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IZA DP No. 12748

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ABSTRACT

Combined and Distributional Effects of EPL Reduction and Hiring Incentives: An Assessment Using Non-Linear DiD[1]

Two decades of unsuccessful marginal labour market reforms provided the political support to reduce the flexibility gap between temporary and open-ended workers by means of a retrenchment of the employment protection benefitting the latter. To support employment levels during the crisis years, these policies have generally been combined with generous employment subsidies. While the theoretical and empirical literature on the two interventions taken in isolation appears generally abundant, almost nothing is known when they come combined. Analogously, no evidence is available about their distributional effects. This paper aims at filling these two gaps by means of non-linear difference-in-differences duration models estimated on high-frequency employer-employee linked Italian data. Taking advantage of the quasi-experimental conditions set by the Italian "Jobs Act", we find that large firms are less sensitive than small ones to hiring subsidies, unless they come combined with lower firing costs. Small firms substitute temporary for permanent employment, while larger ones do not seem to give up on fixed-term contracts, possibly as a probationary period. The reforms have benefitted domestic workers over foreigners, and those with a lower or more general human capital. No gender effects emerge.

JEL Classification: J08, J63

Keywords: employment protection legislation, hiring incentives, non-linear DiD, duration models, impact evaluation, jobs act

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1. Introduction

Partly due to the good economic performance of some low-protection countries [Elmsekov et al. 1998; Grubb and Wells 1993], deregulation has been a hallmark of labour market policies in advanced economies during the last three decades [Berton et al. 2012]. Advised in the early nineties as a way out of Eurosclerosis [IMF 1999; OECD 1994], until the recent financial crisis deregulation mostly took the form of less binding regulations on temporary employment [Barbieri et al. 2016; Boeri 2010; Cappellari et al. 2012] or, as otherwise called, of *marginal reforms.* The advantages from the resulting *dual labour markets* in terms of productivity and employment opportunities for the marginal workers, however, did not come true [Bentolila et al. 2019]. When the crisis hit, hence, the advice from the EC [Bekker, 2017] and from the OECD [2014] to reduce the gap between temporary and permanent workers, found good grounds in all the EU countries [Eichhorst et al. 2017]. This happened in particular where the pressure for reducing the public debt – Italy, Spain – created the political capital to proceed to unprecedented reductions of employment protection legislation (thereafter EPL) on open-ended contracts [Hastings and Hayes 2016; Meardi 2014; Perez and Rhodes 2014; Picot and Tassinari 2017; Sacchi 2015, 2018]. Alongside EPL convergence between temporary and open-ended contracts (OEC), and in order to support employment during years of growing unemployment, a number of countries also proceeded to introduce generous hiring incentives (HI: see OECD, 2010).

The effects of EPL on employment have been widely studied, and appear rather consistent across theory and empirical evidence. EPL reductions *per se* increase workers' turnover without necessarily implying higher employment levels [Bentolila and Bertola 1990; Bertola 1990; Cazes 2013; Noelke 2016; OECD 2004], if not through indirect effects occurring through more efficient retention [Bassanini and Ernst 2002; Bierhanzl 2005] and workers' allocation on existing jobs [Berton et al. 2017; Hopenhayn and Rogerson 1993; Rogerson 1987]. Less clear-cut results may emerge once the dual nature of contemporary labour markets is considered [Boeri 2010]. A reduction of employment protection legislation on open-ended contracts is expected to ease the transitions to this kind of contract from temporary ones, and hence to reduce labour market segmentation and to benefit those workers – women, non-nationals, the youth – that are more likely to hold a temporary job [Bertola 1999; Lazear 1990]. Sestito and Vivano [2018] find a positive effect of a retrenchment of EPL on open-ended workers onto the transitions from temporary to permanent jobs, but the distributional effects across workers that may follow from EPL convergence have been so far – to the best of our knowledge – neglected in the empirical literature.

More surprisingly, less is known about the effects of hiring subsidies. Although in theory the effect is clear – a lower cost of labour raises its demand – the empirical evidence is rather scarce [Cahuc et al. 2019; Neumark and Grijalva 2017]. Ciani and De Blasio [2015] exploit the limited availability of funds to assess the impact of monetary incentives to transform temporary into open-ended contracts, and find that this effect is positive and sizeable (+83%) in a large Italian region. Berson and Ferrari [2015] calibrate a standard job search model on French data and compare the performance of three different types of hiring subsidies, suggesting that

a tax on temporary employment to fund incentives for open-ended hires performs better than experience-rated contributions and of a tax on separations. Sestito and Viviano [2018] take advantage of a recent reform of the Italian labour market and show that hiring incentives have been a driving force of open-ended employment creation in 2015, as they explain 20% of direct creation of permanent positions (with respect to a more modest 8% effect ascribable to a related EPL reduction), and more than 80% of conversions from temporary ones. The same reform has been studied by Boeri and Garibaldi [2019]: although in a more descriptive framework, they also find a positive correlation between hiring subsidies and total open-ended hires. Neumark and Grijalva [2017] apply a difference-in-differences strategy to US state-level aggregate data: they find a positive effect of tax credits on hires, even if this effect outperforms that on net employment growth. Eventually, Cahuc et al. [2019] exploit a French reform that in 2009 introduced tax credits for hires in small firms within a structural search model, and suggest that this policy had net positive employment effects without affecting wages.[2]

Even less is known about the combined effect of HI and EPL policies, although, as put forward above, this is exactly the mix that has distinguished labour market policies in the EU during the crisis years. Only Boeri and Garibaldi [2019] – by adapting a job search model originally proposed by Garibaldi and Violante [2005] – have studied this issue explicitly. Their theoretical results point to a positive (null) effect on hirings (separations) in firms receiving HI but not affected by EPL reductions; instead, where EPL reduction comes combined with hiring incentives, the effect is positive on both hires and separations. However, the empirical evidence supporting these theoretical results is not casted within a clear causal framework. Providing counterfactual evidence on the interaction effects of EPL reduction and HI is exactly where our paper mainly differs from the mentioned literature, besides studying their distributional implications.

By taking advantage of the recent Italian "Jobs Act" – that reduced EPL in large firms only – as combined with the budget laws for 2015 and 2016 – that introduced generous HI for a subset of workers only – we identify five different policy regimes, differing by EPL and HI intensity, and of which we can assess the impact using very detailed high-frequency administrative data within a non-linear difference-in-differences approach. Our results suggest that small firms – those below the threshold for the application of the reduced firing costs – reacted rather sharply to the hiring incentives, in particular by substituting temporary employment with (less costly than before) open-ended ones. Larger firms, instead, appeared less responsive to the hiring subsidies, and only when combined with lower firing costs. Instead of substituting temporary with permanent employment, they probably also increased the use of fixed-term contracts and of promotions to OEC, a hint that larger firms did not give up on long probationary periods (Berton et al. 2011). The reforms have benefitted domestic workers over foreigners, and those with a lower or more general human capital. No gender effects emerge.

Our paper compares to the most recent related research as it exploits detailed microdata on single-country quasi-experimental conditions, rather than cross-country macro-comparisons, as suggested in Bentolila et al. [2019]. Moreover, we argue that we improve on the existing knowledge with respect to a number of other relevant dimensions. First, as our statistical unit is the worker – and not the firm, like e.g. in

[2] Earlier works include Cipollone and Guelfi [2003], Martini and Trivellato [2011] and Neumark [2013].

Boeri and Garibaldi [2019], Cahuc et al. [2019] and Sestito and Viviano [2018], or even more aggregate units as in Neumark and Grijalva [2017] – we are able to study the distributional effects of the reforms – a gap in the literature we also mentioned above – in particular with respect to workers with different human capital attainment. This links our paper to the empirical literature on the allocative properties of labour market institutions, and on its implications upon productivity [Berton et al. 2017; Bjuggren 2018]. Second, thanks to the granularity of our data, we are able to virtually eliminate the measurement errors that usually affect the studies that exploit firm-size EPL discontinuities to identify treated and control units.[3] In this sense, Italy has historically provided the case for a relatively abundant literature [Boeri and Jimeno 2005; Garibaldi et al. 2004; Kugler and Pica 2008; most recently, Boeri and Garibaldi 2019; Sestito and Viviano 2018]. Third, our duration approach allows to control for how the target and the control populations evolve over time, and therefore to extend the analysis beyond the short run considered, for instance, in Sestito and Viviano [2018]. Fourth, compared to other national case-studies [Cahuc et al. 2019], the variability over time of the generosity of the Italian HI as well as their very high take-up rate [Inps 2019] respectively (i) increase the room for identification and (ii) reduce self-selection bias, on top of bringing intention-to-treat (ITT) estimates nearly identical to an average treatment effect on the treated (ATT).

The paper proceeds as follows. Next section details the institutional background and the features of the two mentioned reforms. Then, in section three and four, we describe the data and the sampling strategy we use for the analysis, and present some descriptive evidence. Section five describes our duration difference-in-differences approach, while related results – separately for unemployed and temporary workers – are presented in section six, which also includes the robustness checks. We then draw some concluding remarks in section seven. Appendixes, eventually, include short-term analyses, a wider institutional framework, details about how we computed firm-size thresholds and a tentative strategy to single out the effect of EPL reduction on individual workers.

2. Institutional background

Our pre-reform background is defined by Law 92/2012 (the "Fornero reform", after the name of the Labour Ministry under the Monti government) that – after two decades of marginal reforms – eventually succeeded in reducing employment protection for workers with open-ended contracts in firms with more than fifteen employees.[4] In cases of layoffs motivated under a disciplinary reason that a labour court rules illegitimate, reinstatement is possible only if the judge deems that the supposed reason for firing simply did not exist, or that the applicable collective agreement identifies an alternative punishment, e.g. an unpaid leave period. Moreover, the dismissed worker is entitled of a compensation ranging from five to twelve monthly

[3] See Appendix A for details.
[4] See Appendix A for details about the definition by law of firms below/above the (fifteen employees) threshold for the applicability of lower/stricter EPL See Appendix B for a description of EPL in Italy before 2012.

salaries, on top of all foregone social security contributions. Instead, in the other cases in which a disciplinary layoff is judged illegitimate – i.e. when the court cannot conclude that the supposed reason was inexistent or the applicable collective agreement is silent about the proper punishment – the dismissed workers are only entitled to a monetary compensation ranging from twelve to twenty-four monthly salaries. For layoffs motivated by an economic reason, instead, reinstatement is possible only if no reason actually existed; in those cases, laid-off workers are also entitled of a monetary compensation ranging from five to twelve monthly salaries. In all the remaining situations of unlawful economic dismissals – i.e. when courts cannot rule that the economic reason put forward by the employer was inexistent – workers are only entitled to a monetary compensation ranging from twelve to twenty-four monthly salaries. No severance pay – to be understood as firing cost to be paid to the dismissed worker in case of a lawful dismissal – has been introduced.[5]

Although it markedly reduced firing costs for firms employing more than fifteen workers, Law 92/12 was still deemed to leave discretionary power to labour courts, hence failing to solve one of the (supposed) major limitations of the Italian labour law, namely its high degree of uncertainty [Cavaletto and Pacelli 2014]. Coupled with widespread delays in civil trials, it used to result into high expected costs of labour disputes for both workers and employers [Sestito and Viviano 2018].

The Decree 183/14 – a part of a wider labour market reform better known as "Jobs Act", implemented under the Renzi government – intended to remove this residual degree of uncertainty in firing costs. All new hires in firms with more than fifteen employees dating from March 7th 2015 would be subject to more flexible firing rules. More precisely, Decree 183/14 established as a general rule that unfair individual layoffs are subject to a monetary compensation only, amounting to two months of pay for every year of seniority, up to a maximum of 24 months of salaries.[6] Reinstatement during our treatment period is limited to discriminatory dismissals and a few cases of disciplinary layoffs.[7] The temporal (before and after March 7th 2015) and sectional (below or above the 15-employee threshold) discontinuities are exactly what we take advantage of to apply a difference-in-differences (DiD) identification strategy.

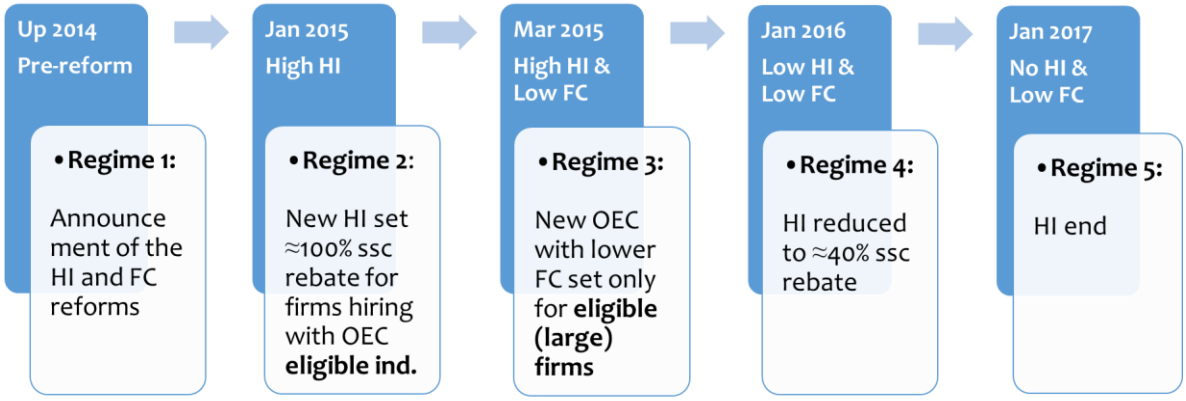
Furthermore, the Financial Stability Law for 2015 (no. 190/2014, art. 118) introduced a large hiring incentive benefitting all new open-ended contracts dating from January 1st 2015, under two conditions: i) the benefitted workers had not held any open-ended contracts in the six months before the incentivized hire, and ii) they were not apprentices in the same firm (for whom another incentive scheme was already in place). The HI was a 3-year 100% exemption from social security contributions up to a threshold of €8,060 per year, corresponding to full rebate of contributions for around 80% of firms [Sestito and Viviano 2018], and was initially planned to be applicable to all open-ended hires starting from January 1st 2015. The Financial Stability Law for 2016 (no. 208/2015), however, reduced the subsidy to a 40% rebate of social security contributions –

[5] We are here assuming that the dismissed worker is appealing against his or her layoff. Most disputes are instead solved under extrajudicial agreements. In these cases, the "law in the books" acts nonetheless as a standard for the "law in action".
[6] On 26 September 2018 a High Court ruling deemed this provision, based on seniority only, not coherent with the Italian Constitution, so returning to the labour courts the task of setting the right compensation for unfair dismissal (within the bounds set by the law, i.e. 6-36 months as of DL 87/2018).
[7] Still related to cases in which the supposed reason for firing simply did not exist.

with a cap of €3,250 per year – for two years only, for all new open-ended contracts activated during the year 2016. The Financial Stability Law for 2017 did not include this kind of HI any more, which were substituted by a set of more fragmented provisions.[8] In the case of the HI, the different temporal (before and after January 1st 2015 and 2016) and sectional (eligible and not eligible workers) discontinuities are what allow us to identify its impact.

Figure 1 helps visualizing the timeline of the policies under scrutiny within a five-period/regime schedule. Regime 1 (January 2012 – December 2014) is the pre-reform window; in Regime 2 (January 2015 – March 7th, 2015) only the more generous HI scheme is in place; in Regime 3 (March 7th, 2015 – December 2015) this is coupled with the reduced firing cost (thereafter FC) in large firms; in Regime 4 (January 2016 – December 2016) the HI becomes less generous, but the FC is still in place; in Regime 5 (from January 2017) only the FC reduction is in place. Reforms were announced a few months in advance. In section 6.4 we analyse the eventual anticipation effects, both as possible delays in hirings while waiting for the HI to be in place, and as a selection of individuals hired with a pre-reform OEC when firing costs were going to be (but not yet) reduced.

Figure 1 Timeline of the policies under scrutiny



3. Data and Sample selection

The analysis is carried out using an administrative archive called Comunicazioni Obbligatorie (CO). It is the administrative register of the entire population of daily labour market flows, managed locally by the Italian regions, under the coordination of the Ministry of Labour. The database includes the population of job contracts that have been opened, terminated or transformed by companies that were active since January 2008. The CO is a linked employee-employer panel, hence for each job spell it records information relative to both workers and their firm. It includes workers' demographics (age, gender, nationality, education and domicile),

[8] Namely, from January 2017 onward, the HI remained available only for the youth below 35 years old, for women and for some categories of marginal unemployed workers.

contract type, occupation (5-digit ISCO code), sector of activity (6-digit NACE code), starting and end dates as well as reason for contract termination. The contract types are: (i) open-ended, (ii) standard direct-hire fixed-term dependent contracts, (iii) agency workers, (iv) apprentices, (v) consultants (so-called *parasubordinati*), (vi) interns (so-called *tirocinanti*), (vii) on-call jobs and (viii) domestic work, as recoded by Veneto Lavoro [2016].

We have access to the CO collected in Piedmont, a large region in the North of Italy. It covers about 7% of inhabitants, of GDP and of workforce within Italy (ISTAT 2017). Its economy is in transition from a strong manufacturing vocation, made of private firms larger than the national average, to a service economy not fully developed yet. For this reason, it felt strongly the consequences of the economic crisis. Since 2015 total employment has been growing again, as well as disposable income.

From the Piedmont CO data, we draw two 1:6 flow samples, each tailored to specific estimation purposes.[9] Our period of analysis starts two years prior the HI reform, on January 2013 (i.e. six months after Law 92/2012 was implemented) and it ends at the current censoring date (September 2017). The first sample includes the unemployment spells starting between January 2013 and December 2016, and we follow them until termination or until the right-censoring date. For the second sample, in the same time window we select the standard direct-hire fixed-term dependent-contract job spells (hereafter temporary contracts, TC, for the sake of simplicity, while we labelled "flexible contracts" any type of non-permanent contract, to avoid misunderstandings) excluding those (i) in the public and agricultural sectors and in domestic jobs, as the reforms do not apply to them; (ii) lasting one-day only, to decrease the noise due to extremely short events; (iii) entirely contained within other employment spells, assumed to be secondary jobs.[10]

Following the literature using administrative data, we define a job-to-job transition any job change occurring within thirty days. Consistently, not-work spells lasting less than thirty days are not considered unemployment and are not included in the unemployment sample.[11] This strategy also allows us to exclude most of frictional unemployment from the analysis, as it is not targeted by the two policies under study. In addition, we use multi-spell samples: the same individual can contribute to both samples (about 32% of persons) and can also contribute to the same sample with more than one spell (Table 1 below). Eventually, we highlight that no estimation is possible beyond month six of any spell (either of temporary job or unemployment) as after six months everyone becomes eligible to the hiring subsidy; spells are hence observed up to 180 days. The econometric approach takes care of this features.

We complement CO data with firm size – as measured according to the fifteen-employee rules – computed applying the perpetual inventory method to ASIA data (*Archivio Statistico delle Imprese Attive*, maintained by the National Statistical Office using social security records) as combined with observed workers' flows. In each month we are able to define whether a firm is either below the 15-employee threshold

[9] We sample individuals randomly, based on their birth date. We include only those in the working age (15-70) bracket.
[10] Flexible contracts are anyway viable outcomes for the unemployment spells.
[11] As a consequence, in what follows unemployment spells lasting one month refer to 30-60 calendar days.

(implying a lower EPL level and not treated by the EPL reform under scrutiny) or above (see Appendix A for details). This strategy minimises the risks of misclassification, however we build up a buffer around the threshold and define "large" all firms with more than 17 employees and "small" those with less than 13 employees in order to separate firms that were reluctant to grow before the FC reform and might decide to cross the threshold afterward (Garibaldi et al. 2004).

4. Descriptive evidence

The samples used for the analysis are described in Table 1. That of temporary contract (TC) jobs includes 108,708 spells – evenly distributed across the four years we consider – held by 61,484 individuals of which 43% are females, 21% not-native and 36% are aged less than 30 in 2015. For about 40% (24%) of them we observe only one (two) episode; 20% of spells ends in one month, 31% in 2-4 months, 48% of in five or more months. As for the unemployed (U), we observe 207,277 spells – again, evenly distributed across the four years we consider – involving 122,631 individuals (48% are females, 28% not-native and 32% are under 30 years of age). For 39% (24%) of them we observe one (two) unemployment spell. Among them, 12% ends in one month, 23% of them in 2-4 months, and 65% of them in at least 5 months.

As emerges comparing our samples to the whole working population [Istat 2018], the proportion of women, youngsters and migrants is higher in our data than in the general population. This is due to the CO structure, which records labour market flows instead of stocks, thus recording more than proportionally those workers facing more fragmented careers, as they are more likely to generate a flow. However, since CO data actually track all labour market flows, our sampling procedure does not introduce any bias in the data we use for estimations.

Table 1 Descriptive statistics

	TC-spells	U-spells
# persons	61,484	122,631
# spells	108,708	207,277
# person-months	384,605	806,797
# spells / person	1.77	1.65
% persons with 1 spell	64.17	38.72
% persons with 2 spell	20.14	24.32
Spell duration 1 month	20.28	12.39
Spell duration 2-4 months	31.58	22.96
Spell duration >=5 months	48.13	64.66
% female	0.43	0.48
% under 30	0.36	0.32
% not-native	0.21	0.28

Source: *own computations on CO-ASIA data*

Temporary contract and unemployment spells can terminate in a set of competing outcomes, described in Table 2. Termination of an unemployment spell can be toward an OEC either in small or large firms, i.e. below the FC threshold (5.1%) or above it (1.8%); toward a flexible contract of any kind (62.3%), or be

censored (i.e. persisting in unemployment: 30.8%). Termination of a TC can be toward unemployment (38.9%) or a flexible contract (17.9%); toward an OEC either in a firm below the FC threshold (3.6%) or in a larger one (2.6%). In the last two cases, we can observe either a job-to-job change – a transition across different firms – or a contract transformation within the same firm. Most transitions (78% in large firms, 66% in small ones) from a TC toward an OEC are within-firm contract transformations.[12] We are not able to bring this feature to our econometric model, as job-to-job movements are too rare and a competing-risk model with seven very unbalanced outcomes turns out to be too demanding. However, descriptive evidence will be relevant to interpret the estimation results.

Table 2 Termination of temporary (TC) and unemployment (U) spells

	TC-spells	U-spells
Unemployment	38.9%	-
Flexible contract	17.9%	62.3%
OEC large firm	2.6%	1.8%
Of which transf.	*77.95%*	-
Of which j-t-j	*22.05%*	-
OEC small firm	3.6%	5.1%
Of which transf.	*65.64%*	-
Of which j-t-j	*34.36%*	-
Censored	37%	30.8%
Total (no.)	108,708	207,277

Source: *own computations on CO-ASIA data*

Figure 2 displays the number of eligible and non-eligible spells terminating in an OEC, separately for TC-spells and U-spells. Among TC work spells, the number of observed new open-ended contracts increased substantially during 2015 among eligible individuals, with the sharpest increase detectable in December 2015 and 2016, i.e. the month before HI were reduced in generosity (in 2016) or eliminated altogether (in 2017), suggesting an anticipation in hiring and transformations to permanent contracts while more convenient regimes were still in place. This is consistent with the national trends in hiring and transformations described by Boeri and Garibaldi [2019] by means of social security data, hence providing our results with sound external validity despite the regional scope of our data.[13] Among the eligible unemployed, we also detect an increase in the number of U-spells which terminate in an OEC both in 2015 and, to a lesser extent, in the 2016. Two peaks appear also during the pre-reform period, namely in September 2013 and September 2014. This might be due to the widespread practice – anecdotal evidence – to hire workers under TC until the early summer, and to rehire (or promote to an OEC) them later in September, in order to save on paid holidays. These workers appear as unemployed in our sample, and are more likely to be eligible than not-eligible as they are usually longer-

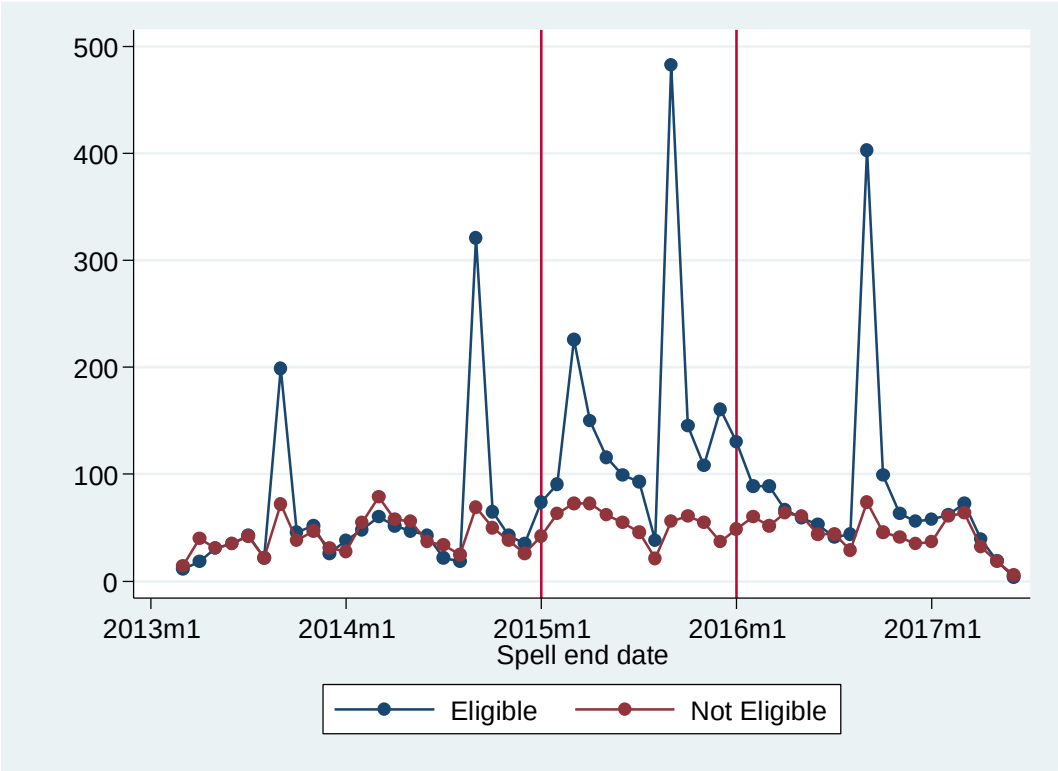
[12] The proportion of contract conversion results to be predominant since we are conditioning on individuals holding a TC (TC-spell sample). The share of persons getting an OEC through contract conversion computed on the full sample (TC + U spells samples) is 34.9%, much closer to the figure computed on national data (about 26%, INPS 2019), that includes nationwide variability.

[13] In the same spirit, in Appendix C we apply to our data the same short-run models that Sestito and Viviano [2018] estimate on data from another large Italian region, i.e. Veneto. Results are comparable, and differences can be rationalised in terms of different regional economic models. This goes again in the direction of supporting the external validity of our results.

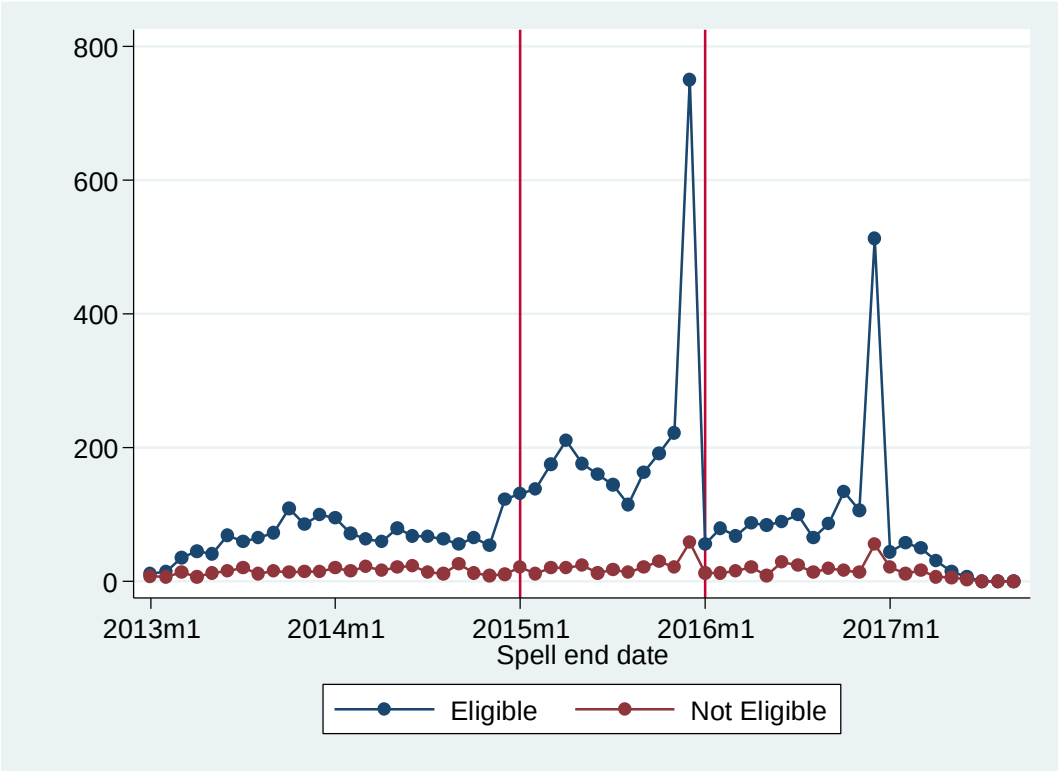
term TC holders. The possibility that a difference between eligible and not-eligible workers already existed during the pre-reform period will be formally tested later on in the paper (section 6.4).

Figure 2 Number of U- and TC-spells ending in an OEC by date and eligibility status.

Panel A: *Unemployment spells*



Panel B: *Temporary contract spells*



Source: *own computations on CO-ASIA data.*

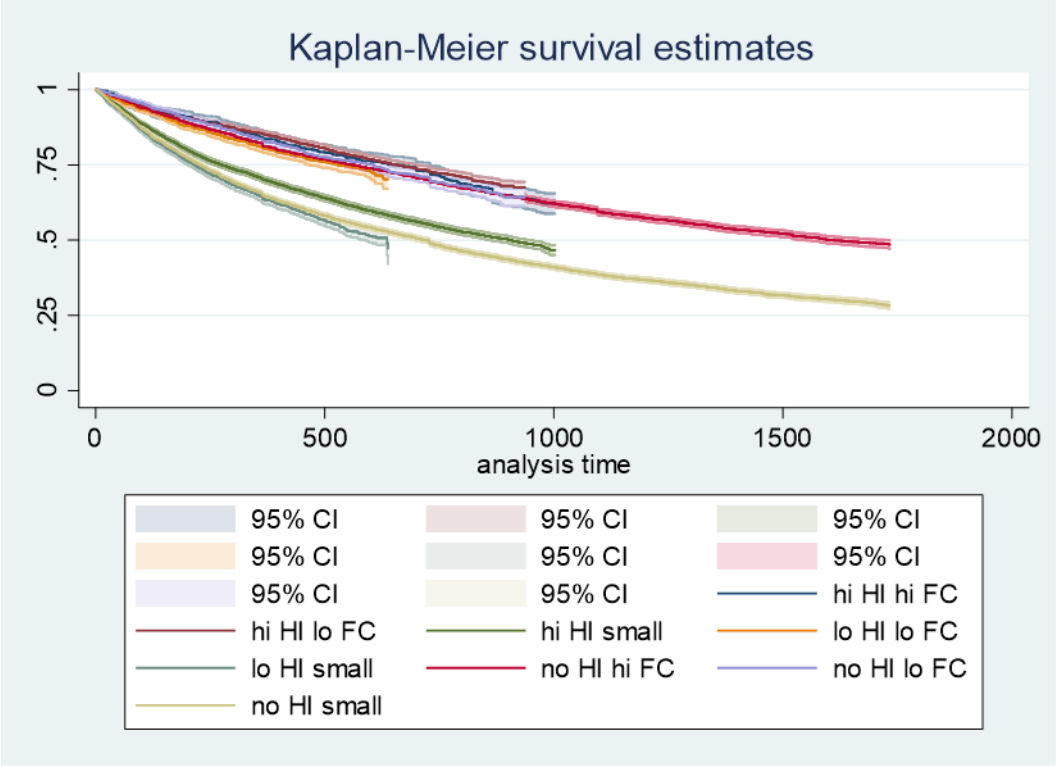
4.1 Duration of OEC

One important aspect that might be influenced by the reduction in FC is the length of the open-ended contracts which, despite their name, might have reduced substantially their duration as a result of lower firing costs faced by the firms, as suggested by the theory discussed above [Bentolila and Bertola 1990; Bertola 1990]. On the other hand, a higher worker turnover could increase the quality of the match [Berton et al., 2017; Rogerson, 1987] and hence increase tenure. Furthermore, the lower labour cost due to the HI might have induced firms to invest less in screening, possibly resulting in a lower match quality and more fragile employment relationships. Unfortunately, we cannot be conclusive on this issue, as HI persisted until December 2018, i.e. beyond our observation period. We can however run some descriptive exercises. In order to understand whether the two policies influenced the duration of the new open-ended contracts we estimate the survival profile of all new open-ended contracts activated between January 2013 and December 2016, both as new hires and as transformations of temporary contracts within the same firm.

The interaction of HI and FC regimes produces several different kinds of OEC. We are able to distinguish among eight different treatment levels: high HI (2015), low HI (2016), no HI (other periods or not-eligible); high FC (large firms before March 2015), low FC (large firms after March 2015), small firms (not affected by FC reform). The survival estimates for each of these eight combinations are displayed in Figure 3. After three years less than 75% of OEC are still in place in large firms, and less than 50% in small firms (lower curves). This shows a quite low persistence in OEC. Furthermore, Figure 3 suggests the existence of two main patterns in contract duration. While large firms do not seem to react much either to HI generosity or to FC reduction, small firms react to HI generosity: high HI significantly increase OEC duration with respect to low HI, while the effects of low HI are not significantly different from no HI.

A comparison with the most updated evidence by the National Social Security Office [INPS 2019], allows to detect some aggregate trends beyond our observational period. First, with respect to 2014, survival probabilities of new OEC activated among eligible individuals did marginally improve in 2015 (when HI were highly generous) and decreased in 2016 (when HI generosity was reduced) but still were higher than in the 2014. Second, after three years, less than half of the new OEC subsidised by the HI were still on-going. Third, firing rates follow the usual pattern, i.e. after the first adjustment phase tend to decrease with seniority; sudden peaks in firings appear at the 37th month of the OEC, i.e. just after the last month of the subsidy. The aggregate evidence hence mirrors again our micro- and single-region-based one. Moreover, it suggests that a portion of new OEC were terminated once the HI were over.

Figure 3 KM Survival estimate of open-ended contract duration by contract type



***Source**: own computations on CO-ASIA data.*

4.2 Quality of the match

Following the same descriptive approach, we can define "quality of the match" the correlation between job quality and individual education. Hence, we compare the job quality indicators detailed in Box 1 and the education level of workers by the eight types of OEC listed in the previous section. Focusing on the physical content of the job (Table 3) the usual gradient by education emerges, with less educated workers holding jobs with higher physical content. However, among high-educated workers in small firms, we see that HI are associated to an increase in the median physical content of jobs. This happens also in large firms, with either EPL regime. Table 4 shifts the focus upon the intellectual content of a job. We find again the usual gradient by education. However, we see that in small firms, as well as in large firms when reduced FC become low, HI are associated to a decrease in the median intellectual content of jobs held by graduated workers. If anything, this evidence suggests a decrease in the quality of the match, although possibly not statistically significant. We now move to causal analysis.

Box 1 Eurofound human capital variables

In our analysis we include several Human Capital variables derived from a set of indicators on the task content, methods and tools used at work, developed by Eurofound [2016]. This set of indices have been constructed at the level of the job, defined as the detailed combination of occupations (ISCO 08) and sectors (NACE Rev 2.0) at two-digit level. Information about the tasks content and methods involved by jobs are obtained by combining data from three international surveys on skills, tasks, competences and working conditions, in particular: the Programme for the International Assessment of Adult Competencies (PIAAC), the European Working Conditions Survey (EWCS) and the US Occupational Information Network dataset (ONET). An outline of the framework developed by Eurofound to organize the tasks indices is the following:

1 Content of tasks:
- 1.1 Physical
- 1.2 Intellectual
- 1.3 Social

2 Methods & Tools used for carrying out the task:
- 2.1 Methods: autonomy
- 2.2 Methods: team work
- 2.3 Methods: Routine
- 2.4 Tools: machine (excluding ICT)
- 2.5 Tools: ICTs

Tasks are classified across two main axes: one refers to the content of tasks (the *what* of the activity), the other refers to the methods and tools used for carrying out the task (the *how* of the activity). Both axes encompass a number of categories of tasks & methods, further divided in subcategories (not shown). At each level in this classification, the task/method category is converted into a *job level index*, measuring the extent to which a job involves that particular task or method. The values of each index can range from zero to one, reflecting the intensity with which each job involves carrying out work in each of the task/method categories.

In the econometric analysis we have chosen to use the most pervasive and widespread task indexes from each domain, both in the main specification and in the heterogeneity analysis. In particular, we have constructed dichotomized variables to characterize jobs involving high (higher than the median) intensity of: intellectual tasks (tasks aimed at manipulation, transformation and resolution of complex information and problems); social tasks (tasks that involve a direct personal contact with the public, such as selling, teaching or attending patients); autonomy (which refers to the degree of latitude of workers in their tasks;) and ICT tools (basic office and programming).

Table 3 Median value of physical content of OEC job for low/medium/high educated workers

Policy Regime	Level of education		
	Low	*Medium*	*High*
Large firms			
No HI high FC	0.353	0.308	0.174
High HI high FC	0.354	0.319	0.182
High HI low FC	0.353	0.299	0.191
Low HI low FC	0.353	0.314	0.218
No HI low FC	0.353	0.272	0.161
Small firms			
High HI	0.362	0.319	0.229
Low HI	0.362	0.317	0.245
No HI	0.362	0.314	0.211

***Source**: own computations on CO-ASIA data.*

Table 4 Median value of intellectual content of OEC job for low/medium/high educated workers

Policy Regime	Level of education		
	Low	*Medium*	*High*
Large firms			
No HI high FC	0.384	0.471	0.554
High HI high FC	0.384	0.424	0.562
High HI low FC	0.393	0.469	0.533
Low HI low FC	0.384	0.450	0.541
No HI low FC	0.384	0.492	0.562
Small firms			
High HI	0.392	0.450	0.506
Low HI	0.384	0.450	0.497
No HI	0.384	0.454	0.516

***Source**: own computations on CO-ASIA data.*

5. The NL-DiD model and the empirical specifications

5.1 The general setting

As Sestito and Viviano [2018] explain – and emerges from the institutional background described above – the not-overlapping time-schedules and target populations of the HI and FC reforms under scrutiny, allow in principle to separately identify the effects of the two policies within a double diff-in-diffs approach. Nonetheless, we deem this exercise of questionable reliability, for three reasons. First, because the time-frame in which the HI is in place while the reduced FC is not, is limited to the first two months of 2015, when EPL reduction had already been announced. Hires under open-ended contracts treated with the generous HI but not

yet with the reduced FC when the FC reduction was early to come, are likely to differ from the untreated hires of our regime 2 for reasons unobserved to the researcher, e.g. because they are of high value to the hiring firms. This selection could bias the results. Second, because small firms could be a bad control for the EPL change if they react differently from larger ones to the HI; as our analysis suggests, they actually do. Third, because the FC changed across firms of different size, being therefore – with respect to the individual, our statistical unit – endogenous to their job-search process. This third reason might be considered minor, as it assumes that individuals face the choice of accepting a job offer in a large or in a small firm. In high unemployment periods, as 2015 still was, this is unlikely, although possible. For all these reasons we prefer, in our analysis, not to estimate the FC and HI effects separately.[14] Instead – what represents one of the novel contributions of our study – we consider individuals as exposed to different *policy regimes*, that represent different mixes of HI and EPL (Figure 1). More specifically, and against a benchmark of no reforms (Regime 1) we aim to assess the impact of the following situations:

- High hiring subsidies (Regime 2)
- High hiring subsidies and low firing costs (Regime 3)
- Low hiring subsidies and low firing costs (Regime 4)
- No hiring subsidies and low firing costs (Regime 5)

As anticipated in the data section, to evaluate the impact of each policy regime on the individual probability of obtaining an open-ended job, we separate the analysis of unemployed individuals eventually obtaining a job, and the analysis of temporary contract holders eventually moving out of that contract. They are both at risk of getting an open-ended job, but face different current conditions (i.e. past vs. present job characteristics) and different competing risks. Of course, the same individual often experiences both situations, churning between unemployment and several forms of flexible work. For this reason, we include detailed measures of the individual working career in the previous three years, to account for persistence in career patterns and insure common trend.[15] This is detailed in section 5.4 below.

5.2 The duration model

To study the impact of the four policy regimes described above in the long run, we need to fit a DiD approach into a competing-risk duration model. This for two reasons. First, because endogenous selection of HI-eligible individuals into permanent employment would worsen the quality of the eligible individuals' pool over time, so biasing downward the effect of the reforms as time into temporary contracts or unemployment

[14] A tentative assessment of the impact of the EPL reduction alone under the hypothesis that employment dynamics is dominated by labour demand (i.e. that workers cannot really choose among alternative employment outcomes) is proposed in Appendix D. The spirit of this exercise is purely explorative, and follows – although we insist on that the effect of the HI cannot be singled out exploiting Regime 2 – from the willingness to parallel the possibility to learn something about the HI alone by comparing Regimes 3, 4 and 5, i.e. when FC is constant and HI decreases.

[15] I.e. persistence due to individual preferences (e.g. attitude to move), scarring (e.g. bad reputation due to temporary contracts in the past) or other reasons generating trapping in a particular career pattern. See Berton et al. [2011] for a discussion.

goes by. Second, because we need multiple exits, so that a linear probability model would not be suitable. We hence estimate non-linear DiD (NL-DiD) models of the following type:

$$(1) \qquad Pr\{Y_{it} = k\} = \Lambda(\tau_t \alpha_1 + \sum_{r=2}^{5} R_{t,r} \alpha_r + E_t \alpha_6 + \sum_{r=2}^{5} R_{t,r} E_t \alpha_{r+5} + X_t \alpha_{11})$$

where subscripts *i* and *t* represent respectively individuals and the time elapsed since the duration process under scrutiny started (either U or TC).

For unemployment spells, *k* includes exits (i) to any flexible job (irrespective of firm size and contract type), (ii) to an open-ended job in a small firm, or (iii) to an open-ended job in a large firm. For temporary employment spells the set of possible outcomes is larger and includes (i) unemployment, (ii) a flexible job contract, (iii) an open-ended contract in a small firm or (iv) in a large firm.[16] For both samples censoring is of course also considered. As anticipated, we pool job-to-job transitions and within-firm contract transformations in the same outcome (iii) or (iv), as the scant number of job-to-job transitions prevents us to identify separately their pattern (Table 2). It follows that results will be mostly driven by within-firm contract transformations.

The equation includes a set of individual time dummies τ_t, dummies for the post-reform policy regimes (R_t), individual eligibility dummies (E_t), their interactions ($R_{t,r}E_t$) and a rich set of individual and past career characteristics (X_t). All the included controls are presented in more details in the next section.

Although our data would allow a continuous-time setting, we will treat time as intrinsically discrete, with transitions occurring at monthly intervals. The computational gain is substantial, as the likelihood function of our models boils down to that of a standard multinomial logit model [Jenkins 2005]; i.e. $\Lambda(.)$ is the logistic cumulative distribution function evaluated at (.). This simplifies the analysis, under several respects. First, it allows a richer specification in terms of interactions and controls, and also in terms of the number of possible outcomes, that is important for the aim of our exercise. Second, this allows a fully not-parametric specification of the duration process. This is crucial as the effects of the residual unobserved heterogeneity – possibly still present after controlling for X_t – are mitigated by the use of a fully flexible specification of the baseline hazard [Dolton and Van Der Klaauw 1999]. The price to pay is to assume that the baseline hazard is (piecewise) constant within each monthly interval, and that outcomes within each month are mutually exclusive.

We hence rearrange data in the person-month structure, and model the dependent variable Y_{it} as a polytomous variable taking the value of *k* if in month *t* the process under scrutiny is in the *k*-th status; $k = 0$ as long as the process keeps going, while the set of remaining possible values depends on the specific process we are analysing, as above listed. Whenever $k \neq 0$ the time process ends, except for right-censored records that, by definition, are terminated although the process is still going on.

[16] As discussed in the data section, we exclude firms around the EPL legal threshold of 15 employees. The tiny number of exit from U- and TC-spells toward these firms is treated as a separate competing risk (not shown in the results tables).

5.3 Estimating the effect of the policy regimes

Measuring the effect of being in the *r-th* policy regime – where $r = \{1, \dots, 5\}$ and $r = 1$ is the benchmark case, i.e. that of no policy change – on the individual probability to obtain an open-ended contract within a not-linear setting, is not as straightforward as in the linear case. As mentioned, $R_{t,r}$ is a set of dummies taking the value of one if individual *i* is in the *r-th* policy regime in month *t* of the process. Analogously, E_t is a set of dummies taking the value of one if individual i is eligible to the hiring incentive in month t. However, differently from linear models, the interaction parameters $\alpha_{\{7 \dots 10\}}$ do not represent the average treatment effect. In fact, a non-linear model implies more than one point of attention in the incorporation of a DiD structure, as the effect is measured by an interaction term that is neither constant over time and treatment status, nor zero when the impact is null. Moreover, as Karaca-Mandic et al. [2012] clearly put forward, $\alpha_{\{7 \dots 10\}}$ are not informative either of the sign or of the significance of the effect. Instead, we have to compute it as follows:

$$(2) \qquad E\left[Y_{it} = k \middle| R_{t,r} = 1, E_t = 1, R_{t,r}E_t = 1, X\right] - E\left[Y_{it} = k \middle| R_{t,r} = 1, E_t = 1, R_{t,r}E_t = 0, X\right]$$

Equation (2) holds if we have an invertible function, as the logistic one, and if the common trend hypothesis holds, as it is always necessary when applying DiD [Blundell and Costa-Dias 2009; Karaca-Mandic et al. 2012; Puhani 2012].

5.4 Control variables

The set of controls X_t includes socio-demographic characteristics: (log of) age, gender, nationality; the size of the municipality (proxy for the size of the local labour market), three education levels, detailed human capital measures: index of intellectual, social, autonomy and ICT task content (see Box 1). Having included HC indexes based on industry and occupation interactions, we do not control for these two dimensions further.

To control for individual churning between unemployment and several forms of precarious work, we include detailed measures of the individual working career in the previous three years, as a way to account for persistence in career patterns: number of past employment spells, number of past unemployment spells. In the unemployment spells sample only, we control also for past job characteristics: (log of) days of actual work experience, (log of) duration of the last employment spell, kind of contract and reason for termination (firing, quit, other).

6. Estimation results

6.1 Main results - Unemployed

Table 5 reports our main results for the unemployment spells. The table displays three panels – one for each destination of our competing-risks model – and namely exits to OEC in large firms, in small firms or to any type of temporary or flexible contract. Each panel has five lines, one for each month of elapsed duration since the beginning of the unemployment spell.[17] Then, the whole table is crossed by six columns: the first one displays the elapsed duration, and the second one the benchmark monthly transition probabilities during the pre-reform years, i.e. the numbers against which we need to compare our estimates. Our interest will focus on columns 3-6, i.e. those displaying the marginal effects of the policy regimes 2-5, and the relative percentage change with respect to the pre-reform period (regime 1). We should not be surprised by large percentage changes, as monthly flows are very small so that they easily double thanks to regime changes.[18]

Results tell the following story. The introduction of the very generous HI alone (regime 2) increased the probability of unemployed workers to be hired under an OEC in small firms, although the effect decreases with unemployment duration. Almost no effect instead emerges for transitions to OEC in large firms, while there is some evidence that transitions from unemployment to flexible contracts decreases, a possible symptom that small firms may have substituted flexible employment with subsidized open-ended employment. It is only when combined with EPL reduction (regime 3) that the generous HI scheme for 2015 also increases the probability that unemployed workers get an OEC also in large firms. The probability of getting an OEC in small firms reinforces – not so much in magnitude, but in being present over all the observed unemployment duration – while that of an exit to flexible employment turns – although in only two months – positive, a possible signal that large firms in particular may still prefer to use flexible employment as a probationary period, a possibility also suggested in Berton et al. [2011] and in Boeri and Garibaldi [2019]. When the HI is reduced in 2016 (regime 4) the impact on the probability to get an OEC roughly halves both in large and small firms, and when it is withdrawn in 2017 (regime 5) almost no effect survives.

All in all, hence, we can say that small firms react to the HI by raising the inflow of workers under OEC, which partly substitute flexible employment. Large firms, instead – and as far as unemployed workers are concerned – react to HI only when they come combined with a lower FC, and probably still use flexible contracts as a probationary period for a (possible) future promotion with OECs.[19]

[17] 31 to 180 days, as spells lasting less than one month are job-to-job transitions, while we remind that no estimation is possible beyond 180 days because everyone becomes eligible to the hiring subsidy and no control group would exist anymore.

[18] Full parameter estimates are available upon request to the authors.

[19] We also estimated a model with separate outcomes: "flexible in a large firm", "flexible in a small firm". Results, available upon request, point to a stronger effect in large firms with respect to smaller ones, coherently with our reading of the phenomena. However, estimates are barely significant, indicating that this strategy is too demanding in terms of data.

Table 5 Effects of policy regimes on unemployed workers

Elapsed time (months)	Regime (1) Benchmark transition prob.	Policy Regimes							
		Regime (2) High HI		Regime (3) High HI + Low FC		Regime (4) Low HI + Low FC		Regime (5) Low FC	
Transition to an OEC in Large Firms									
1	*0.0013*	0.0020	**151%**	0.0010	**74%**				
2	*0.0035*			0.0037	**103%**	0.0026	**73%**		
3	*0.0013*			0.0031	**243%**	0.0017	**132%**		
4	*0.0028*								
5	*0.0018*			0.0025	**139%**				
Transition to an OEC in Small Firms									
1	*0.0023*	0.0049	**213%**	0.0041	**181%**	0.0010	**46%**		
2	*0.0025*			0.0050	**199%**	0.0029	**115%**		
3	*0.0023*			0.0046	**197%**	0.0021	**89%**		
4	*0.0027*	0.0024	**88%**	0.0048	**178%**	0.0016	**57%**	0.0018	**66%**
5	*0.0036*			0.0052	**145%**	0.0020	**55%**		
Transition to a Flexible Contract									
1	*0.0799*	0.0346	**43%**					0.0311	**39%**
2	*0.1047*	-0.0211	**-20%**					-0.0201	**-19%**
3	*0.0915*	-0.0230	**-25%**	0.0147	**16%**				
4	*0.0695*			0.0158	**23%**	0.0127	**18%**		
5	*0.0747*							0.0250	**33%**

Source: *own estimations on CO-ASIA data. Standard errors clustered at the U-spell level. Percentage increase with respect to benchmarks. Only coefficients significantly different from 0 at 5% level are shown.*

6.2 Main results – Temporary contract workers

Table 6 is structured as Table 5, but displays estimates about temporary employment spells. The story is pretty similar to that concerning unemployed workers, inasmuch as small firms react to the HI since regime 2, while large firms are more sensitive to the combination of HI and lower EPL. Almost no sizable effect appears for transitions to unemployment or to a new flexible contract (small or insignificant effects). In other words, the policy regimes under scrutiny may have been responsible of a contract reshuffling from temporary to open-ended contracts, coherently with the fact that most transitions are actually within firm contract transformations (Table 2); on the other hand they have not been responsible of changes in the transition from temporary employment to other non-permanent contracts or unemployment, possibly because – during the time window we observe – the hiring subsidies had not expired yet.

Table 6 Effects of policy regimes on workers employed with temporary contracts

Elapsed time (months)	Regime(1) Benchmark transition prob.	Policy Regimes							
		Regime (2) High HI		Regime (3) High HI + Low FC		Regime (4) Low HI + Low FC		Regime (5) Low FC	
Transition to an OEC in Large Firms									
1	0.0036	0.0033	**94%**	0.0052	**145%**				
2	0.0037			0.0065	**178%**	0.0039	**108%**		
3	0.0029			0.0078	**265%**	0.0037	**128%**	0.0053	**181%**
4	0.0031			0.0065	**207%**	0.0037	**119%**	0.0032	**101%**
5	0.0037			0.0077	**209%**	0.0040	**110%**		
Transition to an OEC in Small Firms									
1	0.0028	0.0093	**334%**	0.0061	**218%**	0.0030	**107%**		
2	0.0037			0.0084	**230%**	0.0046	**124%**	0.0063	**173%**
3	0.0032	0.0064	**198%**	0.0078	**241%**	0.0037	**115%**	0.0062	**191%**
4	0.0052	0.0076	**145%**	0.0081	**154%**	0.0053	**100%**		
5	0.0058			0.0095	**162%**				
Transition to Unemployment									
1	0.1246	-0.0521	**-42%**						
2	0.1095			0.0194	**18%**				
3	0.1270			0.0279	**22%**				
4	0.1252			0.0418	**33%**				
5	0.1425								
Transition to a Flexible Contract									
1	0.0793								
2	0.0589								
3	0.0437								
4	0.0499			0.0189	**38%**				
5	0.0427					0.0143	**33%**		

Source: *own estimations on CO-ASIA data. Standard errors clustered at the spell level. Percentage increase with respect to benchmarks.*

6.3 Heterogeneity effects

One of the gaps in the literature that we aim to fill with our exercise is to highlight whether the policies under scrutiny favoured different individuals in different ways, i.e. whether they had distributional effects. In particular, we want to focus on different demographic groups (gender, age and nationality) and on human capital (HC) measures: tertiary education, quality of the current or previous job (as captured by its high intellectual content: see Box 1), an indicator variable that switches on if average past job-spell duration is above the median (hereafter: high general HC), and an indicator variable that switches on if past actual experience is highly concentrated in one sector (1-digit NACE; hereafter: high specific HC). To estimate heterogeneity effects of the four different policy regimes, we augment equation (1) with elements of the following type:

$$C_t R_{t,r} E_t \alpha_c$$

i.e. we interact the variables of interest with the above-described demographics and dichotomized measures of HC. We estimate α_c as described in Section 5.3.

The most surprising result is that very little heterogeneity emerges. Indeed, we check for many possible channels through which the effect of the different policy regimes might differ, but only a few display significant interaction effects. The results from the heterogeneity analyses are synoptically summarized in Table 7.[20]

Foreign individuals (either TC or U) benefitted less from the reforms with respect to their domestic counterparts, in small firms only: a possible explanation is that the reduction of labour cost has allowed a preference for national workers to emerge in small firms, while before 2015 the lower cost of most foreign work – often employed under borderline legal framework – prevailed. No gender difference instead emerges.

University degree holders are generally not advantaged with respect to unemployed individuals with lower educational attainment, as well as younger TC and TC holding a more intellectual job in a large firm. In other words, all this points to the idea that more skilled individuals are not favoured or even penalised by the reforms under scrutiny.

On the contrary, general human capital matters in a systematic way: among U and TC, in small and large firms, those with a longer average duration of past employment spells enjoy a higher probability to get an OEC. In turn, specific human capital has no effect.[21] Coherently with the above comment, it seems that less skilled individuals are favoured by the reforms, referring to those churning among jobs in a not-systematic way but nonetheless accumulating work experience.

[20] Point estimates are available upon request to the authors.
[21] Controlling for coherence between the sector that provided most of the specific HC and the sector in which the worker finds its OEC turned out to be too computationally demanding.

All in all, the reforms turned out to favour those workers otherwise most at risk to persist in unemployment/temporary jobs, decreasing their labour cost and making it more in line with their productivity.

Table 7 Interaction effects of policy regimes for subgroups of unemployed and temporary workers

Unemployed				**Temporary workers**			
Regime (2) High HI	**Regime (3) High HI + Low FC**	**Regime (4) Low HI + Low FC**	**Regime (5) Low FC**	**Regime (2) High HI**	**Regime (3) High HI + Low FC**	**Regime (4) Low HI + Low FC**	**Regime (5) Low FC**
Transition to an OEC in Large Firms							
	+ General HC	+ General HC - Graduated		- Intellectual	-Young		
Transition to an OEC in Small Firms							
- Foreign	+ General HC - Foreign	+ General HC - Foreign			+ General HC - Foreign	+ General HC - Foreign	

***Source**: own estimations on CO-ASIA data. In the table we show the sign effect only for the interaction terms significant at 5% level for at least two consecutive months (time elapsed since the spell starts).*

6.4 Robustness analysis

Two tests are in order. First, we need to assess the parallel trend assumption in the pre-reform period between HI-eligible and not-eligible; second, we should investigate eventual anticipation effects due to the announcement of the reform three months in advance.

To do so we replicate the same non-linear DiD estimation strategy but focussing on the two years before the actual implementation of the new rules, i.e. 2013, used as baseline year, and 2014, as fake reform year. We depict two "fake regimes" in 2014: the first going from January to September 2014 and the second going from October to December 2014. The first tests the parallel trend assumption; the second, being after the announcement date of the reform, assesses if any anticipation effect was in place.

The results of this exercise – reported in Table 8 – provide neat evidence on the absence of any anticipation effects after the announcement of the reforms, among both temporary workers and unemployed. Regarding the parallel trend hypothesis, we only detect two small positive coefficients associated to being eligible in the pre-reform months among the unemployed. However, the size of the effect is much smaller than the magnitude we observe during the post reform periods, with a 60-70% increase with respect the baseline versus an increase that is two-three times bigger in the periods actually treated. We reckon all this as reassuring regarding the robustness of our estimates.

Table 8 Robustness to placebo and announcement effects: Marginal Effect of being Eligible in pre-reform periods versus Jan-Dec 2013

	Unemployed		Temporary Workers	
Elapsed time (months)	**Period: Jan-Sep 2014**	**Period: Oct-Dec 2014 (announcement)**	**Period: Jan-Sep 2014**	**Period: Oct-Dec 2014 (announcement)**
Transition to an OEC in Large Firms				
1	0.0005 (60%)	n.s	n.s	n.s
2	0.0018 (79%)	n.s	n.s	n.s
3	n.s	n.s	n.s	n.s
4	n.s	n.s	n.s	n.s
5	n.s	n.s	n.s	n.s
Transition to an OEC in Small Firms				
1	n.s	n.s	n.s	n.s
2	n.s	n.s	n.s	n.s
3	n.s	n.s	n.s	n.s
4	n.s	n.s	n.s	n.s
5	n.s	n.s	n.s	n.s

Source: *own estimations on CO-ASIA data. Standard errors clustered at the spell level. Percentage increase with respect to benchmarks.*

7. Conclusions

In 2015, the Italian government launched an important structural reform of labour market, called Jobs Act, which reduced EPL for all new open-ended contracts starting from March 2015 and activated in firms with more than fifteen employees. With the aim of promoting a wider and pervasive use of the new open-ended contracts, the Jobs Act was coupled – since January 2015 – with a very generous hiring subsidy.

The different timing of the two interventions, as well as non-overlapping target populations, generated a sequence of what we called *policy regimes* – meaning different combinations of hiring incentives and of firing costs - the impact of which we assessed. This is quite new in the literature, as the common approach is to focus either on the impact of hiring incentives, or on the effect of reducing EPL, while the combination of the two, although common in actually implemented reforms, is seldom studied. This is one of the distinctive features of our work.

We estimate the causal impact of the different regimes using a difference-in-differences approach within a non-linear framework, in order to allow the estimation of competing-risk duration models. The specific econometric strategy to do so, although known in the econometric literature, is not commonly applied in impact evaluation studies; this is another distinctive feature of our work.

Our results suggest that small firms – those below the threshold for the application of the reduced firing costs – reacted rather sharply to the hiring incentives, in particular by substituting temporary employment

with (less costly than before) open-ended ones. Larger firms, instead, appeared less responsive to the hiring subsidies, and they reacted significantly only when HI were accompanied by lower firing costs. They probably also increased the use of fixed-term contracts, hinting that larger firms did not give up on long probationary periods.

We then focus on the presence of heterogeneity in the effects of the regimes. This is the third specific feature of our work, as literature in this respect is very scant. Quite surprisingly not much heterogeneity emerges. The most clear-cut message refers to high general human capital workers that enjoy a higher probability of obtaining an open-ended contract, while high specific human capital, higher education, higher intellectual content of previous jobs either does not help or penalises workers. Our descriptive evidence provides a coherent hint that the new regimes decreased job quality for graduate workers

All in all, the reforms turned out to favour those workers otherwise most at risk to persist in unemployment/temporary jobs, decreasing their labour cost and making it more in line with their productivity [Bertola 1999, Lazear 1990]. This is coherent with the occupational emergency generated by the prolonged financial crisis and the need to help more fragile workers. However, this is a different choice with respect to supply side kind of policies aimed at increasing productivity through training. It is more socially desirable with respect to a generic/passive income support policy, as it moves individuals into a supposedly more stable employment status. On the other hand, it might be open to discussion the choice to support low productivity jobs in a country in which they have constantly grown over the last decade, while high productivity jobs decrease and average productivity is stagnant.

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Appendix A: Computing firm size and the 15-employee threshold

The article 18 of Law 300/1970 established that the regime with higher degree of EPL applies to all employers who, during the last six months:

a) employ more than 15 employees within the single production unit;
b) employ more than 15 employees within the same municipality where one or more production units are located;
c) employ more than 60 employees in Italy.

Recovering a precise measure of firm and plant size is hence crucial to our purposes. Our CO data allow to measure very precisely, on a daily basis, any kind of worker flow in Piedmont. In turn, stock data, such as employment level at a given point in time, are not available and need to be computed. For this reason, we complement CO data with a firm size measure from an administrative database called ASIA (Archivio Statistico delle Imprese Attive), produced by the Italian National Statistical Office (Istat). ASIA contains all enterprises carrying on economic activities in manufacturing, commerce and services and that were active for at least six months in a calendar year. It provides, separately for each company and its production units, the average number of workers holding a job positions in the year, including management but excluding consultants and agency workers [Istat 2015].

ASIA is however not enough in order to compute the 15/60 employee threshold. Indeed, not all contracts are to be included: apprentices, training contracts, temporary agency workers and workers who are relative up to second grade; part-time workers and on-call jobs contribute *pro rata temporis*.

To solve this measurement puzzle, we computed monthly firm size in three steps:

- First, we start from the reasonable assumption that – over the time window observed through CO data, i.e. from 2009 to 2017 – the only type of contract of which we cannot retrieve the stock starting from flow information, is the open-ended one. Indeed, it is only for very long-lasting contracts that – over a window of nine years – we observe neither the starting nor the closing dates.
- Second, we compute the stock of open-ended contracts in each firm / plant at the end of 2011 (our sample frame starts in January 2012) by subtracting from the employment stocks observed in ASIA in 2011, the stocks of temporary contracts observed in CO that are common to the reference population of ASIA.[22]
- Third, we use the perpetual inventory method to update – month by month – this initial stock with the flows – from CO data – that are relevant to compute the size as defined by article 18 of the labour code. More precisely, for each production unit *i*, at the initial time *t*, we are able

[22] For each firm, we also subtract one more unit from its main plant, as a minimum measure of management with independent job positions.

to observe E_{it}, i.e. the initial stock of employees of a production unit relevant for computing the threshold as described in steps (1) and (2) above. Then, we obtain an estimate of the stock in each following month, E_{it+k} using the information on net employment flow N (hiring-termination) in the specific production unit i according to the following equation: $E_{it+k} = E_{it} + \sum_{j=1}^{k} N_{it+j}$. The equation is then applied for every month during the period of analysis (2012-2017).

We deem important to highlight some additional features that make our measure of firm size very precise and prone to minimize measurement errors with respect to the existing literature [e.g., Berton et al. 2018; Garibaldi et al. 2004; Kugler and Pica 2008], since it follows as much as possible the prescription of the article 18 of Law 300/1970:

- Since we have the exact date of activation/termination of each contract, and we observe whether a contract was part time or full time, we can generate N_{it} as an average of the previous six months full time equivalent net flows within the unit of production i.
- Since we observe the exact contracts codes, we can include only the contracts which contribute to reaching the threshold for applicability of the EPL for computing both the initial stock E_{it} and the net flow N_{it}.
- Since we observe flows (CO) and stock (ASIA+CO) at unit of production level, we can precisely apply the rules of the 18 of Law 300/1970 to determine whether a given firm is subject or exempted to the applicability of the stricter EPL on dismissals.

Despite we will apply all the three conditions above mentioned (a-c), for convenience, we will continue to talk about the "15-employees" thresholds, as it common in previous literature.

Appendix B: EPL in Italy before 2012

Starting from a situation of no limitations to individual layoffs, employment protection legislation in Italy was introduced during the sixties with Laws 604/1966 and 300/1970, the latter being better known as the "Workers' Statute". Indeed, under the civil code of 1865, open-ended contracts were explicitly forbidden. Recognizing an existing *de-facto* situation in which many employers aimed at integrating their labourers open-endedly within the production process, the fascist civil code enforced in 1942 introduced the possibility to stipulate open-ended employment contracts; nonetheless, it also granted the complete freedom to dismiss workers without any justification. Law 604/1966 represented a turning point with respect to this principle, inasmuch as it introduced the idea that individual layoffs must be justified in order to prevent abuses from the employers. In particular, an employer is legitimated to dismiss a worker if a *just cause* exists – damage of equipment, fight or violence towards other colleagues – or in case of a *justified reason,* that can be either *subjective* – major breaches of contract obligations – or *objective,* when the organization of the production process would make impossible the continuation of the employment relationship. In case the dismissed employees deem illegitimate the layoff, they have to bring the case to a labour court. Under Law 604/1966, the labour judge has the authority to ascertain whether the dismissal was legitimated by a just cause or a justified reason. In case it was, nothing is due to the worker and the termination is final. Otherwise, the employer is obliged to choose between starting a new employment relationship with the dismissed workers, or to compensate them with a sum ranging from 2.5 to 14 monthly salaries, depending on firm size and workers' seniority.[23] In no case a severance pay is present. Law 300/1970, with its well-known article no. 18, introduced a more generous compensation system for illegitimate dismissals in firms employing more than fifteen workers. According to its provisions, an illegitimate layoff is deprived of any legal effect, leads to reinstating the dismissed worker to the former position, and to the compensation of all foregone salaries and social security contributions since the layoff date. As for Law 604/1966 – which keeps applicable to small firms – no severance pay is present for legitimate dismissals, while layoffs that are deemed discriminatory are null.

Based on the above-described provisions included in Law 300/1970, Italy has usually been presented as an exemplary case of rigid labour markets. This view was far from being unchallenged [Contini and Revelli 1992; Contini and Trivellato 2005; Del Conte et al. 2004], but what is relevant to our purposes is that since the early nineties the institutional setting defined by Laws 604/1966 and 300/1970 started to be considered questionable. Nonetheless, after a decade of partial labour market reforms that led to an almost complete liberalization of the use of temporary contracts [Davidsson 2011], it was only the economic crisis in 2008 that created the necessary political capital and institutional context to proceed to a revision of Law 300/1970. Following a process that Sacchi [2015] has defined *conditionality by other means*, the European Union required Italy to proceed to structural labour market reforms – in particular to revise the employment protection system concerning workers in larger firms – in order to receive financial support in the aftermath of the

[23] This picture indeed already includes the novelties introduced under Law 108/1990; see Kugler and Pica [2008].

sovereign debt crisis. The fourth Berlusconi government made two attempts to fulfil this request through the introduction of contractual derogations to the labour law, but in both cases, it proved unsuccessful. In 2010 it tried to circumvent article no. 18 by allowing employers to stipulate open-ended contracts with a provision to move the settlement of labour disputes (including those about dismissals) from labour courts – which decide according to the law – to arbitration boards, which instead decide according to equity principles. The government eventually withdrew this provision, after the President of the Republic deemed that it might be unconstitutional. In August 2011 a new attempt was made with Law 148/2011. Its article no. 8 provided that plant- and local-level collective agreements had the possibility to derogate to labour law, thereby including the norms concerning individual layoffs. This kind of collective agreements had to be signed by the most representative unions at the national or local level, a condition that could give rise to a massive number of lawsuits from national union representatives, questioning the representativeness of local signers. For this reason, the labour market provisions of Law 148/2011 were considered not sufficient by the European Commission, the European Central Bank and the International Monetary Fund, that withdrew their support to the Berlusconi government and made pressure for the instatement of a new "technical" government. Berlusconi resigned in November 2011, when the former European Commissioner Mario Monti became the new Prime Minister.[24]

[24] For more details about the Italian institutional framework until the Monti government, see Berton et al. [2012].

Appendix C: Short-term analysis

In order to identify the causal effect of the HI and FC reforms in the short run, we adopt a DiD estimation strategy that allows to control for composition, seasonality and individual unobservable effects. Also, for the sake of comparability of existing results, we follow Sestito and Viviano [2018] rather closely.

First, we look at the probability of getting an open-ended contract and we identify the effect of HI by comparing the change in monthly flow into open-ended jobs in the pool of eligible versus not-eligible workers, before and after the HI reform. Within this setting, there is no obvious way to assess the impact of the reduction of firing costs on the likelihood of getting an open-ended job at the individual level, as the relevant firm size is part of the outcome of a transition and cannot be included among the treatment variables. Sestito and Viviano [2018] resort indeed to aggregated data to circumvent this limitation. Second, we focus on contract conversions from fixed-term to open-ended contract by firm size and we estimate the separate effects of FC and HI, by comparing the conversion rate of eligible versus not-eligible workers in firms of different size classes. In this case, as we condition on the transition to occur within a firm, size can be safely included in the right-hand side of our equations, and treated as a conditionally exogenous variable. The third exercise we perform in this section is a subgroups analysis of the effects, which allows us to understand possible heterogeneities in the response to the policy among gender and age groups.

C.1 Probability of getting an open-ended contract

The identification of the effect of the HI reform on the probability of getting an open-ended position may be obtained by comparing open-ended hires and conversions of eligible ($No_{P_{6m}} = 1$) and non-eligible individuals before and after the HI reform by estimating the following linear probability model:

$$Y_{im} = \alpha + \boldsymbol{\beta} I\left(Post2015m1 * No_{P_{6m}}\right)_{im} + No_{P_{6m}im} + Employed_{m-1} + \gamma_m + \gamma_i + \varepsilon_{it} \quad (Eq.C1)$$

where i denotes individual and m months. The included controls are: γ_m month dummies (interaction of calendar year and calendar month, in order to allow seasonality to change over years), γ_i individual dummies (to capture time-invariant individual effects), $No_{P_{6m}}$ a dummy for the condition of individual monthly eligibility and $Employed_{m-1}$ a dummy for lagged employment status. Y_{im} is the binary outcome, and equals to zero in all the months the individual is at risk of getting an open-ended contract, i.e. when he is employed with a flexible job contract or not-employed, and takes value one in the month *m* of the transition to an open-ended contract. The key variable is the interaction $I(Post2015m1 * No_{P_{6m}})_{im}$ that takes value one if the individual is eligible for the HI in a month *m* after the HI reform was implemented. The DiD estimator β measures the average difference in monthly probability of getting an open-ended contract between eligible and not-eligible after the reform (January-June 2015) minus the same difference before the reform (January 2013-December 2014). Hence, the coefficient (multiplied by 100) indicates by how many percentage points an eligible person is more or less likely to get an open-ended contract in January-June 2015 with respect to a

person who, in the same months, was not eligible for the HI, at the net of any pre-reform difference between the two groups, possible effects of seasonality and individual unobservable characteristics. Results from the estimation of Equation C1 are reported in Table C1. The probability of getting an open-ended contract is estimated separately for men and women and for different populations "at risk", i.e. the unemployed and those already employed without an open-ended contract.

The results confirm a strong positive effect of the HI in the immediate aftermath of the reform on the probability of finding an open-ended contract. Both sexes display similar coefficients and patterns; the probability of finding an open-ended position increased for both men and women by more than four percentage points. Moreover, the effect of the incentive on the flow from flexible employment into open-ended employment is higher than that from unemployment among both men and women.

Table C2 shows the DiD coefficient of equation (C1) estimated separately by age and employment status. The main message is the following: the positive effect found among unemployed is driven by older age groups, while youngsters drive the results for workers employed with a flexible contract.

Table C1 Short-term effect of HI on the probability to find an open-ended job by sex and employment status
(Individual DiD estimates up to June 2015)

	Women			Men		
	All	Unemployed	Employed	All	Unemployed	Employed
	b/ci95	b/ci95	b/ci95	b/ci95	b/ci95	b/ci95
$\boldsymbol{\beta}$ (Effect of HI)	0.052***	0.034***	0.053***	0.045***	0.033***	0.041***
	[0.044,0.059]	[0.025,0.043]	[0.035,0.071]	[0.039,0.050]	[0.027,0.040]	[0.027,0.056]
Adj.R2	0.11	0.13	0.13	0.11	0.12	0.13
#obs.	640,653	357,431	283,222	810,740	473,426	337,314

***Source**: own computations on CO data. Note: the table displays the coefficient β estimated from Equation (A1). Other included controls are fixed effects for: time (year*month); individuals; eligibility (no permanent contract in the previous 6 months) and employment status at t-1. Robust SE clustered at individual level. Significance * p<0.10, ** p<0.05, *** p<0.01.*

Table C2 Short-term effect of HI on the monthly probability to find an open-ended job by age and employment status in the previous month (Individual DD estimates up to June 2015)

	Unemployed			Flexible Employment		
	Young (15-32)	Mid (33-45)	Old (46-70)	Young (15-32)	Mid (33-45)	Old (46-70)
	b/ci95	b/ci95	b/ci95	b/ci95	b/ci95	b/ci95
$\boldsymbol{\beta}$ (Effect of HI)	0.024***	0.040***	0.034***	0.053***	0.052***	0.030***
	[0.013,0.035]	[0.031,0.050]	[0.026,0.042]	[0.031,0.074]	[0.035,0.069]	[0.008,0.051]
Adj.R2	0.09	0.11	0.17	0.11	0.13	0.15
#obs	360,168	247,059	223,630	305,154	172,626	142,756

***Source**: own computations on CO data. Note: the table displays the coefficient β estimated from Equation (A1). Other included controls are fixed effects for: time (year*month); individuals; eligibility (no open-ended contract in the previous 6 months). Robust SE clustered at individual level. Significance * p<0.10, ** p<0.05, *** p<0.01.*

C.2 Probability of contract conversions from fixed-term to open-ended contracts

We now restrict the attention only to individuals who were employed with a standard fixed-term or apprenticeship contract, with the aim to estimate the effect of the two reforms on the probability of "promotion" into open-ended employment. In order to estimate the effect of both HI and FC reforms we specify equation (C2), where the eligible are only standard fixed-term contracts with no open-ended jobs in the previous six months, while not-eligible are the apprentices and those standard fixed-term workers with an open-ended job in the previous six months. Moreover, we include in equation (C2) the enforcement of the reform of FC in March 2015, by looking at how the probability of conversion varies between firms of different sizes. Hence, we estimate the probability of conversions by estimating the following linear probability equation:

$$Y_{im} = \alpha + \boldsymbol{\beta} I\left(Post2015m1 * No_{P_{6m}}\&Fixed\right)_{im} + No_{P_{6m}}\&Fixed_{im} + \boldsymbol{\delta} I(Post2015m3 * large)_{im} + \boldsymbol{\theta} I\left(Post2015m1 * No_{P_{6m}}\&Fixed\right) * (Post2015m3 * large)_{im} + \gamma_m + \gamma_i + \gamma_g + \varepsilon_{it} \qquad (Eq.C2)$$

where γ_m are month-year dummies as in equation (C1), γ_i individual dummies, γ_g firms size groups dummies (three categories). The coefficient β identifies the effect of the HI, comparing before and after January 2015 the workers employed with a fixed-term contract eligible for the HI to the not-eligible. The coefficient δ captures the effect of the FC, comparing the behavior of large (more than 17 employees) versus small (less than 13 employees) firms before and after the introduction of the FC in March 2015. Interactions between the two policies are identified by the coefficient θ.

Results from Equation (C2) are reported in Table C3 distinguishing between women and men. Equation C2 is fully specified in column 4, while columns 1-3 present different specifications of Equation C2 where the two policies are included alone or in combination. The first column reports the effect of HI only, which is always positive and highly significant. The second column reports the effect of FC only, which is not-significant. In column 4, the interaction between the two policies is positive and significant only among men, suggesting that the reduction of FC in large firms acted by strengthening the effect of the HI for them only. Differently from what we observed for the HI, which benefitted both men and women in the same way, it emerges a gender pattern when we focus on the effect of FC alone or in combination with the HI.

Table C3. Short-term effect of both HI and FC on the probability of conversion from fixed-term to open-ended job contract (Individual DD estimates up to June 2015)

	Women				Men			
	(1) HI b	(2) FC b	(3) Both b	(4) Interacted b	(1) HI b	(2) FC b	(3) Both b	(4) Interacted b
β (Effect of HI)	0.022***		0.022***	0.020***	0.024***		0.024***	0.022***
δ (Effect of FC)		0.001	0	-0.001		0.001	-0.001	-0.002**
θ (Effect of HI*FC)				0.004				0.006**
Adj.R2	0.04	0.04	0.04	0.04	0.05	0.05	0.05	0.05
#obs	331581	331581	331581	331581	415370	415370	415370	415370

***Source**: own computations on CO data. Note: the table displays the coefficient β estimated from Equation (A2). Other included controls are fixed effects for: time (year*month); individuals; HI eligibility (no open-ended contract in the previous 6 months); FC eligibility (large firms vs mid and small). Robust SE clustered at individual level. Significance * p<0.10, ** p<0.05, *** p<0.01.*

Results in columns 3 of Table C3 are directly comparable to those in column 2 of Table 5 in Sestito and Viviano [2018]. The latter show that in Veneto – another very large region in the North of Italy, but with a fairly different economic structure – the probability of conversion from a temporary to an open-ended contract is raised by 0.28 percentage points thanks to the firing cost reduction, and by another 1.5 percentage points thanks to the hiring subsidy. These results appear very much in line with ours, even once we cast them in relative terms with respect to pre-reform conversion frequencies (1.3% in Piedmont, and 1.1% in Veneto): in the very short run, the FC had a negligible impact on conversions in both regions, while the HI more than doubled the frequency of transformation from temporary to open-ended jobs. This somehow limits the external validity concerns that may arise from using data from a single region.

Eventually, in order to deepen our understanding of the effects of the two policies on the probability of transformations of temporary contracts into open-ended ones, we estimate the model in column 4 by different age groups. Results reported in Table C4 suggest that the positive and independent effects of *both* the two policies is actually driven only by young male workers, while among older ones the interaction term of the policy is never significantly different from zero.

Table C4. Short-term effect of both HI and FC on the probability of conversion from fixed-term to open-ended job contract (Individual DD estimates up to June 2015)

Short run	Women			Men		
	Young (15-32) b	Mid (33-45) b	Old (46-70) b	Young (15-32) b	Mid (33-45) b	Old (46-70) b
β (Effect of HI)	0.022***	0.023***	0.014***	0.022***	0.021***	0.023***
δ (Effect of FC)	-0.001	-0.001	-0.003	-0.003**	-0.002	-0.001
θ (Effect of HI*FC)	0.003	0.005	0.005	0.008*	0.006	0.003
Adj.R2	0.02	0.06	0.08	0.02	0.07	0.07
#obs	149,754	116,981	64,846	185,546	136,222	93,602

***Source**: own computations on CO data. Note: the table displays the coefficient β, δ, θ estimated from Equation (A2). Other included controls are fixed effects for: time (year*month); individuals; HI eligibility (no open-ended contract in the previous 6 months); FC eligibility (large firms vs mid and small). Robust SE clustered at individual level. Significance * p<0.10, ** p<0.05, *** p<0.01.*

Appendix D: A tentative estimate of the effect of Firing Costs

As explained in the main text, whether measuring separately the causal effect of firing cost (FC) reduction in our framework is possible, is at least questionable. The main reason is that being hired by a small or a large firm, from the point of view of worker, is an outcome, and therefore highly at risk to be endogenous. In this section – albeit with the caveats we have just recalled – we propose a tentative strategy to identify – separately from the impact of the hiring subsidy (HI) – the effect of the reduction of the FC within a not-linear DiD approach. First, we revert from our policy-regime-based approach, to a more standard specification of model (1) (Section 5.2) in which the DiD is applied separately to HI and to the FC:

$$Pr\{Y_{it} = k\} = \Lambda\big(\tau_t\alpha_1 + X_t\alpha_2 + H_t^{2015}\alpha_3 + H_t^{2016}\alpha_4 + E_t\alpha_5 + H_t^{2015}E_t\alpha_6 + H_t^{2016}E_t\alpha_7 + F_t\alpha_8\big) \quad (Eq.D1)$$

where H_t^{2015} (H_t^{2016}) is a set of dummies taking the value of one if the hiring incentive for 2015 (2016) is in place in month *t* of the process, and F_t switches on if the FC reduction was in place. Analogously, E_t is a set of dummies taking the value of one if individual *i* is eligible to the hiring incentive in month *t*.

Then, mimicking the standard DiD, the effect of the firing cost reduction is computed as a double difference of the type:

$$\{E[Y_{it} = k_1|F_t = 1, X] - E[Y_{it} = k_2|F_t = 1, X]\} - \{E[Y_{it} = k_1|F_t = 0, X] - E[Y_{it} = k_2|F_t = 0, X]\} \quad (Eq.D2)$$

where k_1 (k_2) represents an exit to an open-ended job in a large (small) firm.

Claiming causality is less straightforward. Eq. (D2) includes a proper counterfactual and a proper common trend (i.e. the probability that small firms hire with OEC before and after the reduction in FC) only if two conditions hold. First, given that HI are in place when FC are decreased, if HI affect differently small and large firms (and they do), then the effect of HI has to be neutralized. This is why we set their coefficients to 0. I.e., to measure the FC effect alone, in eq. (D2) we set the HI coefficients to 0, to exclude the effect of HI on the $k_1$and k_2 outcomes. Second, if small firms decide to grow above the threshold thanks to the reduced FC, i.e. they were reluctant to grow before the reform and now have no reasons to be reluctant any more, then also this effect has to be neutralized. Hence, we introduce a buffer around the threshold to set aside these firms.[25]

Table D1 displays the results of the analysis of the effect of the reduction of firing costs on the probability of getting a permanent contract in a large with respect to a small firm for unemployed and temporary workers. The results for the unemployed, displayed in Table D1, show that decreased FC has mixed and ambiguous effects on the probability for unemployed individuals of getting an OEC. The effect is negative,

[25] Firms sized in the buffer (firms with 13-17 employees) are included in a separate outcome, and not deleted from the data; Garibaldi et al. [2004], prove however that the effect of the EPL threshold on the propensity of firms to grow, although statistically significant, is economically very small.

than positive and then again negative in the second, third and fourth month, respectively. Than it turns to be not-significant in the following months. When we check for possible heterogeneities and we compute the effect of the firing costs reduction separately for different subgroups of unemployed, namely by gender, age, level of education and tasks content of the previous job we cannot detect any specific pattern. Differences across different subgroups are never significant but for high educated (versus low educated) for whom, the effect of firing cost reduction is in general more pronounced, either more negative or more positive (results not shown, available on request).

Turning the attention to the results for flexible workers, Table D1 shows a bit more consistent signs profile: very few coefficients are significantly different from zero but all are positive, suggesting that if any, the reduction of firing costs has benefited more employed individuals, in particular by facilitating job-to-job movements. No strong differences by HC emerge in this pattern (results not shown here) but for those holding a university degree, for whom the positive impact on being stabilized by their own firm lasts longer.

Table D1 Effect of the FC reduction on the Probability of getting an OEC for Unemployed and Temporary workers

	Elapsed time (months)					
Outcome	**1**	**2**	**3**	**4**	**5**	**6**
Temporary workers: Within-firm promotion to OEC						0.005
Temporary workers: Job-to-job transition to OEC	0.002	0.001			0.002	
Unemployed: Transition to OEC		-0.001	0.003	-0.001		

Source: *own computations on CO-ASIA data. Notes: only coefficients significant at 5% are shown. Standard errors clustered by spells. The included controls are: monthly dummies for individual eligibility and post policy and their interaction; individual time; age; sex; nationality; education; previous job's contract, duration, reason for termination, location, tasks; experience, #employment and #unemployment spells in the previous 3-years.*