

Targeted wage subsidies and firm performance^{*}

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May 11, 2017

Abstract

This paper studies how targeted wage subsidies affect the performance of the recruiting firms. Using Swedish administrative data from the period 1998-2006, we show that treated firms substantially outperforms other recruiting firms after hiring through subsidies, despite identical pre-treatment performance trends in wide set of key dimensions. These positive effects completely disappear after 2007. We attribute this change to a policy reform that removed the involvement of caseworkers from the subsidy approval process. Overall, our results suggest that targeted employment subsidies can have large positive effects on post-match outcomes of the hiring firms, but only if the policy environment allows for pre-screening by caseworkers.

Keywords: wage subsidies, labor demand, firms performance

JEL classification: J08, J2, J6

^{*}We are grateful for helpful suggestions from Anders Forslund and Francis Kramarz.

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1 Introduction

Targeted wage subsidies that reduce parts of the wage costs for private firms hiring unemployed workers are an integral part of active labor market policies (ALMP) in most Western countries (Card et al. 2010, 2015; Kluve 2010). The main objective is to help low-skilled workers find jobs, but a key concern is that the subsidies will crowd out other hires within the same firms. Furthermore, a key aspect of these subsidies that sets them apart from other ALMPs is that they are likely to directly affect the allocation of workers across firms, an issue that has received a lot of recent attention within labor economics (see e.g. Card et al, 2013, and Song et al., 2016). Yet, there exists very little evidence on how targeted wage subsidies affect the sorting process and/or key firm-level outcomes. In this paper, we make three distinct additions to the empirical literature; we study how subsidies affect the selection of workers into various types of firms, we study the impact of the subsidies on firm-level outcomes, and we show how selection and causal firm-level effects depend on the degree of caseworker discretion when subsidies are allocated.

Our analysis uses detailed Swedish administrative data on workers and firms to study targeted wage subsidies under two very different policy regimes. Between 1998 and 2006 all targeted wage subsidies in Sweden needed to be approved by a caseworker at the public employment office. The caseworkers could also propose suitable employer-employee matches (see e.g. Lundin, 2000). This staff-selection scheme is contrasted to a new system introduced in 2007, which granted all employers that hired a long-term unemployed worker the right to receive a wage subsidy, thus substantially reducing the role of caseworkers in the allocation of the subsidies.

To study the selection and the impact of the recruiting firms, we use spell data on unemployed workers and the subsidies they receive. This data is linked to matched employer-employee data which allows us to follow all workers and their employing firms over time. Data from business registers provides information on profits, sales, wage sums, value added and investments for the same firms.

Our analysis compares firms recruiting through subsidies (defined as treated) to other observably identical firms. We focus on small and medium sized firms throughout. For the causal analysis, we compare treated firms to firms that hire unemployed workers without using the subsidy. We adjust for pre-existing differences in firm-size and average worker's characteristics through matching on observable pre-treatment levels in these dimensions and show that treated and matched controls have identical

pre-treatment trends (which we do not match on) after matching. Furthermore, both pre-treatment trends and levels are remarkably similar in key dimensions that we do *not* match on, (most notably wage sums, productivity and profits).

Our main finding is that during the staff-selection regime treated firms substantially outperforms the comparison firms after treatment in terms of the number of employees, and in terms of various production measures. This pattern is persistent and it does not come at the cost of decreased productivity per worker. However, these results only hold when the targeted wage subsidies are allocated through caseworker selection. In the rules-selection regime, when subsidies are available to all firms that hire workers with a sufficiently long unemployment duration, we find no effects on firm size and productivity measures. Thus, during the staff-selection regime the subsidies only leads to partial displacement of non-subsidized jobs, while during the rules-selection regime, each subsidized job displaces one non-subsidized job.

Using data from business registers we also examine how the subsidies affects profits. For both regimes we find tendencies towards increased profits as a result of the subsidized jobs. For the rules-selection regime, we find effects on profits simply because the the subsidies mechanically reduced the costs of labor.

We also use detailed data to explore the possible mechanisms behind the observed differences between the two regimes. The main difference between the two regimes is caseworkers play an important role in the allocation of the subsidies during the staff-selection regime, while they have a limited role during the rules-selection regime. We, therefore, examine the extent to which they allocate the targeted wage subsidies to different types of firms in different sectors, or allocate the subsidies to other types of unemployed workers. We find no evidence to this effect. Another possible explanation for the difference between the two policy regimes is that caseworkers to a larger extent select firms that use the subsidies to expand. In order to investigate this possibility, we study how the targeted subsidies relate to emphinvestments made by the firms in the two regimes, but again without any evidence to this effect. A final, residual, hypothesis is that caseworkers improve the quality of the match between firms and workers, to the benefit of the firms involved in the targeted wage subsidy scheme.

Our paper is related to several strands of the existing literature. Several previous studies have examined displacement effects of active labor market policy programs. Using experimental variation Crépon et al. (2013) document substantial displacement effects from a job placement assistance program in France. In this paper, we are able

to investigate a different type of displacement effect. Crépon et al. (2013) study if job search assistance displaces employment for non-treated unemployed in the same area, while in this paper we study displacement of employment at firms targeted with wage subsidies. Some previous studies examine displacement at the firm level. Kangasharju (2007) uses Finnish data that links firms and workers, and finds that employment subsidies in Finland increased the firm’s payroll by more than the size of the subsidy.¹

Other papers study spillover effects at the market level. These include, for instance, Blundell et al. (2004), Lise et al. (2004), Ferraci et al. (2013), Pallais (2014), Gautier et al. (2015) and Lalive et al. (2015). These studies either use geographical variation and/or theoretical models to study spillover effects at a more general level, including market equilibrium effects. Here, we focus on allocation workers across firms and on how targeted wage subsidies affects firm performance.

Several studies examine how such subsidized wage affects the unemployed workers covered by the wage subsidies (see survey evidence in e.g. Card et al. 2010, 2015; Kluve 2010). While this provides valuable information, our results show that we can learn more about the impact of wage subsidies by focusing on the private firms that take part in the subsidy schemes and by focusing on how workers are allocated across these firms.²

Two existing studies examine how active labor market programs affect firm behavior and firm level outcomes. Blasco and Pertold-Gebicka (2013) study a large scale randomized experiment on the effects of counseling and monitoring. By comparing firms in Danish areas where the experiment was conducted with firms in other areas, they find that firms in treated areas hire to a larger extent unemployed workers, but these firms also experience greater turnover. Lechner et al. (2013) exploit that German local employment offices determine the mix of ALMPs, which allows them to compare firms operating in different labor markets with different ALMP mixes, and find that in general firms do not benefit from ALMP programs. In this paper, we use data that

¹Other studies on displacement effects include studies that have used surveys of employers to study displacement. For instance, Bishop and Montgomery (1993) survey more than 3500 private employers in US and conclude that at least 70% of the tax credits granted employers are payments for workers who would have been hired even without the subsidy. In a similar vein, Calmfors et al. (2002) discuss Swedish survey evidence.

²Andersson et al. (2016) evaluate a *training* program in the U.S. and include various measures of firm quality as outcomes. These measures include firm size, firm turnover as well as firm-effects defined as in Abowd, Kramarz and Margolis (1999). Overall they find modest effects on the quality of the firms where the formerly unemployed workers find jobs. The effects of subsidies may very well be different, however.

links firms and workers to study firms that actually use subsidized jobs, whereas these two previous studies focus on effects on all firms in a certain area.

The paper proceeds as follows. In section 2, we discuss the relevant institutions. Section 3 presents the data. Section 4 explores sorting and presents the matching strategy. Section 5 presents the main results and section 6 assesses the underlying mechanisms. Section 7 concludes.

2 Institutional setting

2.1 Public employment policies in Sweden

The Swedish Public Employment Service (PES) is responsible for all aspects of Active Labor Market Policies (ALMPs). The overall aim is to contribute to a well-functioning labor market for both unemployed individuals and firms. The main focus is to provide targeted policy measures to the unemployed by using policy measures such as job search counseling, labor market training, targeted wage subsidies and practice programs. Another aim is to support firms in the recruitment process, in particular by providing a free and publicly available vacancy database. The PES is divided into 280 local public employment offices. Each unemployed individual is assigned to a caseworker at the local office, and caseworkers are responsible for providing policy programs to the people assigned to them.

2.2 Wage subsidies and two policy regimes

In this paper we focus on targeted wage subsidies. These subsidies target different sets of unemployed individuals and reimburses (parts of) the firms labor costs if they hire one of the eligible targeted individuals. The purpose is to provide firms with incentives to hire those that otherwise would struggle to find unsubsidized work.

We analyze two different subsidy schemes. The first, the Employment Subsidy Program (Anställningsstöd), was the main subsidy program in place between 1998 and 2006. The program was targeted *and* selective. It was targeted to individuals unemployed for at least 12 months and at least 20 years old.³ The program replaced 50 percent of the total wage costs (including payroll taxes) of the worker for a maximum

³The subsidy could, in some cases, be paid for people not long-term unemployed, provided that they had participated in other programs or had been temporarily employed.

duration of 6 months.⁴ The program was *selective* in the sense that each subsidized job had to be approved by a caseworker at the local PES office. The importance of caseworkers is confirmed by implementation surveys. Lundin (2000) showed that caseworkers initiate the subsidies in most cases, even though firms always have the opportunity to decline suggestions from the caseworker. In addition, Harkman (2002) shows that caseworkers have fairly strong, and varying, views on the appropriateness of different programs. Taken together, this suggests that firms access to the subsidy program to a large extent depended on the caseworkers. We therefore refer to this subsidy scheme as the *staff-selection scheme*.

The second scheme we study is the “New Start Jobs program” introduced in 2007. This program is targeted by not selective. Similar to the previous scheme, the target was individuals who had been unemployed for at least 12 months. But any worker who had been unemployed for at least 12 months during the last 15 months had the *right* to receive the subsidy if they found a job.⁵ The overall size of the subsidy is similar to the previous system. The New Start Jobs program has a slightly lower replacement rate but a longer duration. It replaces 31.42 percent of the wage cost for a time equal to the duration of unemployment (i.e. at least 12 months).

The main difference between the two policy regimes is that the staff-selection scheme involves caseworker selection, whereas the New Start Jobs program does not. Under the new scheme, firms employing an eligible individual have the right to the subsidy.⁶ That is, caseworkers does not have to approve each subsidy, and in most cases caseworkers are not even involved in the allocation. Under the new regime, caseworkers can still act as facilitators in forming new employer-employee matches, but their counseling activity is neither required for starting new subsidized jobs nor binding. Instead, the firms are solely responsible for initiating the procedures to apply for the targeted wage subsidy. Since allocation of the subsidies are determined by the

⁴In October 1999, the program was extended to include two different types of subsidies. Besides the old one with a maximum duration of 6 months, a new subsidy with a subsidy rate of 75 percent of the total wage costs for 6 months and then 25 percent for another 18 months was introduced. This subsidy was targeted to workers who had been unemployed for at least 36 months (changed to 24 months in January 2000).

⁵Differently from the Employment subsidy program, the New start job subsidy does not require the individual to has been registered as unemployed. Poor health, incarceration or other reasons for absence from the labor market could suffice.

⁶The only requirement is that the the prospective worker provides sufficient documentation of eligibility. The firms also have to fulfill some basic requirements, such as not having significant amounts of unpaid taxes. From January 2017 a new requirement is that the participating firms need to have a collective agreement with a labor union.

rules for the subsidy and not by caseworkers, we refer to this second program as the *rules-selection scheme*.

3 Data

We use data from several Swedish administrative registers. Data from the Swedish Public Employment Service (PES) provides information about all registered unemployed individuals. In particular, it includes detailed information about all individuals covered by the Employment subsidies and the New Start Jobs programs, including the start and the end date of each subsidy. By using unique personal and firm identifiers, this data is merged to matched employer–employee data from the RAMS register.⁷ This database contains information on all employment episodes for all employees in Sweden, including information on yearly labor income, which we use to compute the sum of wages paid each year by the firms. By following firms and workers over time, we obtain information on the number of employees, hiring rates, and separation rates at each firm. All firm level outcomes are constructed on a yearly basis.⁸ We focus on both the total number of workers and the number of workers currently employed who were hired using the employment subsidies. The latter includes both workers currently covered by the employment subsidies and workers remaining in the firm after the subsidy has ended.

We use information on firms’ operating costs and profits, assets value, revenues, yearly turnover, investments, value added and other firms’ production measures. This data is contained in Statistics Sweden’s business register of firm-level accounts. Operating profits are the difference between operating revenues (generated from the firm’s core business activities) and operating expenses (such as costs of goods and production), minus depreciation and amortization; value added is the total value that is added at each stage of production, excluding costs for intermediate goods and services, and is equivalent to total revenues minus intermediate consumption of goods and services;

⁷From the PES database we have information on all workers hired through the wage subsidies, but the PES data does not include information on the hiring firm. The matched employer–employee data does not include information on the exact starting date of each employment episode either. Thus, since a worker can start multiple jobs, we need another way to link each wage subsidy to a particular firm. We do this by only keeping the job with the highest salary, since job spells with lower wages are unlikely to be the ones initiated through a wage subsidy.

⁸The number of hirings is the number of workers employed in the current year who were not employed in the previous year. The number of separations corresponds to the number of workers employed in the firm in the previous year but not in this year.

firm productivity is defined as valued added per worker; investments per worker are the total yearly amount spent on land and machinery, net of the disinvestments in the same categories and divided by firm size.

Finally, we use population registers (Louise) to construct information on the characteristics of the employees at the firm-year level. These include age, level of education, marital status, immigrant status and gender.

4 Empirical strategy

We compare firms recruiting through subsidies (defined as treated) to other observably identical firms. We sample treated firms and comparison firms each year during the period 1998–2008 using the matched employer–employee data. Let us illustrate the sampling procedure for year t . We initially sample all firms with less than 30 workers in year $t - 1$, excluding self-employed. We then select firms that are observed in both year t and year $t - 1$, so that at least one year of firm history is available.⁹ Next, we use the PES information on the employment subsidies to identify firms with subsidized hires. We focus on wage subsidies that start during the first quarter of each year. The reason for this is that our firm level outcomes are measured on a yearly basis, so that by focusing on subsidies that start in the first quarter we make sure that the firm is exposed to the employment subsidy during the entire calendar year.¹⁰ For each firm we only study the first wage subsidy within our observation period.

As our comparison group, we use firms that hire from the pool of long-term unemployed in the same year, but without using the subsidy.¹¹ A long-term unemployed is defined as a worker who found a job after at least six months of unemployment, using the PES data to identify long-term unemployed. Since these comparison firms also hire at least one formerly unemployed worker in the same quarter as the treated firms, they are arguably in a somewhat similar situation as the treated firms. We repeat the sampling procedure each year, which means that a comparison firm could be selected as comparison firm in multiple years.

⁹We drop firms that grow to more than 60 workers within five years. The reason for this is that disproportionately fast-growing firms are likely to be driven by mergers. As robustness checks, we use different firm size cutoffs. For instance, we exclude firms with less than 25 or 35 workers in $t - 1$ or with more than 55 or 65 workers within 5 years from t , and we obtain qualitatively similar results.

¹⁰Firms first hiring with subsidies starting in quarter two to four are removed from the sample.

¹¹As for the treated firms, we focus on firms hiring from the pool of long-term unemployed in the first quarter of the year.

In our empirical strategy, we use matching to correct for observable differences between treated and comparison firms. But, after adjusting for observable firm characteristics, such as firm size, sector and employee characteristics, firms hiring with and without a subsidy could differ in several ways. To adjust for unobserved characteristics we, therefore, use data from before and after the subsidized hiring, allowing us to difference out any time-invariant differences between treated and comparison firms. When focusing on the comparison between the two regimes we employ a triple-difference approach in which we compare across treatment status, time and treatment regime. This allows us to difference out selection patterns that are the same for the two regimes. Below, we will provide graphical evidence that supports our empirical strategy.

Table 1 provides summary statistics for the firms in our sample. During the staff-selection regime, the comparison firms have higher value added, have faster turnover and are more than one worker larger than the treated firms. The pattern is similar when comparing treated and comparison firms during the rules-selection regime. We next focus on a graphical comparison of treated and comparison firms.

4.1 Graphical comparison

As a background to our empirical analysis, we provide graphical evidence. Figure 1 shows the average number of workers in the treated and comparison firms five years before and five years after the start of the subsidy, in the staff-selection regime. Year zero is the year the subsidy starts or, for the comparison firms, the year of hiring an unsubsidized long-term unemployed worker in the first quarter. Note that we focus on small and medium sized firms, which explains why the average firm size is rather small (around 9 workers). From the figure we see that the comparison firms on average are somewhat larger than the treated firms. But, more importantly, the pre-treatment trends are very similar in the two groups: for both treated and comparison firms, the average number of workers remains roughly constant before the subsidy. Figure 8 (in the Appendix) shows a similar pattern for the rules-selection regime.

Based on the strikingly similar pre-treatment trends we are comfortable in comparing treated and comparison firms. However, we use matching to adjust for both the constant pre-treatment difference in average number of workers and other observable firm characteristics.

4.2 Matched samples

To adjust for pre-treatment differences, we for each treated firm select one comparison firm using nearest-neighbor propensity-score matching on firm-level covariates measured one year before the actual or potential start of the subsidy. In our baseline specification, we match on industry indicators (8 categories), firm size, average workers' characteristics and number of separations. The average workers' characteristics that we match include level of education, age, gender and civil and migrant statuses.¹² We perform the matching procedure described above separately each calendar year. This produces two matched samples, one for the staff-selection regime and one for the rules-selection regime.

Figure 2 illustrates the treated firms and matched comparison firms, in the staff-selection regime. Note that we match on the average number of workers in year -1 , which explains why firm size is almost exactly the same for the two groups in that year.¹³ However, note that the average number of workers is very well aligned for all pre-treatment years, despite the fact that we only match on the number of workers in year -1 . Similar result are obtained when considering the rules-selection regime (see Figure 9 in the Appendix).

Besides a basic set of firm characteristics, we match on the average number of workers, hirings and separations the year before the start of the subsidy. We can also compare pre-treatment differences and trends for other firm performance measures that we do not match on. Table 2 report pre-treatment trends for wage sum and log value added. Even if we do not match on these variables, we find small differences between treated and comparison firms, and the pre-treatment trends are very similar. This holds both for the staff-selection regime (columns 1-3) and the rules-selection regime (columns 4-6). The fact that we find similar pre-treatment trends for these variables that we do not match on, lends further support to the comparison of treated and matched comparison firms.

¹²In a robustness analyses below, we match on different sets of variables, suggesting that our results are robust.

¹³We have also examined the balance for the other firm characteristics used in the matching, and as expected they are all well-balanced.

4.3 Empirical model

The above matching step produces the matched sample used for the analyses, and allows us to compute the average treatment effects on the treated (ATET). However, since we observe each cross-sectional unit over time, before and after the actual or potential treatment, we can apply fixed-effects panel data methods to control for time-fixed unobservable characteristics not accounted for in the matching step, allowing us to adjust for both observed and unobserved firm characteristics.

To fix ideas, suppose y_{it} is the number of workers in firm i at time t , with t denoting the number of years since actual or potential (for the controls) treatment. We are interested in the effect of hiring with subsidy a long-term unemployed individual in $t = 0$ on the firm-level outcome, up to T years since the treatment. Our baseline model for the i -th firm size in t is:

$$y_{it} = \alpha + \sum_{s=0}^T \beta_s d_s + \gamma D_i + \sum_{s=0}^T \delta_s (D_i \cdot d_s) + \varepsilon_{it} \quad (1)$$

where d_s is a time indicator equal to one if $s = t$, D_i is the treatment status indicator for firm i , and δ_s is the causal effects of the subsidized hiring event.¹⁴ Model (1) is separately estimated by subsidy regime using the matched data described in the previous section, with up to five pre- and post-treatment years and clustered standard errors at firm level. The specification of this fully saturated regression model resembles that used in a difference-in-differences design where the ATET are allowed to vary with time since treatment.

5 Main results

5.1 The staff-selection regime

We first focus on the staff-selection regime with subsidized hirings in the period 1998–2006, during which the subsidies required approval from a caseworker. Figure 3 shows the difference between treated and comparison firms in the total number of workers (dots) and in the number of subsidized workers (triangles). As already noticed, there is virtually no difference between the treated and the comparison firms before the

¹⁴ The entire pre-treatment period is reclassified into one (reference) interval. We also alternatively leave all the pre-treatment yearly dummies but one to check for trends.

subsidy. The year when the subsidy is used to hire, the number of subsidized workers increases by slightly more than one, which reflects the fact that some firms hire more than one subsidized worker at once. In comparison, in the treatment year total number of workers is almost the same in the two groups. This is because the comparison firms also hire at least one worker in the treatment year. But, beyond the first year, we observe large and persistent differences between treated and comparison firms where the treated firms becomes larger than the matched comparison firms. Five years after the start of the subsidy, the treated firms are on average one worker larger than the comparison firms. The average firm size in our sample is nine workers, so the observed difference is substantial.

In Table 3, we compare treated and comparison firms using our regression model. Column 1 in Panel A for the staff-selection regime summarizes the pattern that we observed in Figure 3. In the subsidy year, there is no difference between treated and comparison firms, but then the two groups gradually diverge and the average number of workers is significantly higher among the treated firms. In Column 2 of the same table, we observed a similar pattern for the yearly wage sum as for the number of workers. Thus, the increased number of workers does not seem to be counteracted by a decreased number of hours worked per worker.

Figure 3 also reveals to what extent the observed difference between treated and comparison firms are due to a difference in the number of subsidized workers or if it also reflects a difference in the number of non-subsidized workers. Here, subsidized workers include everyone hired using a subsidy, including both currently subsidized workers and workers who remain in the firm after the subsidy has expired.¹⁵ As expected, the number of subsidized workers increase by roughly one in the subsidy year, and then gradually decreases over time as some of the subsidized workers leave the firm, reflecting natural turnover in the labor market. Interestingly, two years after the start of the subsidy, the difference in the total number of workers is almost exactly as large as the difference in the number of subsidized workers. Thus, the firms using the subsidize to lower the cost of labor is not just artificially inflated by the subsidies: these firms are the ones characterized by sustained growth over time.

Panel A of Table 3 also reports estimates for several other firm performance outcomes. In Column 4, we examine total valued added and Column 5 gives results for value added per worker (productivity). We see that value added per worker is rather

¹⁵It also includes any subsidized worker hired after the treatment year, but very few of these small and medium sized firms hire another subsidized worker.

similar for treated and comparison firms, with some evidence of a positive effect for the treated firms 1–2 years and 3–5 years after the start of the subsidy. Since the number of workers increases this means that total value added goes up. Overall, this means that the fast growth of the businesses benefitting from the subsidies does not come at the cost of decreased productivity. If anything, productivity is actually positively related with the subsidized hirings, at least a couple of years after the start of the subsidy.

Finally, Column 3 report estimates for profits, for which we mainly find insignificant effects, even though the effect after 3–5 years is positive and significant at the ten percent level.

5.2 The rules-selection regime

We now report results for the rules-selection regime. We focus on firms hiring subsidized workers in 2007–2008, to avoid sampling firms during the great recession (the unemployment rate in Sweden started rising during the first quarter of 2009). Initially, Figure 4 shows the effects on the total number of workers and the number of subsidized workers (triangles). Here, we see almost no effect of the subsidies on the total number of workers. This holds both 1-2 years and 3-5 years after the start of the subsidy. This is confirmed by the regressions estimates reported in Panel B of Table 3 (Column 1), which reveal no significant differences between the treated and the comparison firms in terms of the total number of workers. The same thing holds for the yearly wage sum (Column 2). This pattern holds even though the subsidized workers tend to stay in the treated firms. In fact, Figure 4 shows that around half of the subsidized workers remains with the firm five years later.

Panel B of Table 3 also report estimates for value added, value added per worker and profits. Neither for total valued added nor for value added per worker we see any effects during this rules-selection regime (Columns 4 and 5). For profits, there is a tendency towards increased profits as a results of the subsidy (Column 3). In all years, the effect is positive and it is significant at the five percent level 1-2 years after the start of the subsidy. In other words, the number of workers and value added per worker do not change, but since the labor costs is reduced due to the subsidy, this leads to some positive effects on profits.

5.3 Comparison between the two regimes

We now use an event study analysis in order to make a more detailed comparison between the two regimes. The analysis is performed using the two matched samples and by running our empirical specification above complemented with a set of indicator variables indicating the years before and after the closure (with $t-1$ as reference category). This reveals both the pre-treatment trends as well as the year-by-year effect after the start of the subsidy. We show the point estimates and the 95% confidence intervals for each regime. Initially, Figure 5 illustrates the effects for firm size (number of workers). As already seen, for both regimes there are no significant pre-treatment trends. More importantly, the figure reveals a striking difference between the two regimes. In the staff-selection regime, the subsidies lead to increased employment, while during the rules-selection regime the subsidies have no effect on net employment. Figure 6 reveals a similar pattern for the yearly wage sum, with effects on the wage sum during the staff-selection regime but no effects during the rules-selection regime.

This pattern holds even though the subsidized workers tend to stay in the firms to the same degree in the two periods. In both figures 3 and 4, we see that around half of the subsidized workers that are hired in year zero remain employed in the firm after five years. The difference across regimes, instead, lies in the number of non-subsidized workers. During the staff-selection regime the subsidies lead to an increase in net employment, while during the rules-regime the increased number of subsidized workers is fully counteracted by a drop in the number of non-subsidized workers. That is, during the staff-selection regime there is only partial displacement of non-subsidized workers, while during the rules-selection regime each subsidized worker displaces one non-subsidized worker, so that, during this regime, all subsidized jobs merely reflect displacement of non-subsidized jobs.

However, one might worry that the increased number of workers could affect productivity. But, Figure 7 reveals no negative effects on workers' productivity (log value added per worker), for neither the staff-selection regime nor for the rules-selection regime. If anything we see positive effects on value added per worker during the staff-selection regime, but not during the rules-selection regime.

In sum, for staff-selection regime treated firms outperform the comparison firms after treatment and there is only partial displacement of non-subsidized jobs. For the rules-selection regime we find no effects on firm size and productivity measures, and we have full displacement as each subsidized job displaces a non-subsidized job. That

is, when caseworkers are involved in the allocation process, the subsidized jobs are allocated such they lead to sustained relative growth. In the next section, we will explore the mechanisms behind this result.

5.4 Robustness analyses

As robustness checks, we repeat the analysis by using several alternative specifications for the propensity score and different firm size cutoffs when defining the sample selection criteria. Columns (2)–(4) of Table 6 report the resulting estimates for firm size regressions, which are all qualitatively similar to Column (1) estimates, obtained using the main analyses specification.

Table 7 additionally reports firm size estimates obtained using the baseline specification and partitioning the matched firms into hiring when unemployment is classified as high or low (above or below the national median level, respectively). We do not find substantial heterogeneous effects according to local unemployment conditions existing when firms hire, so that results are quite similar to those obtained in the main analyses. Furthermore, local unemployment conditions do not appear to explain the substantial differences in the post-treatment effects of subsidized hirings across regimes already highlighted in the main analyses.

6 Mechanisms

Table 4 presents firm- and worker-level information relative to the year a long-term unemployed is hired. Columns (1)–(3) focus on treated units in the matched samples, while Column (4) compares matched treated with unmatched controls across regimes.

Column (3) shows that both firms using subsidies and the respective long-term unemployed hired tend to be observationally different across regimes. Under the new regime, the workers hired with subsidy tend to be more likely to be foreigners, older, better educated, and to exit from unemployment faster. Additionally, there exist some cross-regimes differences in terms of the industries that the firms hiring with subsidies are in. In order to isolate potential sources of discrepancies across regimes, we net out everything that is time constant between treated and controls. This is done in Column (4), where we repeat the analyses by pooling the matched treated with the raw (unmatched) controls, and we compare their characteristics across regimes in a difference-in-differences exercise. As a result, most of the differences in firms and

workers' characteristics between treated and controls are eliminated, except for the share of workers classified as youngest and oldest.¹⁶

Although Table 4 shows some evidence that caseworkers allocate subsidies to different types of workers as compared to the new regime, this is not likely to fully explain the substantial post-treatment outcomes differences across the two policy regimes presented in Section 5.3. A possible explanation for these differences, is that caseworkers to a larger extent select firms that use the subsidies to expand, rather than targeting firms that simply use the subsidized workers to displace unsubsidized jobs. In order to investigate this possibility, we study how the targeted subsidies relate to investments made by the firms in the two regimes. Table 5 shows that there are no systematic patterns in the investments of firms hiring with or without subsidies, suggesting that any differences in the hiring patterns and production outcomes across regimes is not related to the fact that firms are expanding.

7 Conclusions

In this paper we study how two alternative targeted wage subsidies schemes are used by firms and the implications for workers and firms themselves over time. Our analyses are relevant from a policy perspective because the two regimes can be seen as being on two opposite poles. In what we called *staff-selection* regime, caseworkers are primarily involved in matching with firms the long-term unemployed eligible for being hired with subsidies. On the other hand, since 2007 the *rules selection* regime requires firms to actively use subsidies to hire workers, leaving a more marginal role to the caseworkers.

We find that, with staff selection, in the post-treatment period the treated firms do much better in terms of relative growth than the matched controls. This holds if we look at both firm size and sum of wages paid to the employees. Importantly, despite firms do not repeatedly hire using subsidies over time, the pattern of relative growth associated with the subsidized hirings is not just transitory. Moreover, the subsidized workers that leave the firm are replaced with an even higher number of employees, leading to actual sustained firm growth. On the contrary, when firms are involved in the selection process, conditional on matching, the subsidies are related to negative firm-level outcomes. This is true despite the fact that caseworkers seem to select firms

¹⁶ In the new regime workers are also more likely to exit faster to job, but the magnitude of the difference is negligible.

with a *less* positive differential pre-treatment trend (if we look at number of employees and wage sum during the period preceding the treatment).

The other firm-level outcomes considered all point to similar conclusions. In particular, when caseworkers match subsidized workers with firms, there is some evidence for the wage subsidies to increase profits in the long run. Instead, under the new regime, after an initial increase in the short run, both profits and value added quickly drop over time. In addition, in the staff selection regime, the increase in size of treated firms does not come at the cost of decreased productivity, which does not show a post-treatment pattern different from 0. All in all, caseworkers armed with high subsidy rates appear to be able to match long-term unemployed workers to firms who persistently grow in size and production because of the subsidised matches.

In order to explain the alternative mechanisms behind this, we explore the allocation of subsidized workers to firms. Across regimes, we find some compositional differences in the type of workers matched to firms, with both young and old workers being more likely to be hired with subsidies in the new regime. On the other hand, by inspecting firms investments, we show that subsidies do not appear to be used by expanding firms.

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Tables and Figures

Table 1 – Sample statistics for treated and comparison firms in the two regimes. All characteristics measured one year before the subsidy

	Staff selection		Rules selection	
	Treated firms	Control firms	Treated firms	Control firms
<i>Group size</i>	11,525	19,735	3,344	4,602
<i>Pre-treatment outcomes</i>				
No. of workers	9.31 (6.97)	10.72 (7.40)	10.08 (7.30)	11.64 (7.62)
Wage sum	1,054 (1,097)	1,222 (1,360)	1,435 (1,443)	1,592 (1,602)
No. of hirings	2.80 (3.47)	3.39 (3.88)	3.21 (3.62)	3.83 (4.08)
No. of separations	2.03 (3.23)	2.67 (3.73)	2.14 (3.01)	2.82 (3.72)
Value added per worker	394.02 (341.64)	425.09 (561.68)	469.53 (368.95)	486.87 (551.84)
log (value added)	7.10 (1.11)	7.20 (1.16)	7.37 (1.15)	7.46 (1.18)
Operating profit	324 (1,200)	359 (2,403)	511 (1,714)	502 (3,015)

Notes: Sample statistics for the sample of treated firms (hiring with subsidy) and comparison firms (hiring without subsidy), before matching. Wage sum (in 1000 SEK) is the sum of all wages paid by the firm during the calendar year. Value added (in 1000 SEK) is total revenues minus intermediate consumption of goods and services. Standard deviations in parentheses.

Figure 1 – Number of workers for treated and comparison firms, before matching
(staff-selection regime)

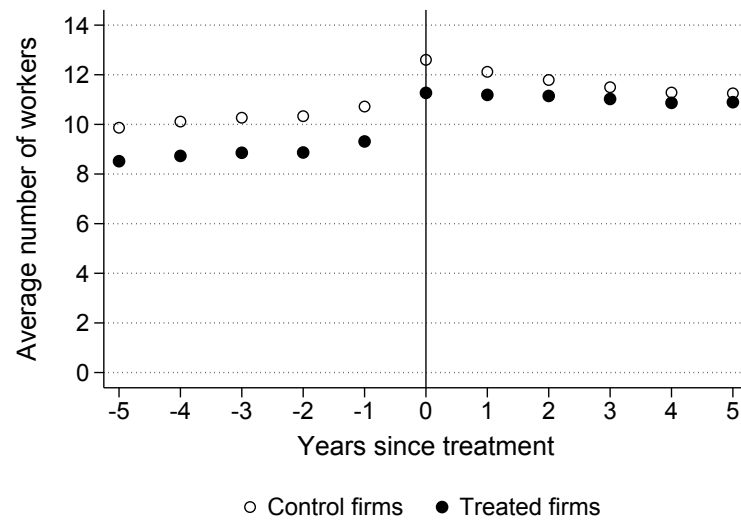


Figure 2 – Number of workers for treated and comparison firms, after matching
(staff-selection regime)

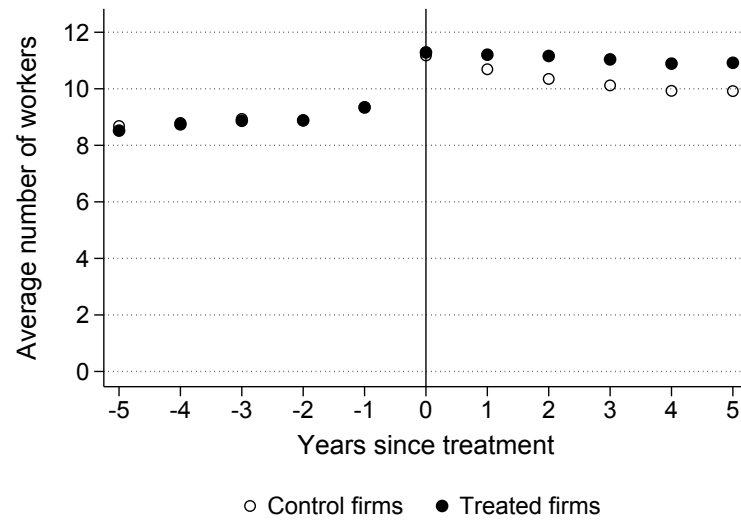


Table 2 – Sample statistics for pre-treatment outcomes for the matched samples

	Staff selection			Rules selection		
	Treated (1)	Control (2)	Difference (3)	Treated (4)	Control (5)	Difference (6)
Panel A ^a						
<i>No. of workers</i>						
$t - 5$	8.52	8.68	−0.16	9.29	9.30	−0.01
$t - 4$	8.74	8.79	−0.05	9.13	9.32	−0.19
$t - 3$	8.87	8.93	−0.07	9.17	9.35	−0.18
$t - 2$	8.88	8.88	0.00	9.30	9.55	−0.26
$t - 1$	9.34	9.36	−0.02	10.10	10.07	0.02
Panel B ^b						
<i>Wage sum (Th. SEK)</i>						
$t - 5$	870.33	860.87	9.46	1,232.42	1,249.43	−17.00
$t - 4$	925.72	914.53	11.18	1,247.10	1,250.12	−3.01
$t - 3$	982.80	961.55	21.24	1,275.89	1,303.65	−27.76
$t - 2$	1,011.97	996.55	15.41	1,326.07	1,356.49	−30.42
$t - 1$	1,059.30	1,038.43	20.86	1,439.03	1,438.97	0.06
<i>Profits (Th. SEK)</i>						
$t - 5$	344.66	373.57	−28.91	341.42	323.04	18.37
$t - 4$	304.83	368.13	−63.30 ^{**}	347.75	258.05	89.70 ^{**}
$t - 3$	294.30	363.81	−69.51	370.00	343.68	26.33
$t - 2$	305.97	339.17	−33.21	435.36	398.46	36.90
$t - 1$	321.01	313.19	7.83 [*]	511.03	409.28	101.75 [*]
<i>Log value added</i>						
$t - 5$	7.05	7.08	−0.03	7.23	7.20	0.03
$t - 4$	7.01	7.06	−0.05	7.21	7.22	−0.01
$t - 3$	7.04	7.03	0.01 [*]	7.21	7.27	−0.06 [*]
$t - 2$	7.07	7.05	0.02	7.26	7.31	−0.05
$t - 1$	7.10	7.06	0.04	7.37	7.38	−0.01

Notes: Statistics for the matched samples described in Section 4.3. Wage sum (in 1000 SEK) is the sum of all wages paid by the firm during the calendar year. Value added (in 1000 SEK) is total revenues minus intermediate consumption of goods and services. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Figure 3 – Difference treated and comparison firms, staff-selection regime



Table 3 – Estimates for firm-level outcomes for the two regimes

	No. of workers (1)	Wage sum (2)	Profits (3)	Value added (4)	Productivity (5)
<i>Panel A: Staff selection^a</i>					
Year of treatment	0.16 (0.10)	41 ^{***} (12)	28 (30)	0.06 ^{***} (0.01)	0.00 (0.01)
1–2 years after treatment	0.71 ^{***} (0.11)	86 ^{***} (18)	15 (27)	0.09 ^{***} (0.02)	0.02 ^{**} (0.01)
3–5 years after treatment	1.01 ^{***} (0.14)	136 ^{***} (26)	58 [*] (33)	0.14 ^{***} (0.02)	0.03 ^{***} (0.01)
No. of observations	208,578	208,578	163,269	169,454	158,366
No. of firms	22,844	22,844	21,012	20,560	18,811
<i>Panel B: Rules selection^b</i>					
Year of treatment	0.01 (0.18)	19 (30)	88 (58)	0.02 (0.03)	−0.01 (0.02)
1–2 years after treatment	0.10 (0.20)	15 (41)	129 ^{**} (60)	0.05 [*] (0.03)	−0.02 (0.02)
3–5 years after treatment	0.13 (0.28)	26 (65)	35 (61)	0.05 (0.04)	−0.01 (0.02)
No. of observations	61,931	61,931	51,544	50,174	46,948
No. of firms	6,660	6,660	6,357	6,170	5,771

Notes: Estimates using the matched samples described in Section 4.3. The model also includes calendar time fixed effects and indicators for treatment status. Wage sum (in 1000 SEK) is the sum of all wages paid by the firm during the calendar year. Value added (in 1000 SEK) is total revenues minus intermediate consumption of goods and services. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Figure 4 – Difference treated and comparison firms, rules-selection regime



Figure 5 – Estimates for total number of workers, comparison of the two regimes

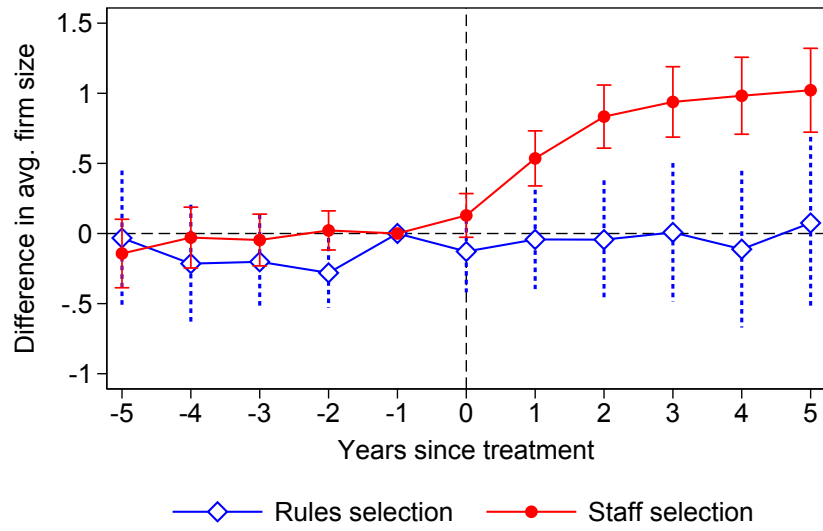


Figure 6 – Estimates for wage sum, regimes comparison

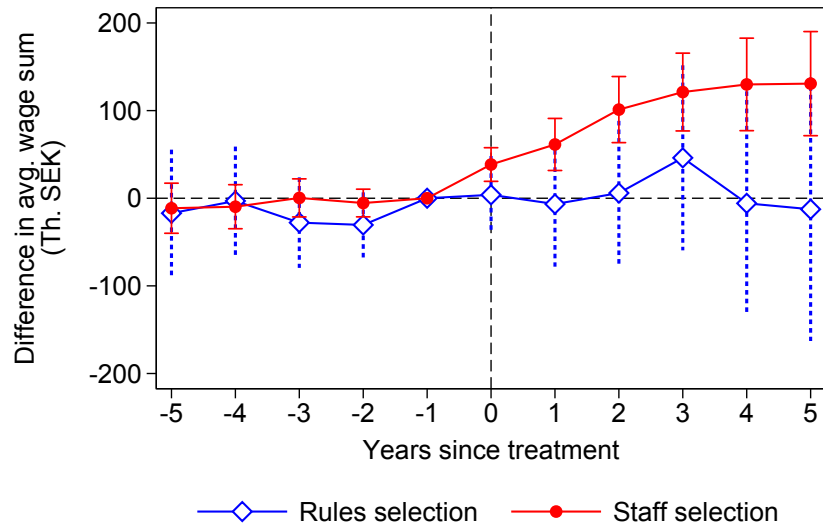


Figure 7 – Estimates for log value added per worker, regimes comparison

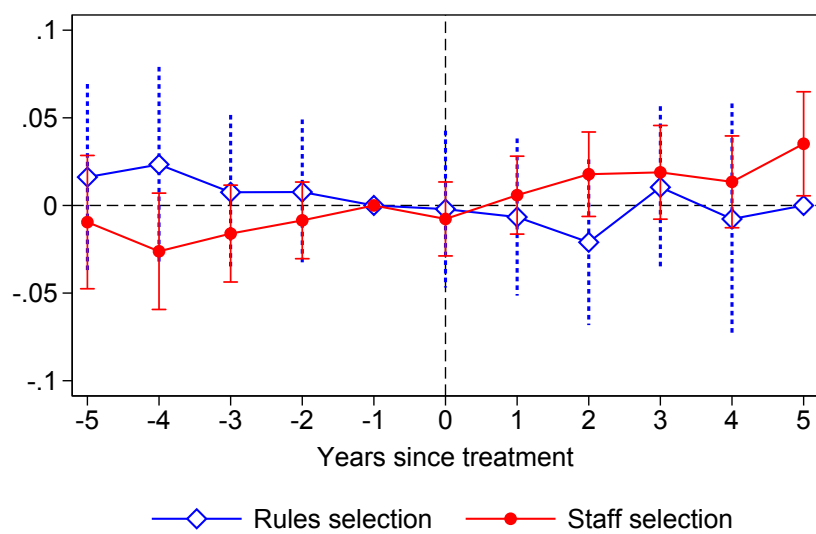


Table 4 – Caseworkers and the allocation of workers and firms

	Staff selection (1)	Rules selection (2)	Difference (3)	Diff. in diff. (4)
<i>Panel A: Workers characteristics^a</i>				
Avg. predicted exit to job in 12 months	0.881	0.869	-0.011***	-0.005***
Share non-Swedish	0.228	0.316	0.088***	0.010
Share males	0.672	0.666	-0.006	-0.004
Share younger than 24	0.194	0.166	-0.027***	-0.035***
Share 25–34 year old	0.300	0.245	-0.055***	-0.016*
Share 35–44 year old	0.233	0.228	-0.005	-0.014
Share 45–54 year old	0.172	0.183	0.010	0.005
Share 55–64 year old	0.100	0.174	0.074***	0.060***
Share older than 65	0.001	0.004	0.003***	0.001
Share in secondary education	0.648	0.589	-0.060***	-0.010
Share in higher education	0.156	0.203	0.047***	-0.017*
<i>Panel B: Firms characteristics^b</i>				
Share in secondary education	0.582	0.568	-0.014***	0.005
Share in higher education	0.180	0.212	0.032***	-0.006
Share in Manufacturing ind.	0.177	0.194	0.017**	0.015
Share in Trade industry	0.277	0.255	-0.022**	-0.010
Share in Hotel industry	0.067	0.101	0.033***	-0.008
Share in Transports industry	0.057	0.068	0.011**	0.003

Notes: Average characteristics of the long-term unemployed hired and of the respective firms hiring. In columns (1)–(3) all quantities are relative to the subsidized hirings and are measured in the matched sample the year of outflow from unemployment, separately pooling 1998–2006 and 2007–2008 hirings. Column (4) reports the treatment status times rules–selection regime interaction coefficient of difference-in-differences specifications where the hiring year matched treated are compared to unmatched controls across the two regimes. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

^a Average hirings quality and average subsidized workers' characteristics. The former is measured as the probability of exiting to job within 18 or 24 months of unemployment (estimated in the full PES sample at individual level as a function of workers' demographics).

^b Share of subsidized hirings by (1) firms operating in the most represented industries; (2) firm–year level averaged workforce education.

Table 5 – Firms investments; matched sample

	Net investments per worker	
	logs	level
<i>Panel A: Staff selection</i>		
Pre-treatment year	0.02 (0.04)	2.70 (4.80)
Year of treatment	0.01 (0.04)	7.37 (7.60)
1–2 years after treatment	-0.05 (0.03)	3.61 (6.87)
3–5 years after treatment	-0.03 (0.04)	-4.45 (6.57)
<i>Panel B: Rules selection</i>		
Pre-treatment year	-0.08 (0.06)	-3.93 (8.03)
Year of treatment	-0.08 (0.07)	-3.14 (5.63)
1–2 years after treatment	-0.10 (0.06)	4.00 (16.57)
3–5 years after treatment	-0.11 (0.07)	4.23 (5.46)

Notes: Firm investments regressions using the matched sample and regressing net per worker-investments in machinery and land on treatment dummy, pre- and post-treatment period dummies, interactions between the two and year fixed effects. The outcomes are defined considering the yearly amount invested net of disinvestments, both in logs and in levels. The Propensity Score specification did not include investments among the pre-treatment controls. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Appendix: Additional Figures and Tables

Figure 8 – Number of workers for treated and comparison firms, before matching (rules–selection regime)

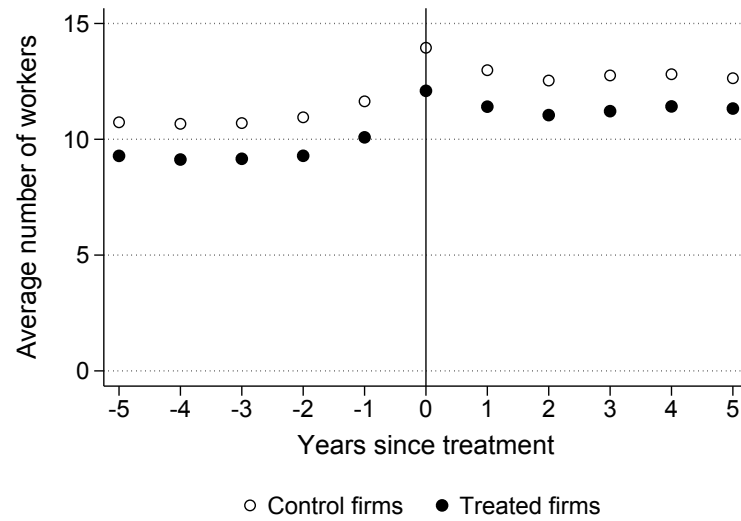
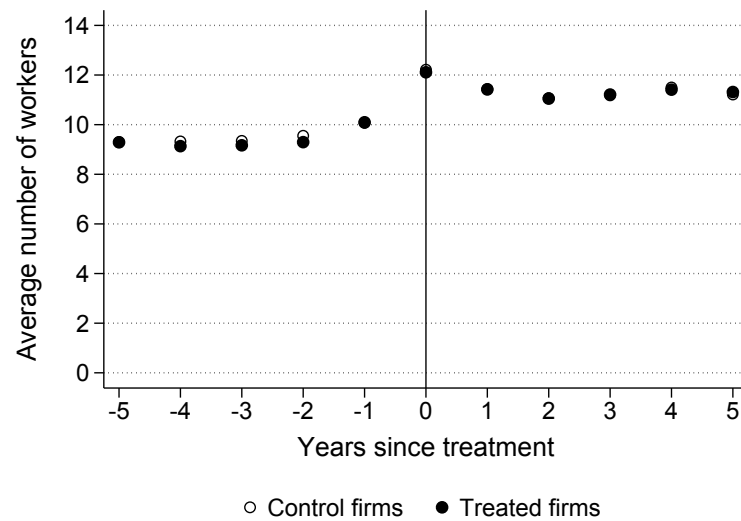


Figure 9 – Number of workers for treated and comparison firms, after matching (rules–selection regime)



Appendix: Robustness analyses

Table 6 – Estimates for number of workers by years since treatment

	Baseline (1)	Firm size (2)	Controls (3)	Sampling (4)
<i>Panel A: Staff selection</i>				
Year of treatment	0.16 (0.10)	−0.08 (0.10)	0.18 (0.14)	0.01 (0.11)
1–2 years after treatment	0.71*** (0.11)	0.45*** (0.12)	0.57*** (0.16)	0.58*** (0.12)
3–5 years after treatment	1.01*** (0.14)	0.98*** (0.14)	1.05*** (0.18)	0.98*** (0.15)
<i>Panel B: Rules selection</i>				
Year of treatment	0.01 (0.18)	−0.09 (0.19)	−0.11 (0.26)	−0.03 (0.20)
1–2 years after treatment	0.10 (0.20)	0.10 (0.20)	0.03 (0.27)	0.07 (0.22)
3–5 years after treatment	0.13 (0.28)	0.29 (0.26)	0.07 (0.39)	0.15 (0.29)

Notes: Robustness of estimates for firm size regressions. Column (1): estimation with baseline Propensity Score (PS) specification used for the main results of the paper; Column (2): PS baseline specification, but using discrete firm size; Column (3): PS specification controlling for firm size, hirings and industry dummies; Column (4): estimation sampling firms smaller than 35 employees the pre-treatment year and not bigger than 65 in any post-treatment year. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Table 7 – Firm size regressions by unemployment rate level; matched sample

	Staff selection		Rules selection	
	Unemployment Low	Unemployment High	Unemployment Low	Unemployment High
Year of treatment	0.275 (0.197)	0.142 (0.111)	0.055 (0.199)	−0.013 (0.476)
1–2 years after treatment	0.701*** (0.219)	0.714*** (0.126)	0.106 (0.223)	0.234 (0.496)
3–5 years after treatment	0.948*** (0.301)	1.024*** (0.151)	0.203 (0.313)	−0.004 (0.606)

Notes: Firm size regressions partitioning firms as hiring when the monthly unemployment rate is high or low (above or below the 1998–2008 median national unemployment rate level, respectively). All regressions are specified as in the main analyses and use the matched sample. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.