the province-level unemployment rate interacted with the type of contract and with eligibility and the direct effect of eligibility and its interaction with contract type.