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IZA DP No. 18024

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ABSTRACT

The Effect of the End of Hiring Incentives on Job and Employment Security*

We analyse the long-term impact of hiring subsidies on both job and employment security. The subsidy that we examine was introduced in Italy through the 2015 Budget Law, with the goal of promoting open-ended contracts. We employ a non-linear difference-in-differences (NL-DiD) approach within a duration framework, using high-frequency, population-wide linked employer-employee administrative data from a large Italian region. Causal results on job security indicate that the subsidy's protective effect is short-lived. Excess separations from subsidised jobs peak in the exact same month in which the monetary incentive expires. No long-term protective effect of the subsidy is observed regarding employment security. These results hold across a wide range of worker and firm characteristics, showing surprisingly little heterogeneity. One notable exception concerns firm size. Furthermore, the expiration of subsidies disproportionately affects workers with low human capital. Our findings suggest that hiring subsidies are not effective in promoting either job or employment security for beneficiaries and that this raises questions about the efficacy of this common and costly policy, particularly when offered unconditionally.

JEL Classification: H2, J2, J3, J6, L2

Keywords: hiring subsidies, job and employment security, Italy, non-linear DiD, duration model

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1. Introduction

The use of subsidies to foster employment is rather common in OECD countries (OECD, 2010). In recent years, both the OECD (2014) and the European Commission (Bekker, 2017) have advised the design and implementation of such policies to reduce the gap between temporary and permanent workers, finding a receptive response in all EU countries (Eichhorst et al., 2017). In more general terms, the provision of subsidies to firms in order to induce them to hire workers who – for whatever reason – face a weak position on the labour market is widespread, averaging 0.11% of GDP in the pre-pandemic decade in the EU (Appendix Table A1), a slightly higher share than in OECD countries (OECD/EC, 2025). The stated aim of these policies is to align firms' labour cost and workers' productivity so that the job match becomes viable. We can label it a "short-run goal" of the subsidy. The "long-run goal", conversely, is that the experience accumulated by workers will then increase their employability and will allow them to remain attached to the labour market, even as they are unsubsidised, either employed by the same firm that received the subsidy or by a different one. While most of the literature focuses on the short-run goal, studies on the long-run one are scanter. This paper aims to contribute to fill this gap and tests – in a causal sense – whether this long-run goal is actually being achieved.

Our case study is the generous subsidy provided by the Italian Budget Law for 2015. All firms were eligible to receive a 100% rebate of social security costs for 36 months, with a cap of about €8,000 per year,[1] to hire workers under an open-ended contract, provided that they did not already hold one in the previous six months. We study the career of these workers and cover both the period during which the subsidy was in place and after it expired, up to 5 years since the subsidised hiring took place. The focus is on four different outcomes: first, we assess the subsidy's impact on the hazard of separation from the subsidised firm. Following the debate around the concept of flexicurity; we

[1] The cap is only slightly binding, since it is above the social security costs for most new contracts (Sestito and Viviano, 2018).

call this *job security*. However, positive implications for the subsidised workers' careers may and should go well beyond the subsidised work episode, inasmuch as workers are expected to increase their employability in the longer run and, hence, also enjoy easier transitions across different employers. Therefore, and secondly, we focus on the hazard of terminating a reasonably continuous employment period that allows for movement across different firms and for short spells of unemployment. We call this *employment security*. Third, we consider that the existence of the subsidy may induce inactive individuals to join the labour market. This potentially negative selection of workers hired in 2015 – in the sense that they may be relatively weaker on the labour market with respect to the pre-intervention workforce – are interesting for a twofold reason. On the one hand, we must consider that supporting the weakest part of the workforce, including by making participation more attractive, is a stated aim of the intervention; therefore, this ought to be retained in the outcome. On the other hand, though, this potentially negative selection represents a source of bias for our estimates, given that monetary incentives come along a change in the eligible workers' composition. Hence, we also compare the effect of the subsidy on the full population to the one that would have been observed had the population composition remained constant after the intervention. Fourth, and finally, we propose a back-of-the-envelope estimate of the effect on net overall employment and a tentative assessment of the intervention's counterfactual unitary cost.

We take advantage of an exogenous discontinuity in the eligibility to the incentives in order to identify the subsidy's causal effects, given that only workers who did not have an open-ended contract in the previous six months (or who were not apprentices in the same firm) were eligible for the incentive. This discontinuity sets the scene for a difference-in-differences (DiD) approach, as we can compare the employment careers of eligible vs. non-eligible workers hired with an open-ended contract both before and after the introduction of the subsidy. We then observe their working careers for five years. Technically, a duration setting is necessary to control for the dynamic selection that occurs as the spell goes by; this implies non-linearities in the model that we take into account in our counterfactual approach. This model is applied to the population of labour market flows for a large, North-western Italian region representing around 7% of the national labour force.

Causal results on job security indicate that the subsidy's protective effect (i.e., a lower hazard of termination with respect to comparable unsubsidised workers) is short-lived, and that excess separations from the subsidised jobs peak at exactly the month in which the monetary incentive expires. Turning to employment security, no protective effect of the subsidy emerge in the long-run. Hiring subsidies, therefore, were not effective in promoting either job or employment security for the beneficiaries, giving rise to questions about the efficacy of a very common policy, at least when offered unconditionally. All results survive a number of robustness and placebo tests. Furthermore, when we match pre- and post-intervention workers, to cleanse such results from composition effects, we find that even when forcing composition to be statistically the same, thereby nullifying the possible activation channel that the policy may have represented, our estimates remained confirmed. Little heterogeneity across groups emerges, and with unclear patterns. Finally, our macroeconomic exercise suggests that total employment growth goes back to pre-subsidy level three years after the intervention, hinting at a quite costly intervention of questionable efficacy.

Our work contributes to the literature in several ways. First, knowledge about the long-run effect of hiring subsidies is still limited and inconsistent; this is actually surprising, given that these policies are commonly applied and are quite costly, so much so that an evaluation of their longer-run impact is strongly needed. Second, the policy intervention that we study here is nearly untargeted, what minimises general equilibrium concerns and allows for a fully-fledged heterogeneity assessment. Third, non-linearities in DiD models and dynamic selection issues are seldom accounted for in the existing contributions. Fourth, to the best of our knowledge ours is the first article dedicated to examining hiring subsidies in a low-productivity country (i.e., where reduction of labour cost is expected to be most effective).

The paper is organised as follows: Section 2 reviews the relevant literature; Section 3 provides the institutional background of the intervention under scrutiny; Section 4 goes deeper into identification issues and the related empirical strategy; Section 5 describes both the data and the sampling strategy; Section 6 shows the results, distinguishing between job and employment security, discussing placebo, heterogeneity, and robustness checks as well; Section 7 presents back-of-the-envelope calculations of aggregate net employment effects and policy implications arising from our study; Section 8 concludes. Several Appendices provide additional results and details about specific technical aspects of the work. Namely, Appendix A discusses the relevance and external validity of our results. Appendix B shows the relevant diagnostics and robustness for the propensity score matching exercise. Appendix C presents Kaplan-Meyer plots of survival in the processes under scrutiny (i.e., subsidised jobs and employment spells triggered by subsidised jobs). Appendix D shows heterogenous effects, while placebo tests appear in Appendix E. Finally, Appendix F provides a more in-depth analysis of the net changes in overall employment from a macroeconomic perspective.

2. Literature Review

The empirical literature on the effects on hirings of the introduction of hiring subsidies is rather sound and is concordant in finding positive effects (Bruhn, 2020; Sjögren and Vikström, 2015)[2]. A positive effect is also found on the probability to transform a temporary contract into an open-ended, subsidised one within the same firm (Ciani and De Blasio, 2015; Sestito and Viviano, 2018; Ardito et al., 2023). Net employment growth is usually estimated as tiny, but positive (Neumark and Grijalva, 2017; Cahuc et al., 2019). Heterogeneous effects are found, pointing to a larger effect

[2] Earlier works include Cipollone and Guelfi (2006), Martini and Trivellato (2011), and Neumark (2013).

in economically stronger areas (Ciani et al., 2024) or for those workers who are stronger on the labour market, such as those with higher human capital, prime age, and native (Ardito et al., 2023).

Studies on long-run effects (e.g., on tenure or separations after the end of hiring subsidies) are instead scantier. In the case of the hiring subsidy designed for small firms in France in 2009, Batut (2021) estimates that separations do not increase once the policy terminates. Sjögren and Vikström (2015) find that being hired through a subsidy leads to a higher probability of pursuing the work relationship after the end of the subsidy in Sweden; furthermore, increasing the length of the employment subsidies leads to a higher probability of retaining the job once the incentives are over. Saez et al. (2021) study another Sweden-driven subsidy targeting all workers under 26 years of age and found that youth employment grows during the reform and does not decrease after the end of the incentives, something that is also coherent with the macro-level effects found by Egebark and Kaunitz (2018). Albeit with crucial differences, which we will discuss below, Modena et al. (2024) examined the duration of the hiring incentives that we also study here and found a higher probability of subsidised contracts to last more than three years. Conversely, Desiere and Cockx (2022) evaluate the effects of Belgian hiring incentives by focusing on the changes caused by the end of the intervention. Both job and employment duration are evaluated (being employed in the same firm for 1-12 consecutive months and being employed for 1-12, not necessarily consecutive, months). Contrary to results reported above, they found that incentives mostly created short-term jobs, while the effects on stable employment were close to zero.

Some studies explore how the end of the incentives impacted on separations depending on the subsidised job's features. Albanese et al. (2024) examine the impact of a wage subsidy on private sector employment in Belgium, comparing the effect of the subsidy for high-school graduates and dropouts. They found that subsidised employment was, on average, much shorter for dropouts, with no long-term benefits, whereas high-school graduates experienced a positive effect, with their job trajectories shifting persistently from low-paid to higher-paid jobs in the private sector. Delpierre

(2019 and 2022) shows that hiring incentives changed the overall job composition, increasing low-productivity jobs; since low-productivity jobs tend to last less long, the change in composition caused an increase in job destruction. Coherently, Depalo and Viviano (2024) found that small Italian firms reduced the ratio between value added and size as well as their capital accumulation compared to firms that were less exposed to the hiring incentives; these firms did not recover after the incentives ended. Ardito et al. (2022) found the same result for productivity in Italy.

Our research aligns with the literature on the long-term effects of hiring subsidies inasmuch as it relies on worker-level register data to assess effects on eligible workers, rather than actual beneficiaries. Further – with limited exceptions using triple differences (Desiere and Cockx, 2022) or regression discontinuity designs (Albanese et al., 2024; Sjögren and Vikström, 2015) – we make use of a DiD approach, a standard in the literature released to date. We also follow Desiere and Cockx (2022) and Saez et al. (2021) in distinguishing between job and employment security, which is crucial to assess the long-term effects of hiring subsidies.

However, we depart from the existing contributions in five distinctive respects. First and foremost, the policy intervention that we evaluate is nearly untargeted, while others focus on small firms (Batut, 2021), the youth (Albanese et al., 2024; Egebark and Kaunitz, 2018; Saez et al. 2021) or the long-term non-employed (Desiere and Cockx, 2022; OECD et al., 2024; Sjögren and Vikström, 2015). This feature of our work enhances external validity, reduces the general equilibrium issues to a minimum (which are directly studied in our time-series exercise in Section 7 anyway), and allows for a comprehensive study of heterogeneous effects. Second, in spite of its relevance for a long-run analysis, a duration setting is introduced only in Sjögren and Vikström (2015) through a Cox proportional-hazard model and, to some extent, in Desiere and Cockx (2022) who control for elapsed unemployment duration in their specifications. However – and third – the non-linearities implied by duration models or discrete dependent variables and the related implications (Blundell and Costa Dias, 2009; Puhani, 2012) are not considered in any previous work. Fourth, our paper is the first to examine, in a counterfactual way, the long-term effects of hiring incentives in a low-productivity country (i.e., in a context in which variations in labour costs may be more effective). Finally, employment incentives in Italy were of an unmatched generosity during the years under scrutiny, topping almost 0.5% of the GDP while the EU average was four times lower (Appendix Figure A1).

3. Institutional framework

The intervention under scrutiny was introduced by the Italian Budget Law for 2015 (No. 190/2014). It was implemented in the form of hiring subsidies for employers hiring workers with an open-ended contract between January 1st and December 31st, 2015. Specifically, employers were exempted from paying workers' social security contributions of up to €8,060 per year for three consecutive years – corresponding to a 100% exemption for most of the new hirings (Sestito and Viviano, 2018). On the firm side, there were no eligibility requirements (e.g., firm size limits) or conditionalities (e.g., investments in training, workers retention, or technology investments) to benefit from the subsidies.[3] In contrast, there were two main requirements for the workers, so that they could be hired by making use of the hiring incentives. First, they should not have been an apprentice in the same firm prior to the introduction of the subsidised open-ended contract. Second, they should not have had any open-ended contract (in any firm) in the six months preceding the new open-ended contract. Incentives, however, did not follow automatically from fulfilling the two conditions and employers had to file a request; while our data does not allow for the identification of the actually subsidised jobs but only of the eligible ones, the take-up rate was fairly high (INPS, 2019), making our intention-to-treat estimates quite close to an average treatment effect on the treated.

The incentive that we are studying did not occur in isolation. Instead, a similar policy was implemented the following year, although on a much less generous scale. Under the Budget Law for

[3] A few sectors were excluded: agriculture, public sector, and households (caregivers and similar services).

2016 (No. 208/2015), the hiring subsidy was reduced to 40% of social security costs – with a cap set at €3,250 per year – and its duration was shortened to two years. This measure was applicable to all new open-ended contracts activated during the year 2016, conditional on the same workers' eligibility conditions adopted in 2015. The measure was not re-approved from 2017 onwards. Ardito et al. (2022, 2023) show a much lower, often not significant, impact of 2016 incentives on firms' hiring decisions. This is why we have chosen to focus on 2015 incentives only.

Furthermore, Law 183/2014 implemented a reduction of employment protection (EPL) for firms employing more than 15 workers (in what follows we label them "large firms") (Bratti et al. 2021). In particular, the Decree 23/2015 of Law 183/2014 introduced a new form of contract with noticeably lower EPL that applied to all new open-ended hires dating from March 7th, 2015. Employment protection reforms should not confound the effects of the 2015 hiring subsidy, given that the former applies to firms and the latter to workers according to different unrelated thresholds and can be therefore identified separately (Sestito and Viviano 2018). Indeed, Ardito et al. (2023) provide evidence that the identifying assumption that eligible and non-eligible workers have been affected in the same way by the EPL reforms, holds in a difference-in-differences setting. However, in order to be on the safe side, small vs. large firms will be always considered separately. In this regard, a crucial aspect is that the changes to EPL had been announced as early as the beginning of 2015. As a result, permanent hires made in the first quarter of 2015, when subsidies were already in place and as EPL changes had been announced but not yet implemented, are likely to be highly selective. In other words, these hires may have involved workers highly valued by firms, to the extent that they had been hired despite the fact that their contracts were still subject to higher EPL, which was expected to decrease soon. Again, and in order to err on the side of caution, we define the treatment period in our analysis as April-December, 2015. This allows us to ensure that employment protection is homogeneous over time, and avoids the inclusion of the (potentially highly selective) permanent contracts.

4. The econometric models

In this section we discuss the econometric approach that allows us to identify the causal effect of the intervention under scrutiny through a (non-linear) difference-in-differences model.

Treated units are those workers whose previous career make them eligible for the hiring subsidy and who started an open-ended contract during the post-reform period, from April to December 2015. The whole career of the treated units is then followed until the end of 2019. Consistently, the control units are non-eligible workers who started an open-ended job during the same time-frame. To ensure a comparable time structure between the pre- and post-intervention periods, we sample potentially eligible and potentially non-eligible workers who started an open-ended contract between April and December 2010 and we follow them until December 2014. Indeed, we choose 2010 as the starting point in order to allow for equally long observation periods of pre-treatment careers as well as post-treatment ones, both before and after the introduction of the hiring subsidy in 2015.

We define two models (and two samples, as discussed in section 5) in order to analyse our two main outcomes of interest – job and employment security. The first focuses on job spells: all sampled spells along the strategy described previously, followed until the worker is employed by the firm that received the subsidy. The second sample extends the observation period of those same individuals over the whole "employment spell" and up to 5 years (i.e., we allow for different employers provided that the work activity is interrupted by an intervening non-work spell that lasts a maximum of 90 days). We estimate a discrete-time duration model in two settings for both the analysis of job and of employment security: (i) the whole set of open-ended contracts started in April-December 2015 and 2010; and (ii) the subset of those open-ended contracts sharing the same observable characteristics in 2010 and 2015. The latter subset is obtained via propensity score matching, as discussed below and in Appendix B, and aims at excluding the effect of the change in the composition of the pool of applicants in 2015, due to the policy's activation effect on inactive and more marginal individuals.

4.1 Job security

Let us begin with the impact of the hiring incentive on job security (i.e., on the hazard that the subsidised job ends at month t during the validity of the subsidy or after it expires). Intuitively, we need to compare – month by month – the hazard across four different groups, namely treated and control units both before and after the introduction of the intervention. Following Dolton and Van der Klaauw (1999), we estimate a discrete-time duration model on data rearranged to take a person-period form. Jenkins (2005) shows that under these conditions the duration model boils down to a standard logit estimation. Formally:

$$Pr\ (Y_{it} = 1)\ = \Lambda\{\sum_{t=1}^{50} \alpha_t \delta_t + \beta Post_i + \gamma E_i + \sum_{t=1}^{50} k_t\, \delta_t E_i Post_i + \theta X_{it} + u_i\} \qquad \text{(Eq. 1)}$$

where:

- i stands for the individual and t for the individual time elapsed since the beginning of the job, and is measured in months;
- Y_{it} takes the value 0 as long as the job survives, and turns to 1 when it is interrupted;
- $\Lambda(.)$ is the cumulative logistic function;
- δ_t are month-specific elapsed time dummies, and are meant to control for dynamic selection in a non-parametric way (so absorbing unobserved heterogeneity eventually left in the data as much as possible: see Jenkins, 2005)
- $Post_i$ is a dummy taking the value 1 if the job started in the period from April 2015 to December 2015 (vs. April-December 2010);[4]
- E_i is a dummy taking the value 1 if individual i's job was eligible for the hiring incentive;

[4] It is worth noting that the *Post* variable does not depend on the individual time *t*, but only on calendar time, here suppressed for readability. Eligibility E instead depends neither on individual nor on calendar time, given that the condition is assessed for each person at the beginning of the open-ended job spell and remains unchanged until the job is over.

- $\delta_t E_i Post_i$ is a set of month-specific interactions aimed at capturing month-specific effects of (eligibility for) the subsidy switching on only during the *t*-th month after the beginning of the subsidised episode; k_t is the set of parameters of interest that we plot in the figures below;
- X_{it} is a set of controls including a quadratic polynomial in age and flags on gender (females), education (graduates), and nationality (non-Italian natives);
- u_i is the usual i.i.d. error component.

Unlike linear models, the coefficients k_t of the interaction terms $\delta_t E_i Post_i$ cannot be directly read as a measure of the magnitude of the effects of interest in our non-linear model. Instead, as recommended by Blundell and Costa-Dias (2009), Karaca-Mandic et al. (2012), and Puhani (2012), the cross-derivative measuring average treatment effects should be computed as follows:

$$E[Post_i = 1, E_i = 1, \delta_t E_i Post_i = 1, X] - E[Post_i = 1, E_i = 1, \delta_t E_i Post_i = 0, X] \qquad \text{(Eq. 2)}$$

which holds if $\Lambda(.)$ is an invertible function, like the logistic one, and provided that the common trend hypothesis holds, as is always necessary when applying a DiD model.

4.2 Employment security

The approach that we advance concerning employment security closely follows the above, with two relevant changes. First, the unit of analysis will no longer be the single eligible or non-eligible open-ended job, but a reasonably uninterrupted employment spell starting with the open-ended contract sampled for the job security analysis; in other words, we prolong the observation period beyond the end of that job spell and possibly going on with other jobs, including temporary contracts of any kind and with the same or other employers. In order to define a continuous employment spell, we must first impose that non-employment gaps should last one quarter at most. Second, the unit of time will be quarters instead of months; indeed, as we no longer need to pinpoint the exact moment on which the subsidy ends, we can save on computational effort and can increase efficiency by reducing the number of parameters to be estimated for equation elements α_t and $k_t \delta_t E_i Post_i$. The estimand will, therefore, read:

$$Pr\,(Y_{it}=1)\;=\Lambda\{\textstyle\sum_{t=1}^{17}\quad \alpha_t+\beta Post_i+\gamma E_i+\sum_{t=1}^{17}k_t\,\delta_t E_i Post_i+\theta X_{it}+u_i\}\qquad\text{(Eq. 3)}$$

where Y_{it} takes the value 0 as long as the reasonably uninterrupted employment period goes on, and 1 if a break occurs.

4.3 Propensity score matching

The introduction of a very generous hiring incentive, which de facto cancels social security costs for most new hires, may have had the effect – intended by the policy makers – of activating out-of-labour-force individuals who had been previously excluded from hiring by firms because of their low productivity or who may have given up searching for a job because they were discouraged. From our standpoint, this means that the effect of the intervention under scrutiny can be conceptually separated into two components: (1) the effect on job or employment security of those workers who would have participated in the labour market even without the incentive – which we can consider the "pure" effect from an econometric standpoint, and (2) an activation effect that brought otherwise inactive or unemployed individuals into work.

To control for this potential change in the composition, and hence to insulate the first component, we adopt propensity score matching (PSM) techniques to match individuals sampled in 2010 with their "twins" sampled in 2015 to impose, to the extent possible, the preservation of pre-treatment sample characteristics on the post-treatment sample. In principle, we would like to keep all 2010 spells and the subset of 2015 spells that includes all and only their twins. In our main analysis, we employ a nearest neighbour propensity score matching with no replacement and a caliper of 0.4.[5] This matches 97% of 2010 spells to 65% of the 2015 sample, which – consistently with our "activation" hypothesis – is larger in size (see Table 1).

[5] A relatively large caliper tends in the direction to preserve the pre-treatment sample. Allowing for replacement may have the effect of disregarding an excessive number of post-treatment observations, in case a small share of them represent a vast majority of pre-treatment units. Hence, following Lunt (2013), we play around with both conditions, trying replacement and a smaller caliper, and provide standard diagnostics in Appendix B.

Comparing results obtained with the full sample to those obtained with the matched sample we can infer whether the pool of employed workers changed after the introduction of the subsidy and if this had an effect on average job and/or employment security.

In Section 6, we present the results obtained with PSM, while Appendix B discusses the matching strategy and its robustness to alternative parametrisations and show the results of diagnostic tests designed to validate its underlying assumptions, thereby ensuring the reliability of our estimates and findings in greater depth.

5. Data, sample selection and descriptives

The analysis was carried out using an administrative archive called Comunicazioni Obbligatorie (CO). This is the administrative register of the entire population of daily labour market flows since 2008, managed locally by the Italian regions, under the coordination of the Ministry of Labour. We gained access to CO data from a large North-Western Italian region, Piedmont.[6] The flows covered are those by firms whose plants are located in the Region, but individuals can be followed outside Piedmont too, in case they change employer and work in another region, provided that their domicile is still in Piedmont. CO database records hires, separations, and contract transformations (e.g., from temporary to open-ended). The CO database is a linked employer-employee panel. Observables include workers' demographics (age, gender, nationality, education, and domicile municipality), contract type,[7] occupation (5-digit ISCO code), sector of activity (6-digit NACE code), start and (should the case arise) end and transformation dates, as well as reason given

[6] Piedmont accounts for about 7% of the Italian population, GDP, and workforce. Its economy is in transition from a strong manufacturing vocation represented by private firms larger than the national average, to a service economy that is not yet fully developed. The dynamics in open-ended employment that emerge in our data, however, resembles the one that we can observe at the national level using aggregate social security data fairly closely, thereby corroborating the external validity of our exercise (see Appendix Figure A2).

[7] Contract types are: (i) open-ended, (ii) standard direct-hire fixed-term dependent contracts, (iii) agency workers, (iv) apprentices, (v) consultants (so-called *para-subordinati*), (vi) interns (so-called *tirocinanti*), (vii) on-call jobs, and (viii) domestic work, as recoded by Veneto Lavoro (2016).

for contract termination. Periods not covered by a job spell are periods of non-employment, and they may include both unemployment and inactivity spells. Following Ardito et al. (2023), CO data is merged with the National Archive of Active Firms (ASIA, maintained by the national statistical office, ISTAT). This allows us to retrieve an accurate measure of the size (at the establishment level) to separate the analysis in terms of small vs. large firms, precisely around the threshold that activates a more binding EPL and its changes introduced in March 2015 and discussed previously.

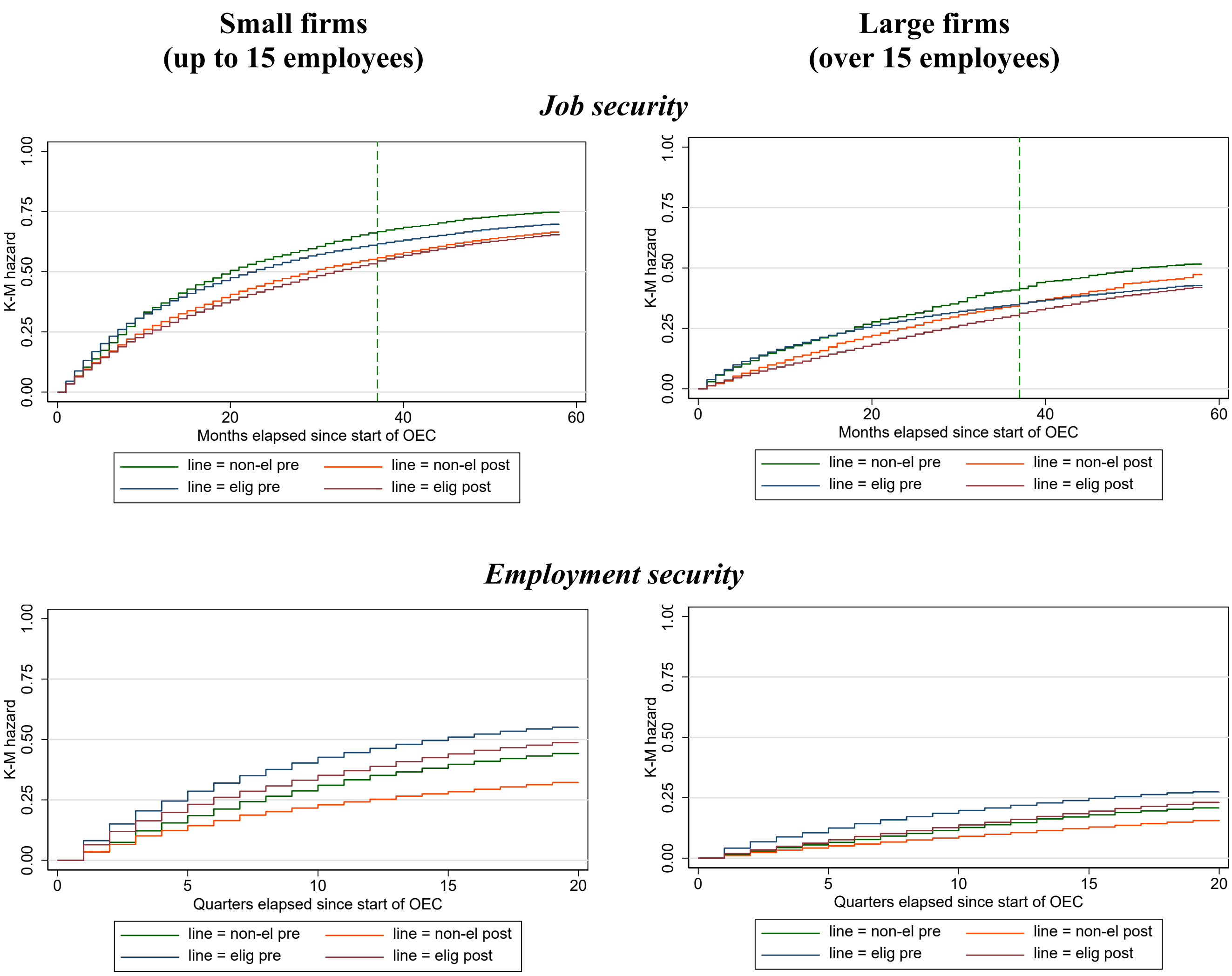
We define two samples in order to analyse our two main outcomes of interest (i.e., job and employment security). For job security, the statistical units will solely be the (eligible and non-eligible) open-ended contracts that started in the two relevant time windows (i.e., April to December, 2015 or 2010 by an employer based in Piedmont). These episodes will then be followed monthly until interruption, either due to a change of employer or because of an episode of non-employment. The second sample, aimed at studying employment security, extends the definition of the statistical units to include not only the open-ended episode started in the two relevant time periods, but also any other possible subsequent job episodes with any kind of contract and including those with other employers, provided that no excessively long period of non-employment intervenes (i.e., 90 days). In this second sample, about 60% of workers are employed by one firm only (i.e., either they never separated from the initial firm over the observation period or they did not find a new employer). The analysis on job and employment security technically coincides for this subset of individuals (see Section 6 for a discussion about the implications of this feature of the data).[8]

Figure 1 plots the Kaplan-Meyer hazard estimates[9] for each treated/time group in order to give a flavour of the hazard rates of separation in the two settings (job and employment security). This provides a benchmark size of the phenomenon under study; it is intended for comparison with the size of the double change in the hazards (the DiD-hazards) that are presented in the following section.

[8] All jobs following the initial open-ended one may also be at plants located outside Piedmont, a condition that holds provided that the employee still has their domicile in Piedmont.

[9] In Appendix C, we also plot the Kaplan-Meier survival rates.

Figure 1: Kaplan-Meyer hazard rate by firm size, period, and eligibility.



Source: Own computations on CO-ASIA data. Note: elapsed time is measured in months (quarters) for job (employment) security.

Table 1 describes the final sample of open-ended contracts selected for the job and employment security analyses, and shows its size, with breakdowns by firm size, eligibility, and treatment period. It offers some support to our hypothesis that the subsidy may induce inactive individuals to join the labour market, thereby introducing a different compositional profile after the reform: indeed, the number of eligible workers in 2015 was growing more sharply than the number of non-eligible ones with respect to 2010.

Table 1. Number of open-ended contracts activated in the observation periods, by eligibility, pre/post treatment period, and firm size.

	Small firms (up to 15 employees)	Large firms (over 15 employees)	Total
Non-eligible workers			
Pre-period (April-December 2010)	11,010	7,438	18,448
Post-period (April-December 2015)	11,980	10,873	22,853
Total	22,990	18,311	41,301
Eligible workers			
Pre-period (April-December 2010)	34,741	28,430	63,171
Post-period (April-December 2015)	57,521	41,526	99,047
Total	92,262	69,956	162,218
Overall total	115,252	88,267	203,519

Source: Own computations on CO-ASIA data. Note: Non-eligible workers are those who had a permanent contract in the previous 6 months or who had been apprentices in the firm; eligible workers were those who did not have a permanent contract in the previous 6 months and were not apprentices in the same firm.

A final remark is warranted so as to highlight the change in sample composition between 2010 and 2015, as revealed by the propensity score analysis described in section 4.3 (for more details, see Figure B1 in Appendix B). Notably, in 2010, we observe a higher proportion of older, high-skilled and experienced workers, as well as a higher proportion of months spent with an open-ended contract in the previous 24 months when compared to 2015. This finding is consistent with the notion that more marginal workers entered the labour market in 2015.

6. Results

In what follows, we present the results of the models outlined previously. First, we focus on job security in the full unmatched sample; then we discuss what changes when keeping the sample composition constant (i.e., restricting the sample to "2010-2015 twins" via propensity score matching). Finally, we will move on to examining employment security in much the same way.

The empirical results of our NL-DiD analyses are presented with graphs that plot the coefficients k_t for each month, or quarter when we study employment security. They are the double difference $(A - B) - (C - D)$, where:

- A is the hazard of termination of the job (employment) spell of the eligible workers in regime "after";
- B is the hazard of termination of the job (employment) spell of the eligible workers in regime "before";
- C is the hazard of termination of the job (employment) spell of the non-eligible workers (the control group) in regime "after",
- D is the hazard of termination of the job (employment) spell of the non-eligible workers in regime "before".

In other words, the NL-DiD hazard is the excess hazard of work termination of the eligible workers in a causal sense (i.e., with respect to the no-policy state of the world). If the policy is effective, then we should estimate a negative NL-DiD-hazard: the policy is protective (i.e., it decreases the hazard of termination of the work spell of the eligible workers in a causal sense). However, if we estimate a positive NL-DiD-hazard, then the policy backfires, making those individuals treated even more vulnerable to job or employment separation than they would have been without the intervention. Of course, if we estimate a non-significant Nl-DiD-hazard, then the policy is (costly but) ineffective.

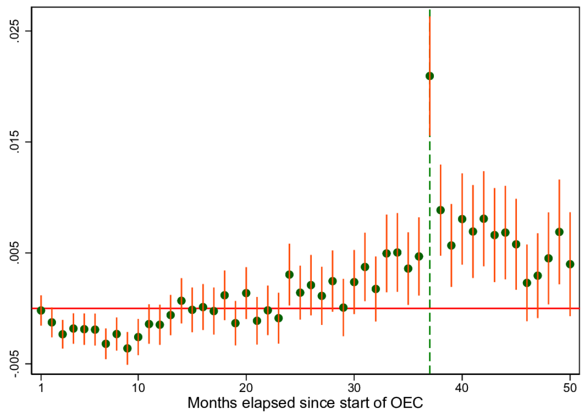
6.1 Job security

Figure 2 displays our main results. The NL-DiD-hazard can be compared to Figure 1 that plots the unconditional hazard by eligibility and period in order to have an idea of the effect's magnitude. We are particularly interested in two aspects: first, in whether subsidised workers were more protected against job loss during the subsidised period, and possibly beyond it, at the subsidised firm; second, in whether separations rose at the end of the subsidy period – i.e., at month 37 and beyond.

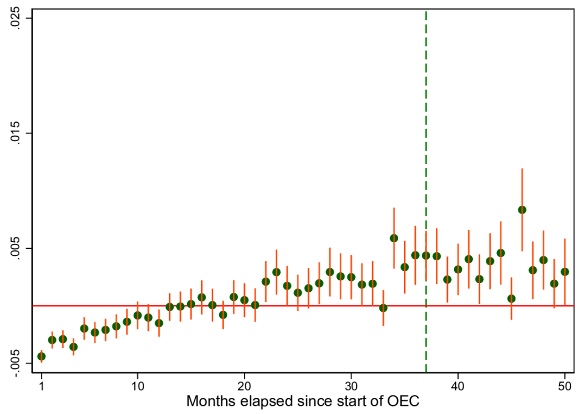
Figure 2. Non-Linear DID estimate of the policy's effect on Job Security, by firm size and sample type

Panel 1: Full sample (Non-linear DID estimates)

1A: Small firms (0-15 employees)

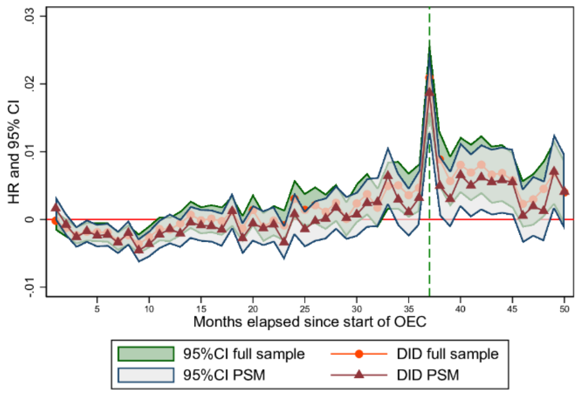


1B: Large firms (>15 employees)

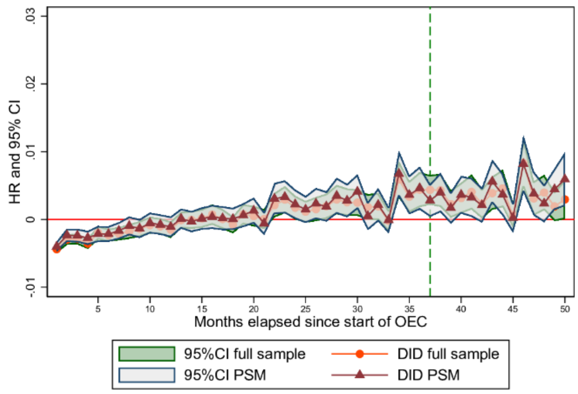


Panel 2: Full sample VS PSM matched sample (Non-linear DID estimates)

2A: Small firms (0-15 employees)



2B: Large firms (>15 employees)



Source: Own computations on CO-ASIA data.

Close inspection of Figure 2 reveals that a protective effect arises in both small (panel 1A) and large firms (panel 1B), but it lasts less than a year from the beginning of the subsidised job. The protective effect is pretty small in both small and in large firms, thereby decreasing the hazard by about 0.001 when the benchmark hazard in the first months of the spell is about 0.2 (Figure 1). After about one year, the policy becomes ineffective. However, we observe an increasing pattern of the NL-DiD hazard, that becomes positive and spikes right at the end of the subsidy (in small firms) or around that month (in large firms), suggesting that either at least part of the hirings due to the programme turned back to being unprofitable without a labour cost reduction, or that employers perceived them as de-facto temporary positions since hiring them. In small firms, the benchmark hazard is about 0.5 at month 37; the estimated spike increases it by 0.2 and this is a rather large effect. In large firms, the benchmark hazard is about 0.25, and the NL-DiD-hazard increases it by about 0.005, a smaller jump, albeit a statistically significant one.

All in all, this first part of the analysis points to an intervention that, although it increased hirings under open-ended contracts (Ardito et al. 2023), did not generate a lasting protective effect on the job. On the contrary, the policy soon became ineffective and even backfired after about 2.5 years, increasing (in causal terms) the hazard of separation of beneficiaries. This might be due to the negative selection of beneficiaries, generated by the policy itself, that induced individuals quite detached from the labour market to move to employment. This was one of the policy's aims, and it is interesting per se; however, we have to check whether the results that we obtained were driven by these additional workers. The lower panels of Figure 2 (2A and 2B) plot the NL-DiD hazards as estimated on the matched samples, thus excluding the change in the composition of applicants after the subsidy was offered. Hazards from panels 1A and 1B (unmatched sample) are superimposed, for the sake of comparability. The patterns are unchanged: estimated NL-DiD hazards are only marginally smaller and marginally less significant when estimated with the matched sample. This is somehow a surprising result, given that we see that average individual and past-career characteristics are quite different in the full and in the matched sample (Appendix B). However, this is consistent with the heterogeneity analysis that we conduct below, which finds very little heterogeneity in the estimated effects of the policy intervention when we disaggregate the sample by gender, non-native/native, under/over 30, non-graduate, blue/white collar, low-skill/high-skill, non-innovative sectors, manufacturing/services, and incumbent/outside hiring (see Appendix D, Figure D.1). Indeed, we observe that the largest heterogeneity is driven by the firm's size. The subsidy's protective effect during the first year of the new job is present in almost all of the subgroups employed in large firms, while it is present only for foreign, or manual workers, or employees in manufacturing in small firms. In the longer run, the main results are confirmed, with most subgroups experiencing the increase in the hazard of separation as the subsidy ends. In particular, those who experience higher susceptibility to the subsidy, and who face higher job terminations when the subsidy ends, are those with lower human capital or those working in small firms.[10]

One final remark is due in order to compare our results to Modena et al. (2024), who found an increased probability of being employed after three years if hired with this same subsidy, in a DiD-linear probability setting. Although the similarities between this research and theirs are clear, there are substantial differences that make the results hard to compare. The most obvious difference lies in the choice of the model and the dependent variable (as different outcomes are estimated), but a more significant distinction concerns the sample selection. In Modena et al. (2024), agency and apprenticeships contracts are excluded, whereas contracts signed in the early months of 2015 are included. These represent a notable difference, because – as discussed previously – they benefit from the hiring incentives, but are unaffected by the reduction in EPL, which came into effect shortly after the incentives, but which had been announced earlier. We believe that this group of workers is strongly selected. Given that these workers are harder to dismiss, they are likely to get hired with the intent of establishing long-term employment relationships, thereby significantly increasing the average duration of eligible contracts. Furthermore, Modena and colleagues overlook the problem of dynamic selection, which is taken into account in our duration setting: as positive duration dependence in employment is likely to exist, and the subsidy at least initially created a protective effect for the eligible workers, the unconditional (on previous elapsed time in employment)

[10] In fact, the increased risk of losing one's job or terminating employment after the second/third year after the start of the open-ended contract is present in most groups; however, (i) it is *always* present among small firms and (ii) among large firms, but it is practically absent among: university graduates, incumbents, the high skilled, and firms in innovative sectors. It should be noted, however, that some subgroups have a small sample size and this might have hampered the significance of their estimates.

probability to be employed after three years estimated in Modena and colleagues is likely to be biased upward for the eligible workers.

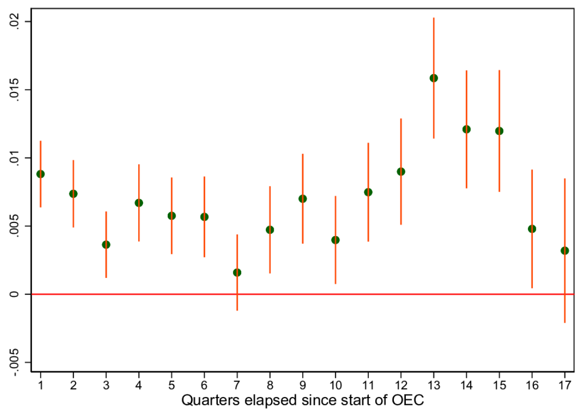
6.2 Employment security

We repeat the analysis provided previously by turning our attention to employment spells across different firms. As anticipated, we define an "employment spell" as work activity that can be interrupted by an intervening non-work spell that lasted a maximum of one quarter.

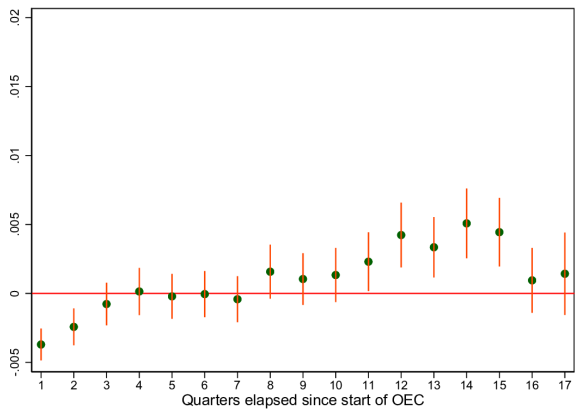
Figure 3: Non-Linear DID estimate of the effect of the policy on Employment Security, by firm size and sample type

Panel 1: Full sample (Non-linear DID estimates)

1A: Small firms (0-15 employees)

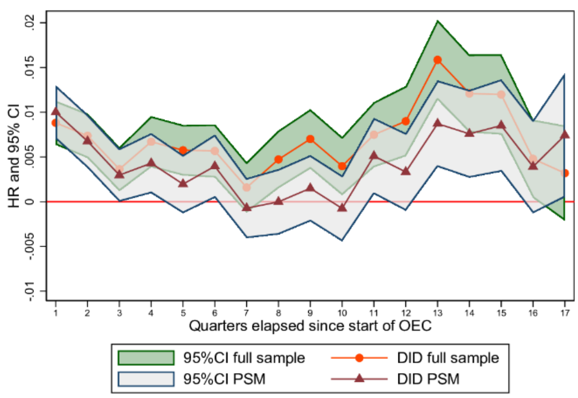


1B: Large firms (>15 employees)

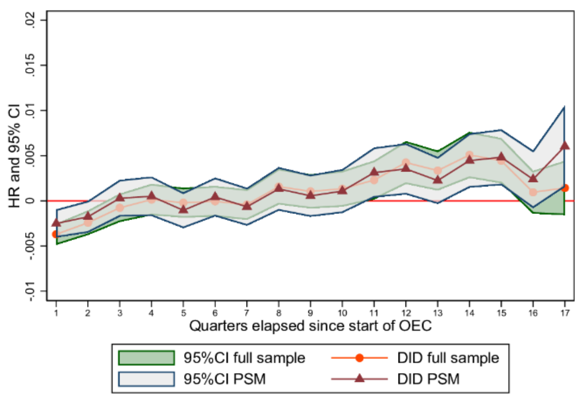


Panel 2: Full sample VS PSM matched sample (Non-linear DID estimates)

2A: Small firms (0-15 employees)



2B: Large firms (>15 employees)



Source: Own computations on CO-ASIA data.

Figure 3 is structured similarly to Figure 2 and presents the results of our estimates on this operationalisation of the idea of employment security. We find a confirmation of a brief protective effect of hiring subsidies with respect to the risk of ending the employment spell among only those initially hired by a large firm. The size of the impact is very small, although it is statistically significant, being the benchmark hazard of about 0.05 in large firms; it lasts only two quarters in the full sample and only one in the matched one. The pattern of job and employment security for those initially hired by a large firm are quite similar in both samples and in both models: the NL-DiD-hazard increases as time goes by.

In turn, the effect of hiring incentives on employment security for those initially hired by a small firm is quite different. We notice a few interesting features among them: (i) the pattern in employment security is always positive in contrast with the protective initial effect found for job security; (ii) there is a quite large increase in the hazard of employment termination during the 13th quarter (i.e., after 3 years); (iii) the effect of the selection is more evident when comparing full and matched sample. This begs the question: how can these patterns be reconciled?

First, let us begin by focusing on the first year of employment: the protective effect that we see in job security turns into an excess hazard in employment security. This can be reconciled by thinking that employment security patterns are an average – month by month – of job security for the stayers (i.e., those who do not leave the first firm) and employment security for the movers (i.e., those who leave). Hence, those who leave seem to be quite unlikely to be able to find another employer within a quarter, so their NL-DiD-hazard of terminating the employment spell is quite high. This is also true in the years that follow, as we average between a policy that does not work for job security (null effects between the 10th-30th months, Figure 2 panel 1A) and a policy that does not work for employment security (always positive effects, Figure 3 panel 1A).

Second, following the same argument, we might remember that the spike in the NL-DiD hazard of job termination at month 37 was very high. This drives the "average" DiD-hazard of employment termination up at the corresponding quarter (i.e., 13^{th}).

Third, we observe that keeping the composition of the pool of beneficiaries constant before and after the treatment, the estimated effects of the policy are shifted down only in the case of small firms, hence suggesting a lower increased risk of termination. In particular, in this case the policy is mostly ineffective and backfires, thereby leading to a higher risk of termination only in the first two quarters and then after three years. We interpret this as an indication that the quality of the "additional" matches generated in small firms were lowered due to the subsidy. Matches that were not immediately profitable were tried, thanks to the lower labour cost, but if the match was not successful then these workers had greater difficulties re-entering the labour market, likely because they were no longer eligible for another subsidy, after the first one.

Finally, we examine the potentially heterogeneous impact of the subsidy on employment security by using the same dimensions as in the previous section (see Appendix D, Figure D2). Consistent with our findings on job security, we observe the most significantly heterogeneous responses by firm size. Furthermore, patterns are relatively homogeneous for those initially hired by large firms, with the exception of younger workers who benefit from more pronounced and longer-lasting initial protection when subsidized. Among those initially hired by small firms we find more heterogeneity: higher separation risk is observed among male, or more mature workers, or among those with low education, or employed in low-skilled jobs, or lacking prior work experience within the first firm. This is consistent with the literature discussed in section 2 and points to the lower efficacy of subsidies when low human capital or low skill jobs are involved.

Ultimately, our findings suggest that the initial disadvantages faced by workers targeted by the policy persist, with no evidence of either temporary or long-term improvement. This result calls the effectiveness of a widely used and quite costly intervention into question, thereby highlighting the need for a re-evaluation of its merits at least when not clearly targeted to specific subgroups of workers, as the existing literature seems to suggest.

6.3 Placebo tests

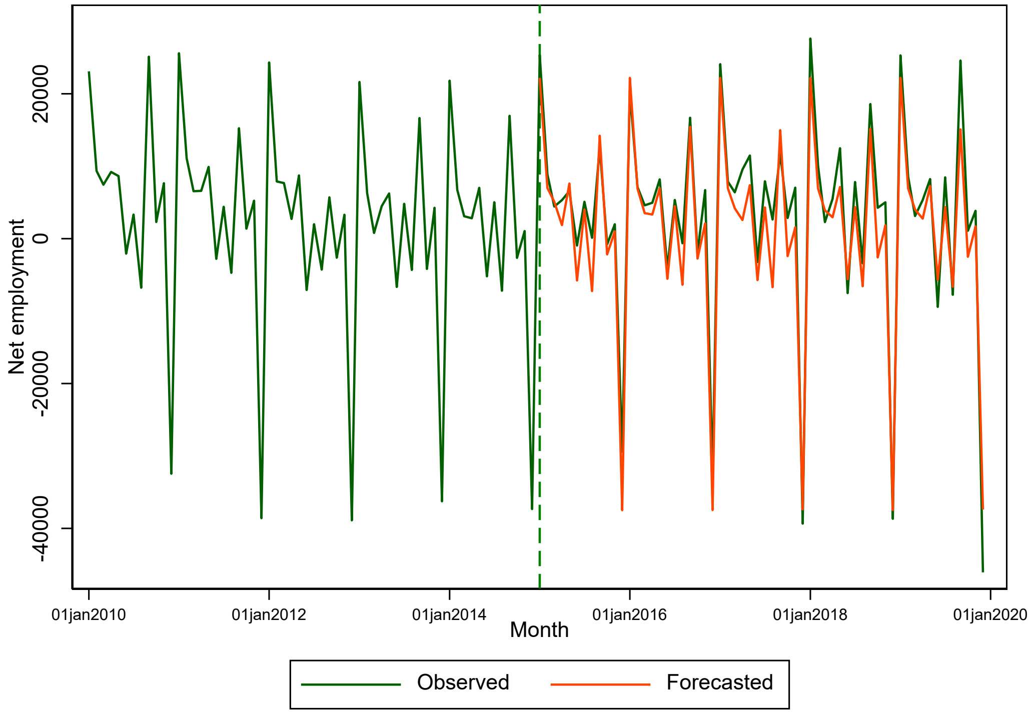
This section presents some placebo tests, in order to support our NL-DiD results. Referring to the four models presented in the previous section (job and employment security, full vs PSM sample), we replicate the models using subsamples that are defined as follows. First, we compare non-eligibles to other non-eligibles, pretending that one of the two was instead exposed to the treatment. In fact, individuals who had an open-ended contract in the previous 6 months were excluded from the subsidy; hence, we split them in two parts: those who had an open-ended contract in the previous 3 months and those who had an open-ended contract in the previous 4 to 6 months. Second, we analogously compare eligibles to eligibles: those who had an open-ended contract in the previous 7 to 12 months and those who had an open-ended contract in the previous 13 to 24 months. No effect of the subsidy should be detected in either case. In Appendix E, we plot the related figures. In the case of job security, the vast majority of NL-DiD monthly hazards are non-significant, as expected for both the placebo on eligible and on non-eligible workers, while only very few of them are, but at scattered months and showing no pattern (Figure E1). The same holds for the placebo on eligible workers only (Figure E2). Moving to employment security models, we detect the same reassuring results (Figures E3, E4). All of this supports our identifying hypotheses, both in the full sample and in the PSM matched sub-sample, and it helps us to claim that our estimates detect a causal effect of the policy, so that the DiD-hazards we estimate are the additional effect generated by the hiring subsidy with respect to a “no-policy” scenario.

7. General equilibrium net employment effects

A key goal of the policy intervention was to boost employment and mitigate job losses. While Ardito et al. (2023) found that the subsidy had a significant impact on hirings, our analysis reveals that its protective effect was short-lived. In fact, we observed a surge in separations once the subsidy came to an end. This raises questions about the net effect of the hiring incentives over time. To better understand this, we conduct a simple calculation of the net change in employment at the aggregate level, taking potential substitutions between different types of contracts into account that may occur when one type becomes relatively more convenient than others.

We employ an ARIMA (Auto Regressive Integrated Moving Average) model in order to estimate the effect of the hiring incentives on total regional employment. For this exercise, our dataset consists of full-time equivalent monthly net flows from January 2010 to December 2019, aggregated across all sectors and contract types, to capture spillover and substitution effects. We used the 2010-2014 data to train the ARIMA model and forecast employment flows from 2015 to 2019 after confirming the stationarity of the series, using both Dickey-Fuller and Phillips-Perron tests (see Appendix F for details). This allows us to simulate a counterfactual scenario, where the hiring incentives were not introduced in 2015. Therefore, we can isolate the intervention’s effect by comparing the forecasted values with the actual observed values. It is important to note that this analysis does not imply causality, but rather provides a descriptive assessment of the relationship between the intervention and the outcomes observed. The results are visualised in Figure 4.

Figure 4: Observed vs forecasted employment net monthly flows, full time equivalent units



Source: Own computations on CO-ASIA data. **Note**: The vertical dotted line indicates the introduction of the hiring incentives, January 2015.

The plot in Figure 4 reveals an additional employment net flow in 2015, 2016, and 2017, as suggested by the positive gap between the observed and forecasted values, although the estimates become less precise over time (the further we move away from time 0 – January 2015 – the more difficult estimating what the net monthly flow might have been becomes). The less pronounced turnover, characterised by smaller positive and negative peaks, can be attributed to the growing proportion of permanent employees compared to temporary ones. Nonetheless, the similarity between forecasted and observed flows indicates that the gap between them is relatively small.

To quantify the impact of the incentives on net job creation, we employ another ARIMA model that incorporates autoregressive and moving averages components, as well as two policy impact variables: "step" and "ramp", as is standard in an interrupted-time-series approach (Appendix

F, Table F1). The “step” variable estimates the change in intercept (level shift in the employment flows) after the hiring incentives were introduced, while the “ramp” variable estimates the slope of the effect (increasing or decreasing over time). In other words, we approximate the post-intervention change in monthly net employment flows with a linear trend. It is worth noting that this approach is not capable of insulating the effect of the incentives for the specific period used in the counterfactual exercises outlined about (i.e., from April to December 2015); instead, it captures the break represented by the incentives in both 2015 and 2016, as described in section 3.

The results indicate that the hiring incentives had an initial positive effect on the average net employment flow, with an estimated increase of 2,519 jobs (step = 2,519). However, this positive impact tended to reduce over time, with a monthly reduction of 73.3 jobs (ramp = -73.3). To illustrate this, the estimated net job creation would be 2,519 – 73.3 = 2,445.7 in the second month after the introduction of the subsidy, and this decline would continue linearly in subsequent months as imposed by the model’s specification. This back-of-the-envelope estimate implies that after approximately 35 months since the incentives were enforced, the net employment flows had returned to their pre-policy levels (2,519 – 73.3x35 = ~ 0, indicating that the employment flows after 35 months are comparable to the pre-intervention ones). This finding is consistent with the hiring incentives policy’s duration, which was limited to 36 months (three years) for the incentives introduced in 2015.[11]

It is essential to interpret these estimates with caution, given that they are intended to provide a rough indication of the net effect of the incentives on full-time equivalent employment in the region, rather than exact figures. Nevertheless, the substantial total cost of the policy in Piedmont, estimated at 1.2 billion Euros,[12] suggests that the transitory positive net employment effect, although roughly

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[11] The linear approximation used here also captures the macroeconomic effects of the hiring incentives introduced in 2016 by construction. They share the same design as for the ones introduced in 2015, although they were much less generous (see section 3 and Ardito et al., 2023, for an analysis of their smaller effects on hirings). Most notably, their duration was set at 24 months, therefore ending alongside those introduced in 2015.

[12] According to INPS (2019), the total cost of the policy, encompassing both the 2015 and 2016 incentives, was approximately 16.7 billion Euros across the entire national territory. Unfortunately, regional breakdowns are not available. Nevertheless, we can estimate that Piedmont's share of the total incentives paid was roughly proportional to its workforce and GDP, which accounts for around 7% of the national total. It is worth noting that these calculations are likely

estimated, came at a significant price. Indeed, assuming that these back-of-the-envelope estimates are accurate, then the policy is responsible for the creation of 41,866 new full-time equivalent jobs (including spillover effects onto other sectors and types of contracts) by the end of the subsidised period (i.e., after 36 months). This translates to a cost of approximately €28,663 per job created, on average. To put this cost into perspective, we need to compare it to the annual face value of the subsidy, which was capped at €8,060 for open-ended contracts activated in 2015, and €3,250 for those started in 2016. Therefore, once we weight our estimated cost according to the average duration of the newly created job positions, using the median duration of open-ended contracts started in 2015-16 – i.e., 872 days – then we obtain a yearly cost of €11,993 per each new job created, which exceeds the annual face value of the subsidy by a significant margin.[13]

8. Concluding remarks and policy implications

In this paper we study the effects of hiring incentives on subsidised jobs' outcomes, with a particular emphasis on what happens when subsidies end. We leverage a policy intervention introduced in Italy in 2015, which provided subsidies to hire workers without an open-ended job in the previous six months and which aimed at fostering employment under open-ended contracts. We distinguish between two different outcomes: job security – i.e., the probability that the subsidized job survived to the end of the hiring incentive – and employment security, i.e., the probability that a subsidised worker was able to stay employed beyond the initial subsidised job, across different employers. These outcomes are critical for understanding the long-term effects of hiring incentives on workers' career trajectories. We utilise a non-linear difference-in-differences approach in a

conservative, as INPS (2019) also indicates that northern regions, including Piedmont, benefited disproportionately more from the hiring incentives compared to other regions.

[13] These findings are consistent with Bondonio and Martini (2004), who examined the impact of hiring incentives paid to artisan firms in Piedmont in 2000-2001. According to their study, "*for every €100,000 of public spending in support of artisan enterprises, total employment over the two-year period has increased on average by 4.78 units*" (p. 4). This implies a yearly cost per employment unit of approximately €10,460. This comparison should however be taken with a grain of salt provided that the two target populations are quite different.

duration setting to control for dynamic selection in order to account for the complex dynamics of job tenure and worker mobility.

Results from the administrative population of labour market flows from a large North-western Italian region yield five key findings. Firstly, the subsidy causes a sizable peak of excess separation hazard right at its end. Secondly, a short-lived and modest protective effect – whence the subsidy reduces the separation hazard – is observed during the first quarters of the subsidized spells. These two main effects – short-term protection and long-term increased risk of separation – are remarkably consistent across most workforce segments. However, our results suggest that workers with low human capital are disproportionately affected by these effects. Fourthly, while the policy may have prompted an activation effect among eligible workers – i.e., people detached from the labour market may have decided to participate as a consequence of the hiring incentives – we show that our main results remain robust to this potential compositional change. Lastly, interrupted time series calculations reveal that the excess net hiring at the regional level, encompassing all kinds of contract types and sectors, remains positive for only three years.

The most concerning implication of our study is the lack of a lasting reduction in the hazard of experiencing prolonged non-employment spells. Notably, the substantial cost of the policy – amounting to 16.7 billion Euros at the national level according to social security administration data (INPS, 2019) – does not translate into improved long-term employment security for beneficiaries. This finding is particularly striking given the largely untargeted and unconditional intervention. Our results suggest that such subsidies may not be an effective means of promoting sustained employment outcomes, thereby highlighting the need for more targeted and conditional approaches to labour market policy.

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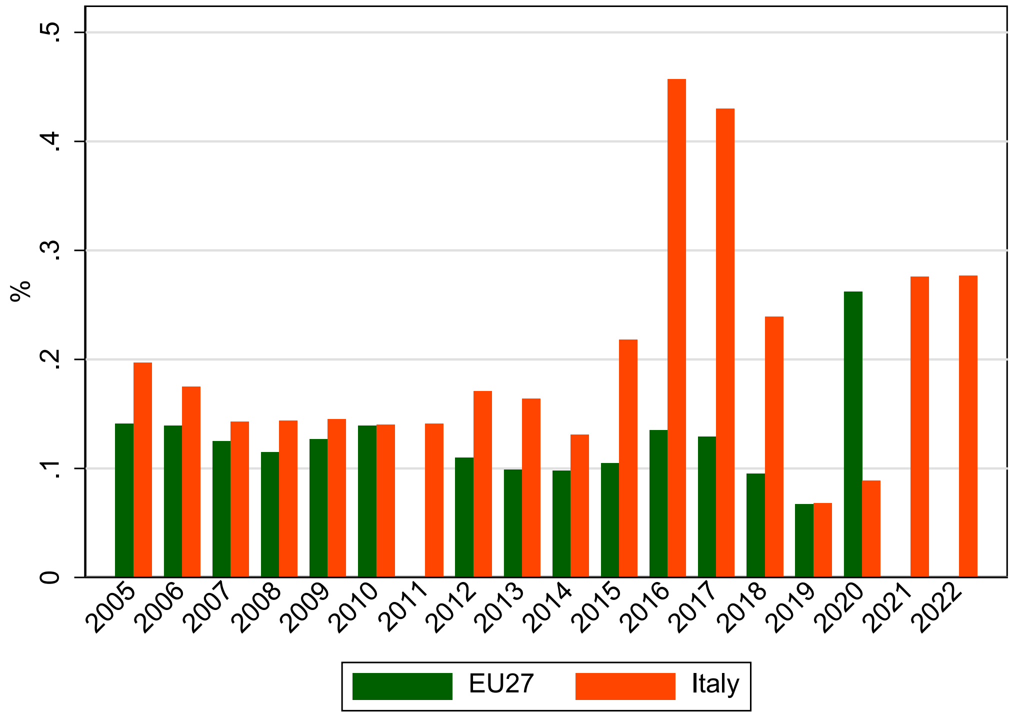
Sjögren, A. and Vikström, J. (2015) "How long and how much? Learning about the design of wage subsidies from policy changes and discontinuities", Labour Economics, 34: 127-137.

Veneto Lavoro (2016) "Grammatica delle Comunicazioni Obbligatorie", web mimeo.

Online Appendix

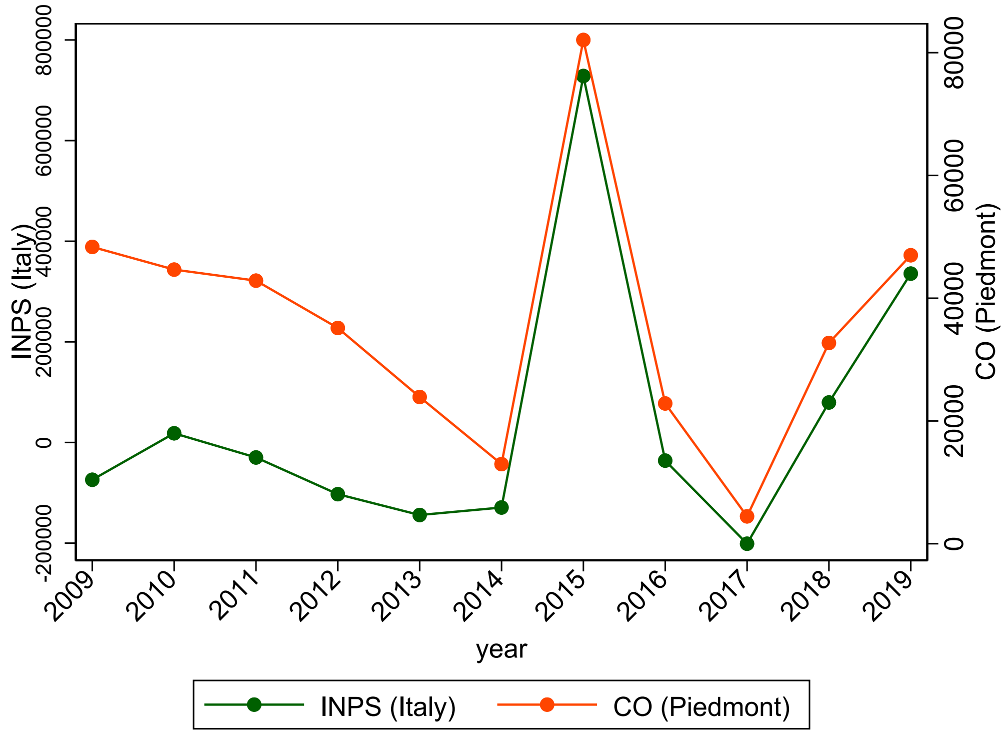
Appendix A: External validity

Figure A1: Public expenditure in employment incentives as a share of GDP, Italy vs. EU27



Source: Eurostat data. Note: missing EU27 data in 2011, 2021 and 2022.

Figure A2: Yearly changes of workers with open-ended contracts, Piedmont (CO-ASIA data) and Italy (INPS data)



Source: own computations on CO-ASIA and INPS data from "Osservatorio INPS dipendenti privati non agricoli" (https://servizi2.inps.it/servizi/osservatoristatistici/15/32/39). Note: (a) private non-agricultural sectors only; (b) both series exclude apprentices and include transformations from other types of contracts; (c) Italy: left-hand scale; Piedmont; right-hand scale.

Appendix B: Propensity score matching

This appendix is devoted to present the main diagnostics from our matching exercise. There is no need here to split the reasoning by job and employment security, as in both cases what is matched is the sampled open-ended job, possibly giving rise to an uninterrupted sequence of employment relationships in the case of employment security. In the following we will therefore abstract from this distinction.

As anticipated in the main text, we use nearest-neighbour propensity score matching, with no replacement and a caliper set at 0.4. The combination of a large caliper and no replacement aims at preserving the pre-intervention distribution, without relying – as a potential implication in case of replacement – on an excessively tiny share of the post-intervention sample, where each observation would be paired to several pre-intervention units. The use of the nearest neighbour is then justified by the large sample size. Nearest neighbour has the advantage of having the lowest bias for all sample sizes, although carrying the disadvantage of higher variance estimates (Huber et al., 2013). As the sample size grows, however, the low-bias property becomes dominant.

The match is done separately for eligible and non-eligible job spells. The eligibility status represents indeed a major determinant of the outcome and captures a substantial divide within our sample, so that in this respect we preferred doing an exact matching. The variable set where we run a probabilistic matching is anyway huge, and includes gender, age (linear in years), one-digit sector dummies, weekly hours, average firm size in the six months preceding the matched jobs, month when the job starts (to capture seasonality), province (in Piedmont there are eight of them), education (with a dummy for tertiary education graduates), occupation (with a dummy for white-collar workers), a dummy that captures whether the new open-ended contract is an upgrade from a temporary contract with the same employer, and nationality (with a dummy for non-nationals); moreover, with reference to the two years before the sampled job: number of employed months, of employed months with an open-ended contract, of employed months in manufacturing, of employed months with a part-time job, of employed months in a high-skill occupation (meaning ISCO code lower than four), number of employment relationships.

For the eligible spells, nearly 100% of the observations sampled in 2010 is matched, as only 19 jobs out of 62,737 fall off the support. This brings to drop 36,161 observations from the post-intervention period, which therefore will not enter the matched sample. Matching leads to a substantial reduction of the mean (median) percentage bias from 8.7 (6.0) to 1.6 (1.3), well below the recommended threshold suggested in Caliendo and Kopeinig (2008). Also, both the Rubin B (13.7%) and R (1.22) tests display values within thresholds suggesting good overall balance. Figure B1 (upper panel) shows instead balancing performance variable by variable, in terms of percentage bias before (bold dots) and after (the triangles) matching. For each variable in the matching set the residual percentage bias is below 5%, but for weekly hours (5.8%). We are therefore reassured that, in spite of the large caliper, balancing is good.

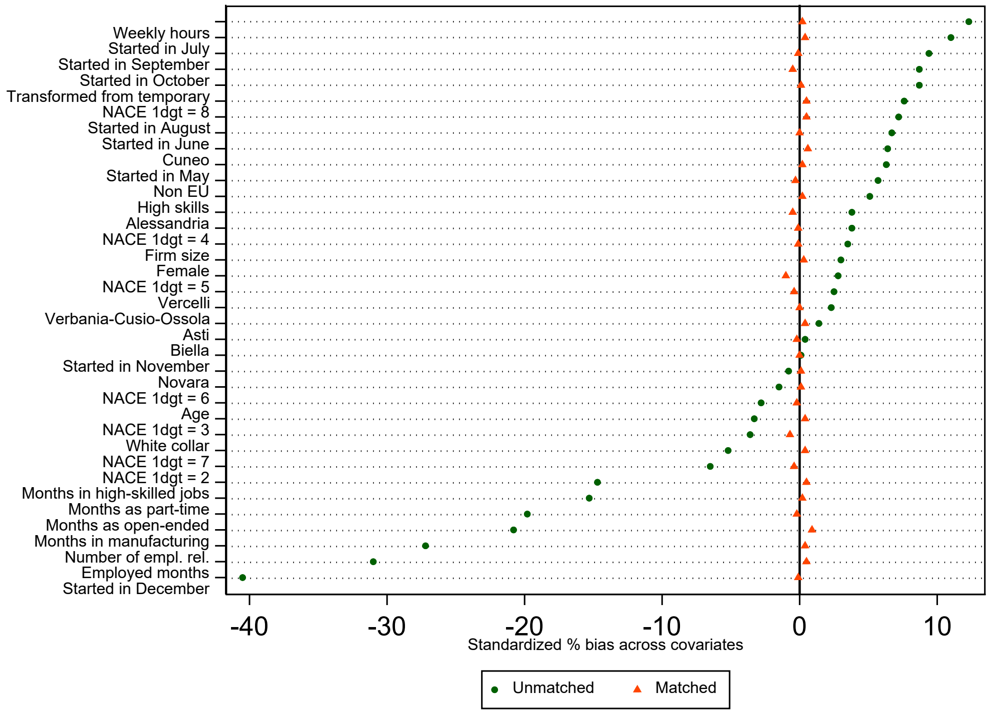
For the non-eligible sub-sample the narrative is partially different, in particular for the common support. Out of the 18,394 open-ended jobs sampled in 2010, only 15,808 (86%) find a twin among the 22,826 episodes sampled in April-December 2015. Balancing is anyway comparable to the eligible cases, as the mean (median) percentage bias falls from 8.5 (5.1) to 1.9 (1.1). Analogously, the Rubin B (R) test reads 16.1% (0.76). Variables where the percentage bias after matching is still higher than 5% in absolute terms are the total number of employed months (12.1%), the total number of months under an open-ended contract (7.8%) and the number of employment relationships (6.9%) (Figure B1, lower panel).

To check the robustness of the estimate results to alternative matching choices, we have also tried with a much narrower caliper (namely set at 0.001), which is expected to further reduce the residual bias, and by allowing replacement, as a way to better preserve the common support. Among the eligible workers, only 46 (out of 62,737 and compared to 19 in our most preferred setting) lay out of the common support. As expected, mean (median) percentage bias after match is even lower, namely 0.6 (0.5). The Rubin B and R tests read 5.0% and 1.01, again in the safe region. When assessed variable by variable, the surviving percentage bias is always below 3%. Differently from the no-replacement/large-caliper setting above, among the non-eligible, nearly 100% of the observations fall within the common support. Mean and median percentage bias on the matched sample are 2.1 and 1.7 respectively, while Rubin B- and R-tests read 16.2 and 0.88. Variable-specific percentage bias remains however high (compared to the recommended standards) for open-ended contracts originating from a temporary one (6.6), months spent under an open-ended contract (7.3), and the number of past employment relationships (9.1). Reassuringly, impact estimate results (available upon request to the authors) are not affected by this alternative matching strategy, suggesting that our arguments in the main text do not depend on matching choices.

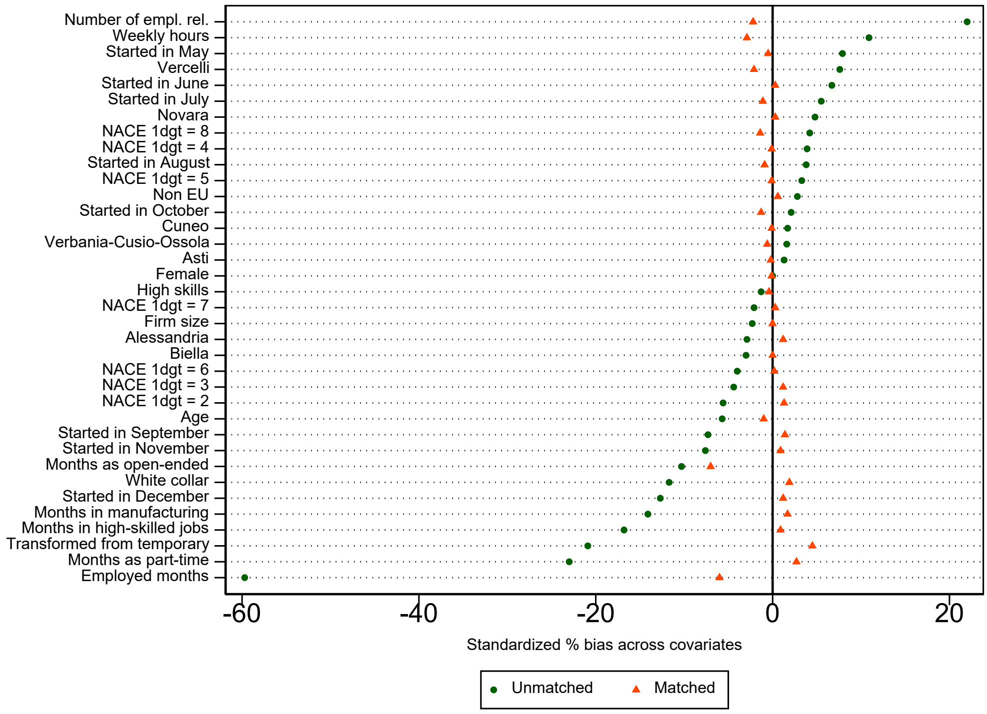
A last thought is about other possible ways to control the potential activation effect of the policy being evaluated here. A functional alternative to the matching approach would be for instance to shrink the sample around the eligibility threshold, e.g. by comparing workers who had the last open-ended experience 3-to-6 months before the sampled spell as the non-eligible units, with workers who had the last open-ended job 7-to-9 months earlier as eligible sub-sample. This strategy would indeed mechanically exclude those workers who decide to step (back) into the labour market as a mere consequence of the introduction of the hiring incentives. We deem however matching superior to this sample selection. Indeed, while matching preserves the distribution of the observables over all eligible and non-eligible workers, the shrinking strategy focuses on the worst non-eligible workers vs. the best eligible ones, with a clear risk of (over)underestimating the (positive) negative effects of the policy. This is confirmed by our investigations using the "shrinking" strategy, the results of which are again available upon request to the authors.

Figure B1: Sample balancing with our most preferred matching strategy (no replacement, caliper at 0.4).

Panel 1: Eligible



Panel 2: Non-eligible



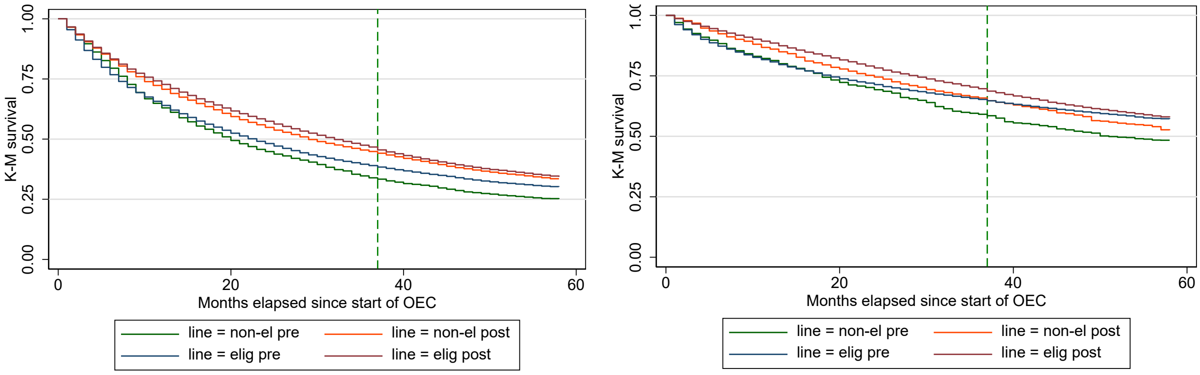
Source: own computations on CO-ASIA data.

Appendix C: Survival analysis

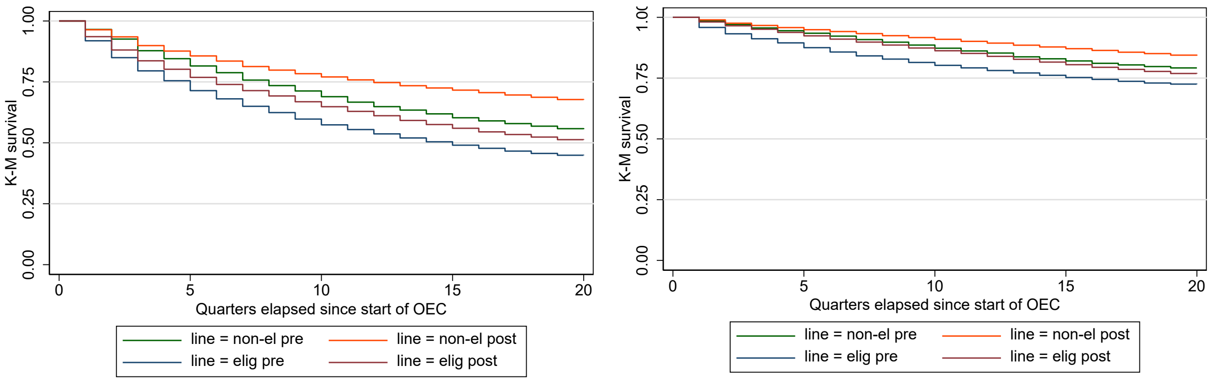
Figure C1: Kaplan-Meyer Survival rate by period and eligibility.

Small firms (up to 15 employees)	Large firms (over 15 employees)

Job security



Employment security

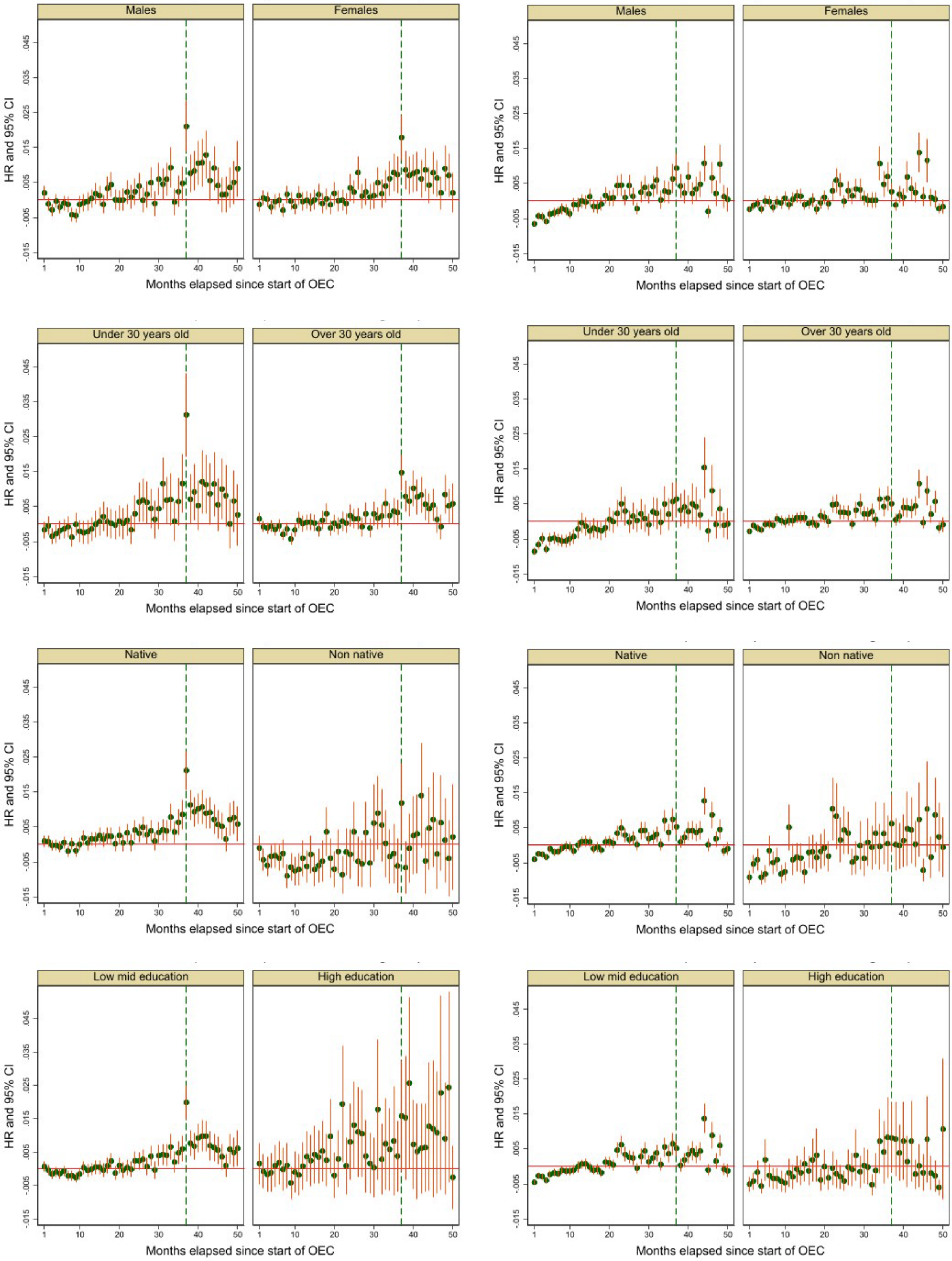


Source: own computations on CO-ASIA data.

Appendix D: Heterogeneity analysis

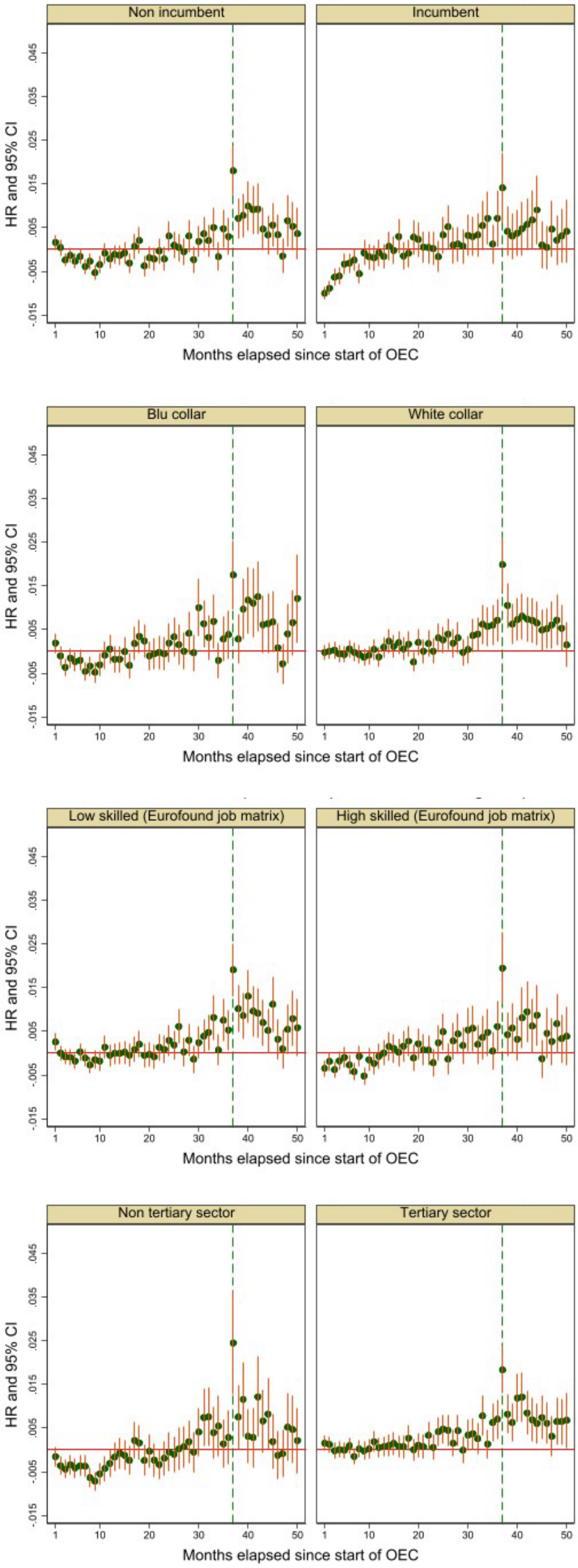
Figure D1. HR of Job Separation, stratified by different segment of workforce (NL DID estimates)

Column A: Small firms **Column B: Large firms**

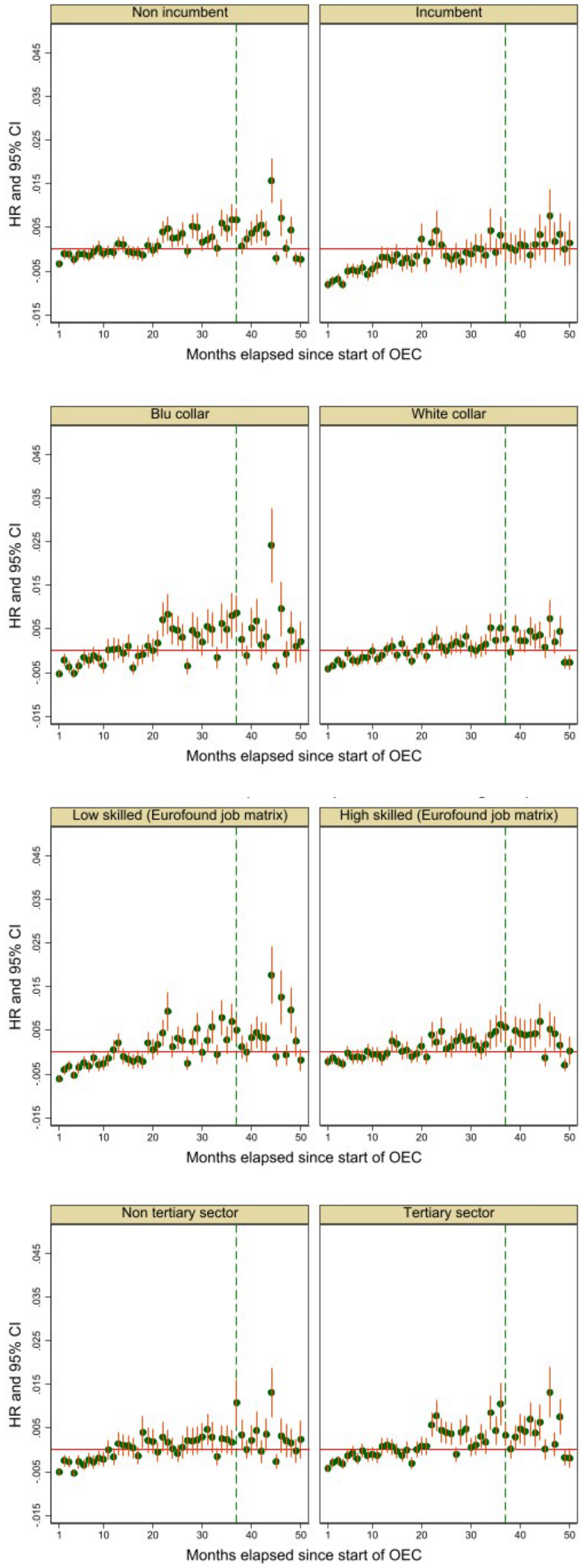


Cont. Fig. A1: *HR of Job Separation*

A: Small firms

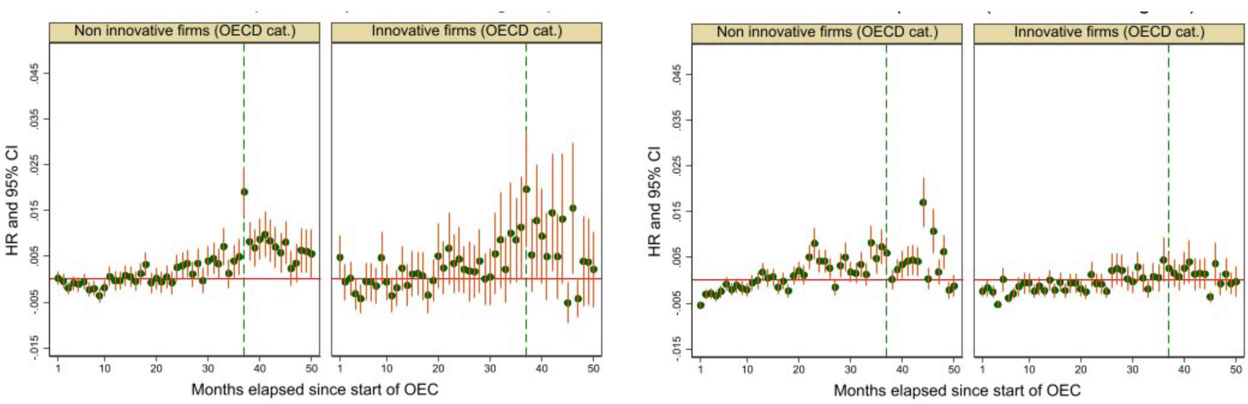


B: Large firms



Cont. Fig. D1: *HR of Job Separation*

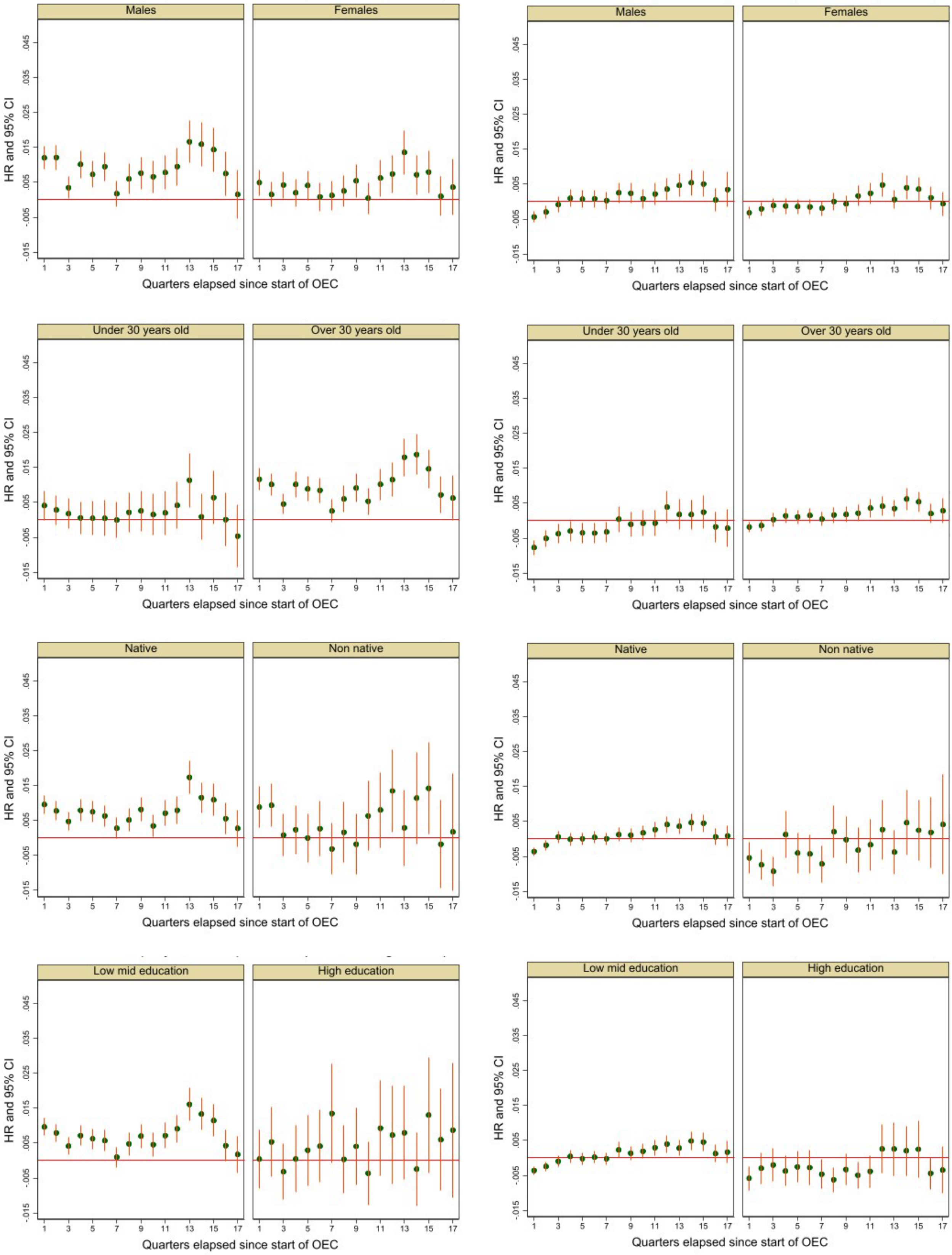
A: Small firms **B: Large firms**



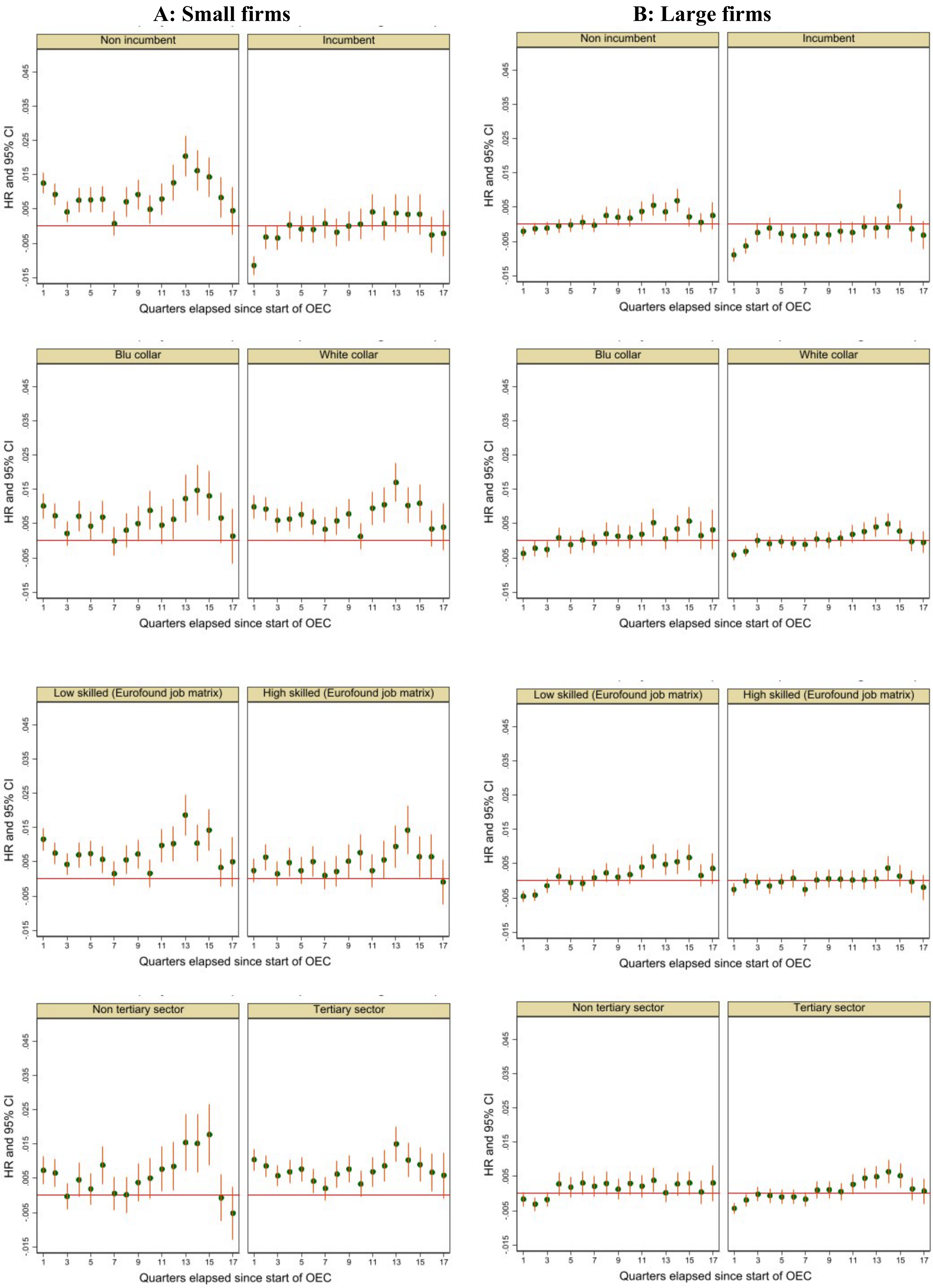
Source: own computations on CO-ASIA data. Notes: marginal effects are estimated with our main model non-linear did equation.

Figure D2. HR of Employment Separation, by different segment of workforce (NL DID estimates)

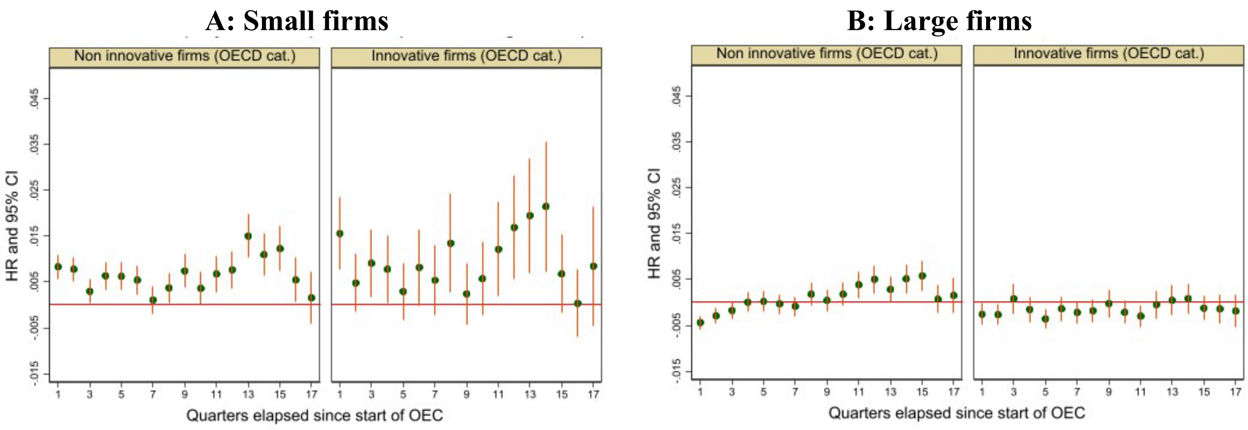
Column A: Small firms **Column B: Large firms**



Cont. Fig. D2: *HR of Employment Separation*



__Cont. Fig. D2:__ HR of Employment Separation



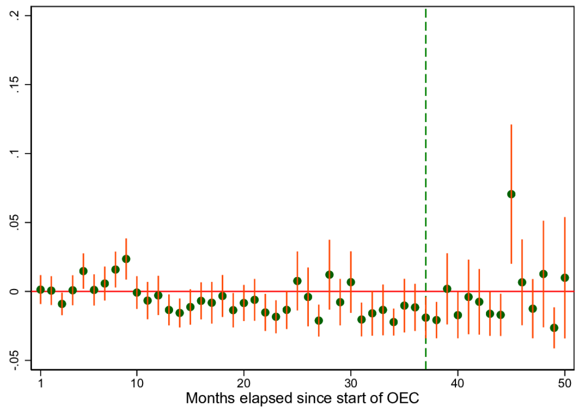
Source: own computations on CO-ASIA data. Notes: marginal effects are estimated with our main model non-linear did equation.

Appendix E: Placebo analysis

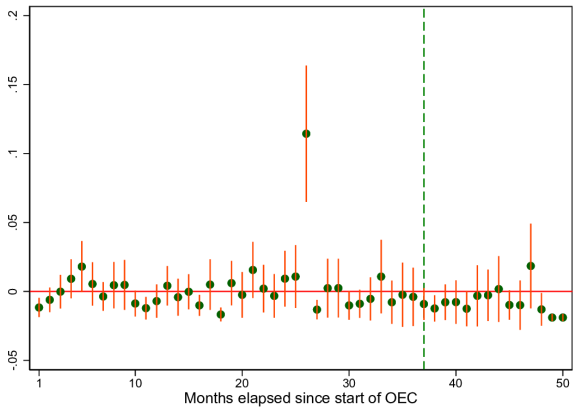
Figure E1: Placebo analysis on Job security – Non-eligible spells: 0-3 vs 4-6 months since last open-ended contract

Panel 1: Full sample (Non-linear DID estimates)

1a: Small firms (full sample)

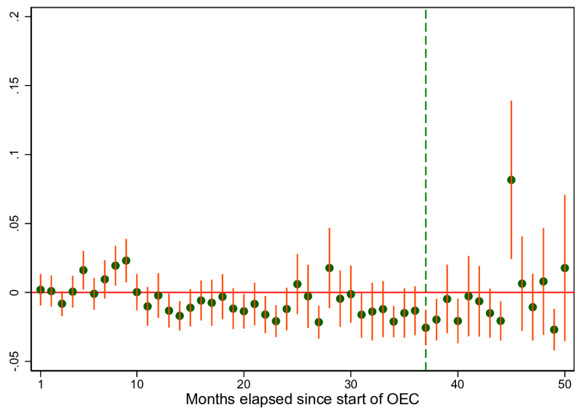


1b: Large firms (full sample)

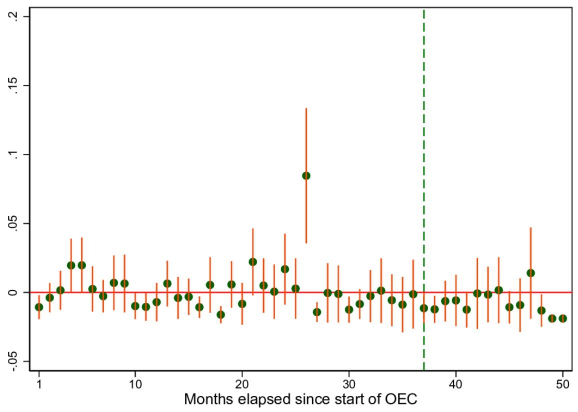


Panel 2: PSM matched sample (Non-linear DID estimates)

2a: Small firms (PSM sample)



2b: Large firms (PSM sample)

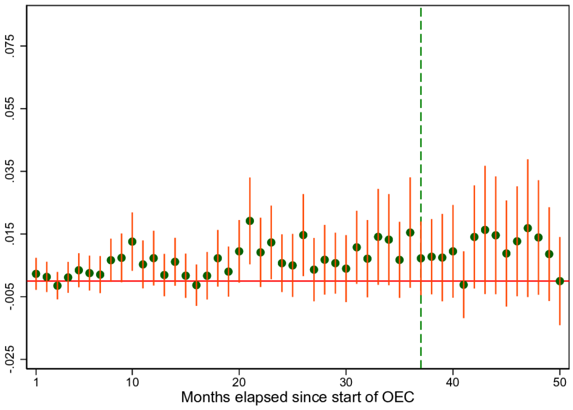


Source: own computations on CO-ASIA data.

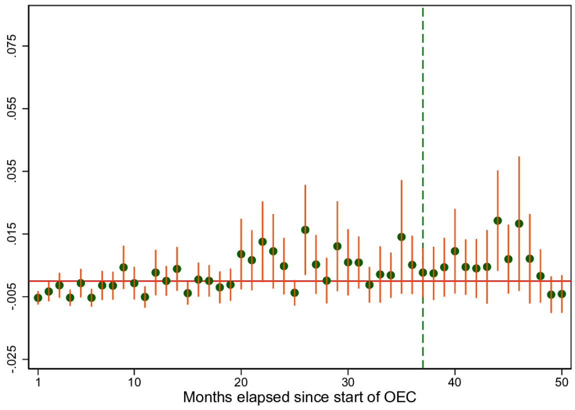
Figure E2: Placebo analysis on Job security – Eligible spells: 7-12 vs 13-24 months since last open-ended contract

Panel 1: Full sample (Non-linear DID estimates)

1a: Small firms (full sample)

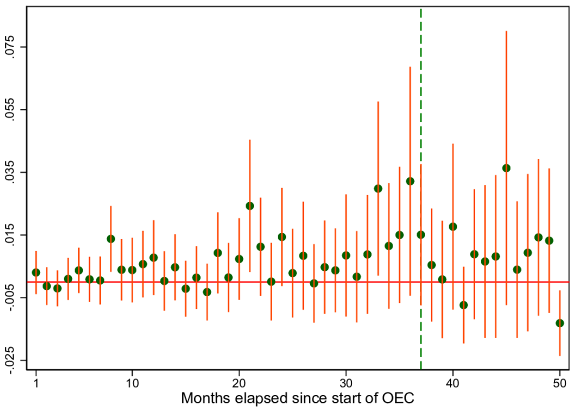


1b: Large firms (full sample)

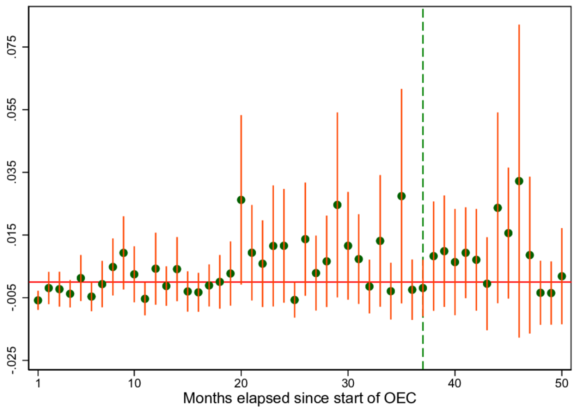


Panel 2: PSM matched sample (Non-linear DID estimates)

2a: Small firms (PSM sample)



2b: Large firms (PSM sample)

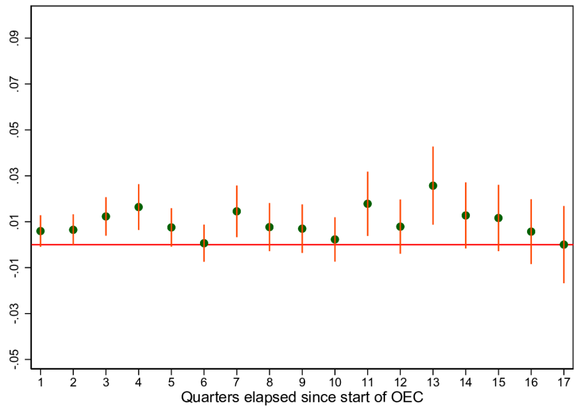


Source: own computations on CO-ASIA data.

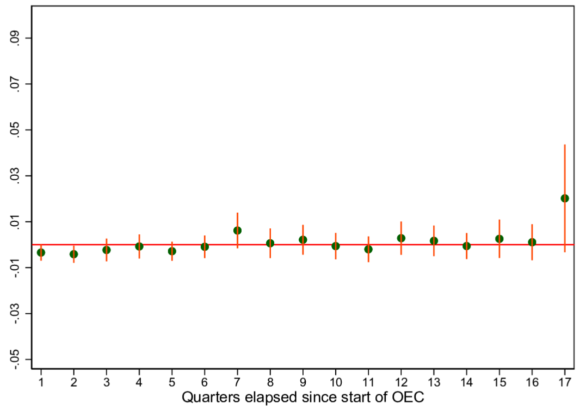
Figure E3: Placebo analysis on Employment security – Non-eligible spells spells: 0-3 vs 4-6 months since last open-ended contract

Panel 1: Full sample (Non-linear DID estimates)

1a: Small firms (full sample)

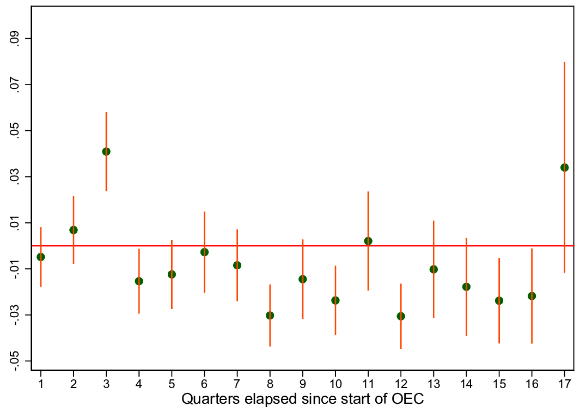


1b: Large firms (full sample)

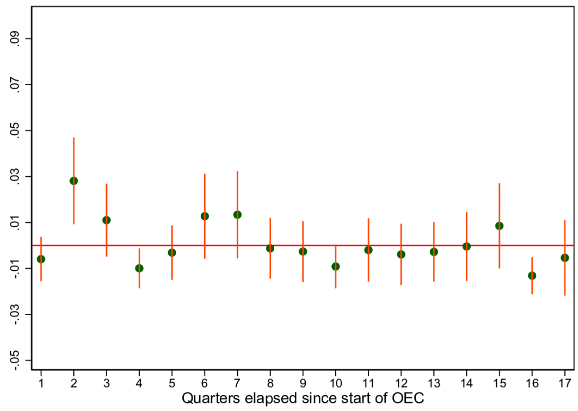


Panel 2: PSM matched sample (Non-linear DID estimates)

2a: Small firms (PSM sample)



2b: Large firms (PSM sample)

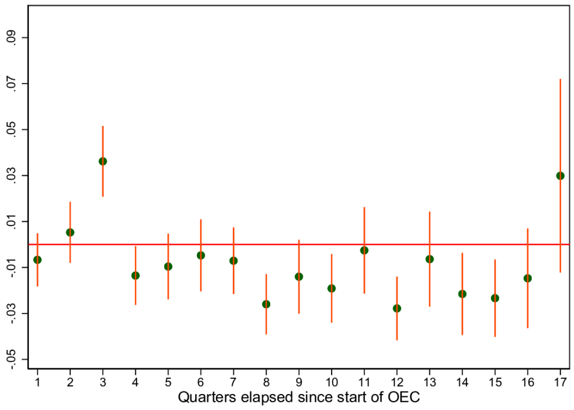


Source: own computations on CO-ASIA data.

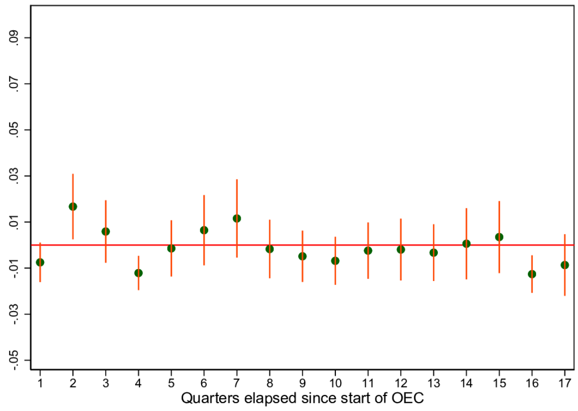
Figure E4: Placebo analysis on Employment security – Eligible spells: 7-12 vs 13-24 months since last open-ended contract

Panel 1: Full sample (Non-linear DID estimates)

1a: Small firms (full sample)

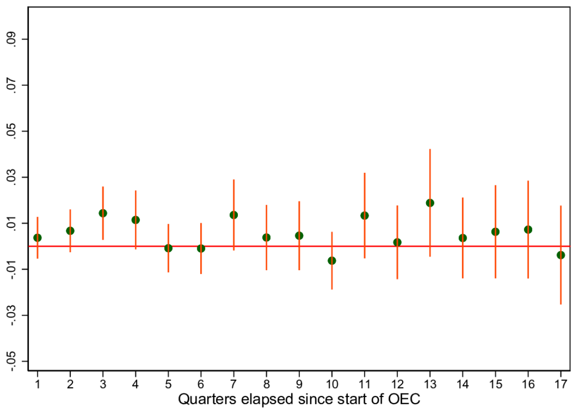


1b: Large firms (full sample)

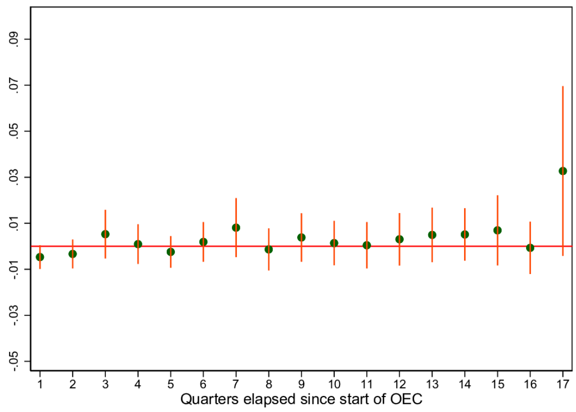


Panel 2: PSM matched sample (Non-linear DID estimates)

2a: Small firms (PSM sample)



2b: Large firms (PSM sample)



Source: own computations on CO-ASIA data.

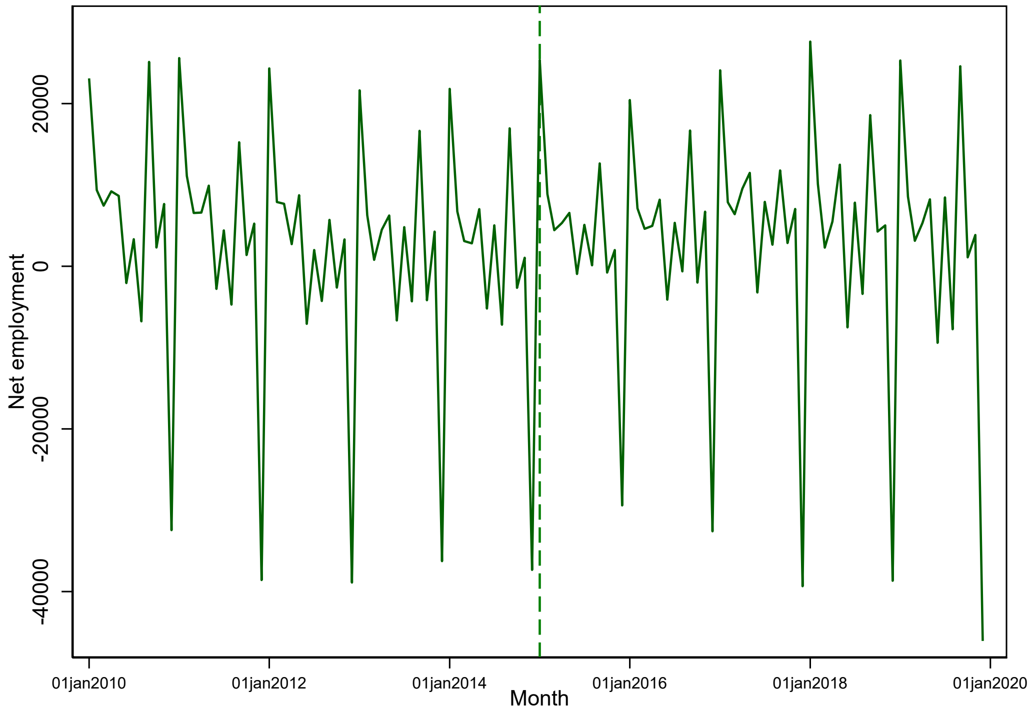
Appendix F: Macroeconomic effects

The aim of this “back of the envelope” exercise is to estimate whether net changes in the stock of employees increased after the subsidy had been financed. To do so we use the administrative archive of the Comunicazioni Obbligatorie (CO) of Piedmont Region to compute 2010-2019 employment in- and out-flows, aggregated at month level. We include hires and separations of any kind of contract observed in any sector in Piedmont, expressed as full-time equivalent (FTE) units. We use the difference between total hires and separations in order to calculate the net employment flow at each time month=1, ...,120. We set month = 1 at January 2010.

We then estimate an ARIMA model, to see whether there is a break in the series after January 2015. This approach, on top of including spillover effects on other types of contracts – a trade-off between temporary and permanent employment may indeed exist – also carries the (supposedly much weaker) effects of the incentives funded under the 2016 budget law. As suggested by its name, ARIMA model has multiple components: (i) autoregression (AR): the variable of interest (y) is regressed on its own past values (lags); (ii) integrated (I): observations can be differenced in order to make the time series stationary; (iii) moving average (MA): regresses the current value of y on past observed white noise error terms.

In order for ARIMA to estimate the parameters, the time series needs to be stationary until the shock happens. That is, the process has to have no trend (constant mean) and its variations around the mean should always have the same width (constant variance). If this holds, autocorrelation is constant over time. When dealing with employment flows, fluctuations and sign changes are expected; we observe a very strong seasonal component where some months tend to have very high or low (negative) net flows. Figure F1 below shows the observed net flows from the 1st of January 2010 to the 31st of December 2019. The phenomenon looks very stationary in the pre-reform period as both its mean and variability do not change visibly from 2010 to 2014. It also shows a very pronounced seasonal component, as expected. The final section of this appendix shows the tests supporting these statements.

Figure F1: Monthly net flows from January 2010 to December 2019



Source: own computations on Piedmont CO data, full sample 2010-2019.

The policy impact estimate is based on the hypothesis that, given a stationary phenomenon, every difference from the period 2010-2014 to the period 2015-2019 is due to the policy. The ARIMA model is implemented with two different policy-impact variables: *step* and *ramp*; it includes autoregressive and moving average components as well, which in this analysis should be considered as control variables. Each net flow is differentiated with its 12-lagged value (value of the same month the year before). This allows a better fit with seasonal data but leads to a loss of power since the whole first year cannot be included in the model ($t = 1,...,108$, while $month = 1,..., 120$). The described model can be written as:

$$Y_t = c + \Phi Y_{t-1} + \beta_1 Step + \beta_2 Ramp + \varepsilon_t + \Theta\varepsilon_{t-1} + \Theta\varepsilon_{t-2} \qquad \text{(Eq. E1)}$$

where:

ΦY_{t-1}= AR(1), autoregressive variable with lag = 1

$\Theta\varepsilon_{t-1}$= MA(1), moving average variables with lag = 1

$\Theta\varepsilon_{t-2}$= MA(2), moving average variables with lag = 2

ε_t = error term

step = binary variable for post reform period, i.e. equal to 0 if $t \leq 60$ (December 2014), else *step* = 1

ramp = linear time variable for post reform period, i.e. equal to 0 if $t \leq 60$ (December 2014), else *ramp* = t - 60

Table F1: Results from ARIMA model estimating: Y = employment net flow (Equation. E1)

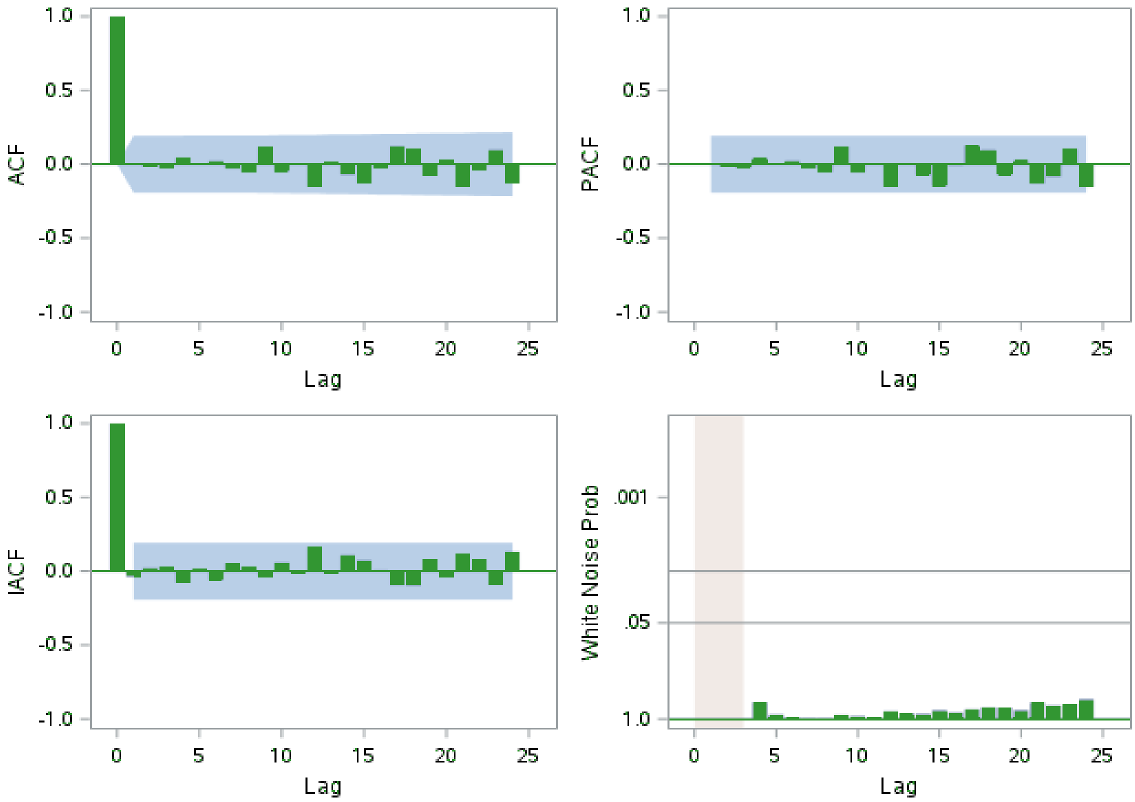
Parameter	Lag	Estimate	Std error	Pr > |t|
Employment net flow moving average (MA1)	1	1.02809	0.14833	<.0001
Employment net flow moving average (MA2)	2	-0.36524	0.09656	0.0002
Employment net flow autoregressive factor (AR1)	1	0.80055	0.13452	<.0001
Step	0	2518.6	1213.2	0.0379
Ramp	0	-73.3403	34.38338	0.0329

Source: own computations on Piedmont CO data, full sample 2010-2019.

Model Diagnostics and Stationarity tests

This section presents a series of diagnostic tests designed to validate the underlying assumptions of our ARIMA models, ensuring the reliability of our estimates and findings. Figure F2 presents the residuals' diagnostics, including the Autocorrelation Function (ACF), Partial Autocorrelation Function (PACF), Inverse Autocorrelation Function (IACF), and White Noise probability. The autocorrelation functions allow us to evaluate whether residuals show any remaining autocorrelation, which would indicate model misspecification. The White Noise probability assesses whether the residuals look like a purely random process, confirming the absence of systematic patterns. The diagnostic plots reveal no significant autocorrelation or structure in the residuals. This suggests that the chosen model adequately captures the underlying dynamics of the series.

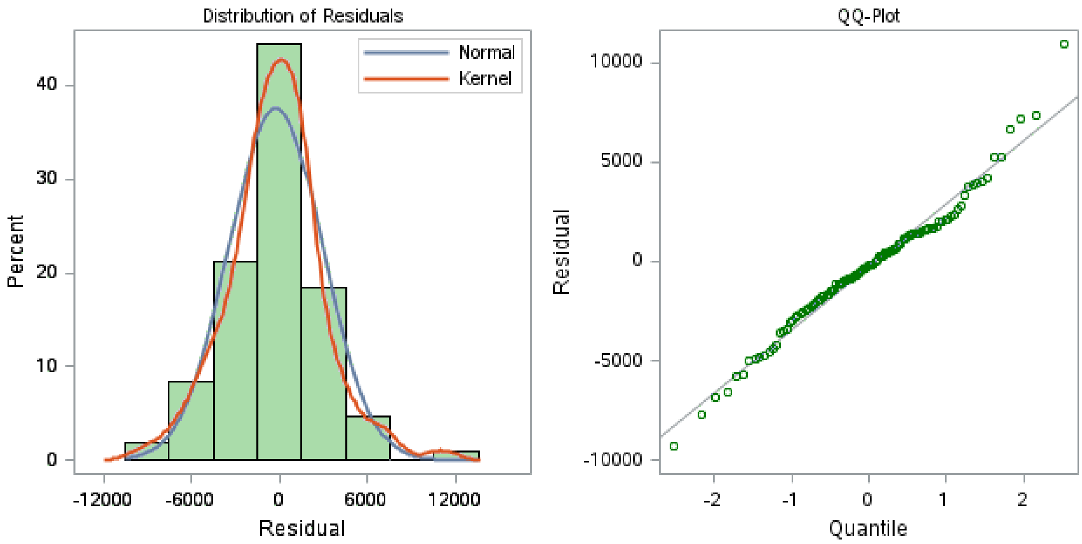
Figure F2: Residuals' diagnostics



Source: own computations on Piedmont CO data, full sample 2010-2019.

Figure F3 allows us to check the normality of the residuals. Specifically, on the left, we have a density plot, and on the right, a quintile-quintile (QQ) plot. In the density plot, the blue curve represents a normal distribution for residuals while the red curve represents the observed distribution of our residuals. As the two curves are similar we can conclude that the residuals are normally distributed. In the QQ plot, the diagonal black line represents the case where residuals are normal (cumulative distribution of residuals and cumulative normal distribution are the same). The blue dots represent the observed residuals quantiles against normal quantiles. Again, the figure leads to the conclusion that residuals are normal, as the dots approximately lie on the identity line.

Figure F3: Normality of residuals and other diagnostics



Source: own computations on Piedmont CO data, full sample 2010-2019.

The exercise in table F2 shows the results of an autocorrelation test on the residuals of the ARIMA model. The test's goal is to determine whether residuals show any pattern. If the residuals are highly autocorrelated then the model fails in capturing some of the variability and therefore has to be considered incorrectly specified. The autocorrelation values displayed in Table F2, indicate that autocorrelation at different lags is close to zero in most of the cases. This confirms that there are no systematic patterns left in the residuals, further validating the model's adequacy.

Table F2: Autocorrelation Check of Residuals

Lags	Chi-Square	DF	Pr > Chi-Sq.	Lags' Autocorrelations					
1-6	0.64	3	0.8872	0.021	-0.036	0.01	0.064	0.004	-0.018
7-12	8.6	9	0.4754	-0.032	0.019	0.136	-0.071	-0.028	-0.215
13-18	14.91	15	0.4578	0.02	-0.082	-0.041	-0.041	0.15	0.142
19-24	21.16	21	0.4494	-0.073	-0.042	-0.131	0.009	0.081	-0.135

Source: own computations on Piedmont CO data, full sample 2010-2019.

We finally tested the stationarity of the time series with two different methods, the Dickey-Fuller and Phillips-Peron tests. Both test the null hypothesis that the series is non-stationary vs. that it is stationary. Each test relies on different test statistics: rho (normalized bias test), tau (studentized test) and F (joint test for unit root, only for Dickey-Fuller tests). The Phillips-Peron test additionally accounts for heteroskedasticity, allowing the variance to change over time. The tests are performed on the variable lagged by 12 months (the same we include in the ARIMA model). Stationarity is tested in three different cases: zero-mean stationarity (zero mean), nonzero-mean stationarity (single mean), and linear time trend stationarity (trend). As shown in Table F3, both Dickey-Fuller (Panel A) and Phillips-Perron (Panel B) lead us to reject the null hypothesis and assume that the series is stationary.

As mentioned above, the series only needs to be stationary before the shock. However, we also performed the exact same test over the entire period analyzed. Interestingly, the tests allow to reject the null hypothesis both for the pre-reform period (2010-2014, Table F4 Panel A) and for the entire period (2010-2019, Table F4 Panel B), i.e. including the post-reform period. This already suggests that the introduction of the hiring incentives in 2015 didn't result in any striking shock.

Table F3: Non-stationarity tests: unit root tests performed on the pre-reform period (2010-2014)

Panel A: Augmented Dickey-Fuller test							
Type	**Lags**	**Rho**	**Pr < Rho**	**Tau**	**Pr < Tau**	**F**	**Pr > F**
Zero Mean	**0**	-42.6914	<.0001	-6.21	<.0001		
	1	-23.5462	0.0002	-3.36	0.0012		
	2	-10.3413	0.0212	-2.07	0.0384		
Single Mean	**0**	-45.9399	0.0004	-6.63	0.0001	22	0.001
	1	-28.7372	0.0004	-3.75	0.0062	7.06	0.0011
	2	-13.6395	0.0405	-2.37	0.1554	2.84	0.3695
Trend	**0**	-48.0493	<.0001	-6.92	<.0001	23.96	0.001
	1	-34.3586	0.0004	-4.12	0.0114	8.53	0.0135
	2	-18.3249	0.061	-2.74	0.2261	3.79	0.444

Panel B: Phillips-Perron test					
Type	**Lags**	**Rho**	**Pr < Rho**	**Tau**	**Pr < Tau**
Zero Mean	**0**	-42.6914	<.0001	-6.21	<.0001
	1	-41.943	<.0001	-6.21	<.0001
	2	-45.6364	<.0001	-6.26	<.0001
Single Mean	**0**	-45.9399	0.0004	-6.63	0.0001
	1	-45.4176	0.0004	-6.63	0.0001
	2	-48.711	0.0004	-6.65	0.0001
Trend	**0**	-48.0493	<.0001	-6.92	<.0001
	1	-47.2331	<.0001	-6.92	<.0001
	2	-49.5446	<.0001	-6.91	<.0001

Source: own computations on Piedmont CO data, full sample 2010-2014.

Table F4: Non-stationarity tests: unit root tests performed on the entire period (2010-2019)

Panel A: Augmented Dickey-Fuller test

Type	Lags	Rho	Pr < Rho	Tau	Pr < Tau	F	Pr > F
Zero Mean	**0**	-117.79	0.0001	-11.18	<.0001		
	1	-77.6651	<.0001	-6.04	<.0001		
	2	-44.7466	<.0001	-4.06	<.0001		
Single Mean	**0**	-118.397	0.0001	-11.21	<.0001	62.85	0.001
	1	-79.1243	0.001	-6.09	<.0001	18.57	0.001
	2	-46.0582	0.001	-4.12	0.0014	8.53	0.001
Trend	**0**	-119.608	0.0001	-11.26	<.0001	63.39	0.001
	1	-82.1833	0.0004	-6.15	<.0001	18.92	0.001
	2	-48.6534	0.0004	-4.14	0.0075	8.64	0.001

Panel B: Phillips-Perron test

Type	Lags	Rho	Pr < Rho	Tau	Pr < Tau
Zero Mean	**0**	-117.79	0.0001	-11.18	<.0001
	1	-118.658	0.0001	-11.17	<.0001
	2	-126.749	0.0001	-11.16	<.0001
Single Mean	**0**	-118.397	0.0001	-11.21	<.0001
	1	-119.227	0.0001	-11.2	<.0001
	2	-127.1	0.0001	-11.18	<.0001
Trend	**0**	-119.608	0.0001	-11.26	<.0001
	1	-120.329	0.0001	-11.25	<.0001
	2	-127.701	0.0001	-11.23	<.0001

Source: own computations on Piedmont CO data, full sample 2010-2019.

Appendix References

Caliendo, M. and Kopeinig, S. (2008) Some practical guidance for the implementation of propensity score matching, Journal of Economic Surveys, 22(1): 31-72.

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