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Highlights

- Studies how targeted employment subsidies affect firm performance
- Subsidized firms are smaller than other firms, but otherwise similar before
- Subsidized firms outperform other firms after the subsidy
- Less clear results after caseworkers no longer needed to approve of subsidy

Targeted wage subsidies and firm performance*

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Abstract

This paper studies how targeted wage subsidies affect the performance of the recruiting firms. Using Swedish administrative data from the period 1998-2008, we show that treated firms substantially outperform other recruiting firms after hiring through subsidies, despite identical pre-treatment performance levels and trends in a wide set of key dimensions. The pattern is less clear from 2007 onwards, after a reform 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, at least if the policy environment allows for pre-screening by caseworkers.

Keywords: wage subsidies, labor demand, firms performance
JEL classification: J08, J2, J6

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1 Introduction

Targeted wage subsidies that reduce part of the wage costs for private firms hiring unemployed workers are an integral part of active labor market policies (ALMP) in most Western countries. The main objective is to help disadvantaged workers find jobs, and most studies tend to find that the policy tool is very efficient in this dimension (for surveys see, e.g., Card et al. 2010, 2015 and Kluve 2010). Despite these positive estimates, policy prescriptions tend to be cautious because of concerns regarding demand side responses (see e.g. Neumark, 2013). These concerns include crowding out of unsubsidized hires and fears that wage subsidies allocate workers to unproductive firms that are able to hire and compete on the market only due to the subsidies. Yet, there exists very little systematic evidence on the characteristics of the firms that hire with targeted subsidies, and on the impact the subsidies have on these firms.

In this paper, we make three distinct additions to the literature: we document the extent to which the characteristics of subsidized firms differ from those of other recruiting firms, we describe the extent to which key firm-level outcomes change due to the subsidies, and we analyze whether these patterns depend on the degree of caseworker discretion when subsidies are allocated. Together, this provides new empirical evidence on key concerns regarding wage-subsidy distortions. The results also provide some novel (and rare) evidence on how ALMPs affect the allocation of workers across firms, an issue that has received much recent attention within the wider labor-economic literature (see e.g. Card et al. 2013, Song et al. 2015 and Card et al. 2018).

Our analysis uses detailed Swedish administrative data on workers and firms in order to study the impact of targeted wage subsidies. We start from spell data on unemployed workers and the subsidies they receive and link this information to a matched employer-employee database which allows us to follow the 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 in order for the subsidies to be of a non-trivial magnitude relative to firm-performance measures. For the causal analysis, we compare treated firms to firms that hire unemployed workers without using subsidies. We adjust for pre-existing differences in firm size and separations, sum of wages paid and average workers' characteristics by matching on observable pre-treatment levels in these dimensions. We show that, after matching, the treated and matched controls have identical pre-treatment trends (which we do not match on). Furthermore, both pre-treatment trends and levels are remarkably similar in key dimensions that we do *not* match on, most notably productivity and profits. We find no evidence that the subsidies are allocated to low-performing firms. The pre-hire performance of the subsidized firms is remarkably similar to that of other recruiting firms, despite the fact that the subsidized hires (by design) have much longer pre-match unemployment spells. The main difference between the two groups of firms is that subsidized firms are smaller. But in terms of productivity, profits and staff composition, similarities in both levels and trends are striking.

We analyze two very different policy systems. 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 rules-selection system introduced in 2007, which granted all employers that hired an eligible long-term unemployed worker the right to receive a wage subsidy, thus substantially reducing the role of caseworkers in the allocation of the subsidies.

In the regime where caseworkers pre-approved subsidized matches, treated firms substantially outperform the comparison firms *after* the treatment, both in terms of the number of employees and in terms of various production measures, despite having identical pre-match trajectories. This pattern is persistent and it does not come at the cost of decreased productivity per worker. That is, in this system, the subsidies are clearly associated with positive changes in firm performance. In the second system, when long-term unemployed are entitled to subsidies without caseworker approval, the results are less clear. We find no corresponding change in firm size and productivity measures among surviving firms. This would suggest larger crowding-out effects and more windfall gains. On the other hand, the subsidies have a clear positive effect on firms' survival rates in the rules selection regime.

We show that the difference between systems is not due to differences in the hired workers' characteristics. If anything, caseworkers target more vulnerable workers and detailed controls for worker characteristics does not change the conclusion. Further evidence suggests that business cycle conditions and/or the increasing share of immigrant workers are unlikely explanations for the differences between systems. A possible hypothesis for the different findings is instead that caseworkers act as gatekeepers guarding against both displacement of non-subsidized jobs and windfall gains, and screening against firms on the margin of exit. As a corroborate of this hypothesis, we show results indicating that caseworkers guard against an overallocation of subsidies to firms with poor internal expectations about future performance. This exercise uses data on investments which (in line with standard investment theory) we interpret as a forward-looking variable capturing the firm's own expectations about future performance and we find that investments are lower for treated firms in the rules-selection scheme but not in the staff-selection scheme.

Our paper is related to several strands of the existing literature. In a recent paper, Cahuc et al. (2016) use a French reform in 2008 to study the effectiveness of hiring credits. Firms with fewer than 10 employees that hire a worker with a wage less than 1.6 times the minimum wage were eligible for the credit. The main result is of a strong and immediate employment effects of the credits. Using experimental variation, Crépon et al. (2013) find that a job placement assistance program in France displaces employment of non-treated unemployed individuals searching for jobs in the same area as the treated workers. In our paper, we find evidence of a different type of displacement, namely that of non-subsidized workers already employed in the firms hiring with the subsidies. Kangasharju (2007) uses Finnish data that links firms and workers, and finds that employment subsidies in Finland increased the firms' payroll by more than the size of the subsidy. Other studies on displacement effects include those that have used surveys of employers. For instance, Bishop and Montgomery (1993) survey more than 3500 private employers in the US and conclude that at least 70% of the tax credits granted to employers are payments for workers who would have been hired in the absence of any subsidy. In a similar vein, Calmfors et al. (2002) discuss Swedish survey-based evidence. Andersson et al. (2016) evaluate a *training* program in the U.S. and consider various measures of firm quality as outcomes. These measures include firm size, turnover, as well as firm-effects defined in Abowd et al. (1999). Overall, they find modest effects on the quality of the firms where the formerly unemployed workers find jobs.[1]

Finally, two recent 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, and examine

[1]For survey evidence how wage subsidies affect the unemployed workers covered by the subsidies see Card et al. 2010, 2015; Kluve 2010). For recent evidence on Swedish data, see Sjögren and Vikström (2015) on targeted employment subsidies and Egebark and Kaunitz (2014) and Saez et al. (2017) on non-targeted payroll tax reductions for youths. The latter of these papers also study spillover (wage) effects within the firms through rent sharing.

if this affected the firms in areas exposed to the experiment. Lechner et al. (2013) exploit that German local employment offices determine the mix of ALMPs to study firm level effects. In this paper, we use data that links firms and workers to study firms that are actually targeted by the subsidies, whereas these two studies focus on effects on all firms in a certain area.[2]

The paper is structured as follows. Section 2 provides the institutional background and discusses the potential role of caseworkers. Section 3 explains the data and outlines the empirical strategy. Section 4 presents the results. Finally, Section 5 concludes.

2 Background

2.1 The targeted wage subsidies

In Sweden, targeted wage subsidies and all other aspects of Active Labor Market Policies are administrated by the Swedish Public Employment Service (PES). The overall aim of the agency is to promote a well-functioning labor market for both unemployed individuals and firms. The PES provides different policy measures targeted to unemployed individuals, including job search counseling, labor market training, practice programs and targeted wage subsidies. Another aim is to support firms in the recruitment process, in particular by maintaining 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 enrolling the people assigned to them into policy programs and to provide job-search assistance.

In this paper we focus on targeted wage subsidies. These subsidies target different sets of unemployed individuals and reimburse part of the firms' labor costs by crediting their tax accounts when an eligible person is hired. The aim is to provide firms with incentives to hire those that otherwise would struggle to find non-subsidized jobs. From the perspective of the long-term unemployed, the subsidized job can be a stepping-stone towards a non-subsidized job. Workers hired through these subsidies are subject to exactly the same regulations (including employment protection laws) as non-subsidized

[2]Other papers studying spillover effects at the market level include, for instance, Blundell et al. (2004), Lise et al. (2004), Ferracci et al. (2014), Pallais (2014), Gautier et al. (2015) and Lalive et al. (2015). These studies use geographical variation and/or theoretical models to study spillover effects at a more general level, including market equilibrium effects. In contrast, we focus on the allocation of workers across firms and on how targeted wage subsidies affect firm performance.

workers.

We analyze two different subsidy systems. The first, the Employment Subsidy Program (Anställningsstöd) was in place between 1998 and 2006. The program was targeted *and* selective. It was mainly *targeted* to individuals unemployed for at least 12 months and at least 20 years old.[3] The program replaced 50 percent of the labor cost (including payroll taxes) for a maximum duration of 6 months. 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) shows that caseworkers sometimes initiate the subsidized match, 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 these (and other) programs. Taken together, this means that caseworkers influence how the subsidies are allocated to different firms and workers. We therefore refer to this subsidy system as the *staff-selection* system.

The second scheme we study is the "New Start Jobs program," introduced in January 2007. This program is targeted but not selective.[4] Similar to the staff-selection system, the new subsidies *target* individuals who have been unemployed for at least 12 months. However, the system is not selective since any worker who has been unemployed for at least 12 months during the last 15 months has the *right* to receive the subsidy if they find 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). Overall, if anything the New Start Jobs subsidies are more generous than those in the staff-selection system.

Thus, the main difference between the two policy systems is that the Employment Subsidy Program involves caseworker approval, whereas the New Start Job system does not. Under the new system, firms employing an eligible individual have the right

[3]Workers with special needs or workers with extensive unemployment histories may obtain a subsidized job before 12 months of unemployment.

[4]Note that the subsidy can be paid on top of the youth reduction in payroll taxes introduced in 2018 which was studied by Egebark and Kaunitz (2014) and Saez et al. (2017).

[5]Differently from the Employment subsidy program, the New Start Jobs subsidy does not require the individual to be registered as unemployed. Poor health, incarceration or other reasons for non-employment could suffice. This also implies that some subsidized jobs may start before 12 months of unemployment if these workers qualify through other types of non-employment, so that the 12 months eligibility threshold is not strictly binding.

to use the subsidy.[6] That is, caseworkers do not have to approve each subsidy, and in most cases they are not even involved in the allocation of the subsidy. Under the new system, 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, firms are solely responsible for initiating the procedures to apply for the targeted wage subsidy. Since the allocation of the subsidies is determined by the rules for the subsidy and not by caseworkers, we refer to this second program as the *rules-selection scheme*.

In both cases, firms hiring through subsidies are subject to the regulations as other hires in most other dimensions. As a consequence, the same employment protection laws apply to both the subsidized and the non-subsidized workers.

2.2 Conceptual differences between the two policy regimes

We will examine if the subsidies are targeted to low-performing firms and if they are associated with large windfall gains for employers, and if the empirical patterns related to these concerns differ between two different policy regimes. The first regime is a system with staff-selection, where subsidies have to be approved by a caseworker, and the second is the rules-selection regime where all unemployed job seekers are eligible for the subsidies.

The caseworkers' involvement can affect the allocation of workers across firms, either by not approving firms that merely use the subsidies to replace non-subsidized jobs and/or by allocating the subsidies such that the quality of the match between workers and firms is higher.[7] Caseworkers can thus affect sorting and selection which may lead to improved firm outcomes. This also implies that the setting may differ from the traditional evaluation one, in the sense that the role of sorting is interesting in itself. This also implies that positive outcomes arising from an allocation towards firms with a more positive forward trajectory is a legitimate successful outcome of the allocation process, at least from the perspective of the caseworker. However, we retain the terms

[6] The 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.

[7] Caseworkers' gatekeeper role within public employment offices has rarely been studied before, despite evidence of the importance of gatekeeper roles having been found in other public sector areas. For instance, Engström and Johansson (2012) and Markussen et al. (2013) show that medical doctors can act as gatekeepers in disability and sickness insurance systems.

treated and comparison/control to refer to firms hiring with and without the subsidies, respectively.

3 Empirical strategy and data

3.1 Data

We use data from several Swedish administrative registers. Data from the Swedish Public Employment Service provides information about all registered unemployed individuals. It contains detailed information about all individuals receiving targeted wage subsidies through our two systems (Employment subsidies and the New Start Jobs), including the start and the end date of each subsidy. By using unique personal and firm identifiers, this data is merged to a matched employer-employee database from Statistics Sweden (RAMS register).[8] This database contains information on all employment episodes for all employees in Sweden. Each employment episode is linked to the corresponding firm and provides us with information on yearly labor income and basic information about the firm. Using the matched employer-employee data we can follow firms and workers over time, which allows us to construct a firm level panel data set with information on the number of employees and the hiring and separation rates in each year.[9] We focus on both the total number of workers and the number of workers 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 also use information on firms' operating costs and profits, assets value, revenues, yearly turnover, investments, value added and other firms' production measures. This data is obtained from 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

[8]The PES data does not include information on the hiring firm, and the matched employer-employee data does not include information on the exact subsidy start date. 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.

[9]The number of hires is the number of workers employed in the firm during the current year who were not employed in the same firm the previous year. The number of separations corresponds to the number of workers employed in the firm the previous year but not the current one.

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. Worker productivity is defined as the total firm's valued added divided by the number of workers. 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, population registers from Statistics Sweden are used to construct information on the characteristics of the employees at the firm-year level. These include age, level of education, civil status, immigrant status and gender.

3.2 Sampling and comparison group

We compare firms recruiting through subsidies (defined as treated) to other observably identical firms. Let us illustrate the sampling procedure for treatments in year t. We first sample all firms with fewer than 30 workers in year $t-1$. The reason for this is one subsidized job constitutes a small treatment for large firms. We therefore focus on small- and medium-sized firms for which we expect to see effects. We also exclude firms with only one worker, and select the firms that survive until year t.[10] This implies that we observe at least one year of firm history.[11] Next, we use the PES information on the employment subsidies to identify firms with subsidized hires during the first quarter of year t. We focus on jobs starting during the first quarter both because our firm-level outcomes are measured on a yearly basis and in order to diminish the influence of short term-vacancies that are used across the summer.

We use the matched employer-employee data to sample firms observed during the 1998-2008 period. The justification for the 2008 restriction is that the subsidy rate was doubled for all new New Start subsidies starting in January 2009 and onwards. Moreover, by focusing on this time period we also avoid sampling firms during the great recession (the unemployment rate in Sweden started to rise during the first quarter of 2009, but the impact was much smaller than in Europe as a whole). For each firm

[10] In most cases firms with only one worker are firms where the owner is the only worker (self-employed). Most of these firms never intend to grow, therefore they are not at the risk of using the subsidies, which explains why we exclude them from our analyses.

[11] We 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 have used different firm size cutoffs and we have studied whether the treatment affects the probability that the firms grow to more than 60 workers but, reassuringly, we found no significant effects and tiny point estimates (-0.004 (se 0.003) and -0.003 (se 0.004) for the two regimes, respectively).

we only study the first wage subsidy within our observation period. This sampling procedure gives us 8,679 treated firms in the staff-selection system and 3,411 treated firms in the rules-selection system.[12]

As comparison group, we select firms that hire from the pool of long-term unemployed the same years and quarters, but without using the subsidy (not during the entire calendar year). We ensure that they have not hired with the subsidy in the past, but allow the comparison firms to use the wage subsidies in the future (5.3% of the comparison firms do this within 5 years). As for the treated firms, we focus on firms hiring from the pool of long-term unemployed in the first quarter of the year. A long-term unemployed is defined as an individual who finds a job after at least *six months* of unemployment according to the PES data. 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.[13] We repeat the sampling procedure each year, which means that a firm can be selected as comparison firm in multiple years.

For both types of subsidies the general rule is that the workers become eligible after 12 months of unemployment. However, we use a 6 months threshold for the comparison group to ensure that we use ineligible, but otherwise similar, workers. Since workers hired after 6 months should have more favorable unobserved characteristics than workers hired after more than 12 months of unemployment, any positive estimates for the subsidies should be considered as "conservative" (i.e. biased towards zero). Note however that, as discussed in Section 2, the 12 months eligibility criterion is not strictly binding (in any of the two regimes) so the treatment group does include some firms which hire workers after less than 12 months of unemployment. To ensure that these choices are not driving our results, we present a robustness analysis where we control for the elapsed unemployment duration (and other characteristics) of the hired workers, leading to very similar results.

In the staff-selection system, the comparison group includes both firms to which the caseworkers actively deny a subsidy and firms which hire a long-term worker without

[12]Note that the number of subsidies are slightly higher in the rules-selection regime (1700 per year) than for the staff-selection regime (960 per year). However, note that these numbers are small compared to the total number of firms in Sweden, so that it is unlikely that this difference between the two regime lead to differential general equilibrium effects.

[13]Note that the comparison group is made up by workers who are not formally entitled (yet) and those that are formally entitled but are not selected by caseworkers in the caseworker regime and those that choose not to participate in the rules selection regime. One reason for failing to use the subsidy when entitled is the stigma effect discussed by, e.g. Neumark (2013).

making a subsidy claim, potentially because the preceding spell was too short. We cannot separate between these groups of firms. Similarly, in the rules-selection system the comparison group includes firms that do not use the subsidy despite being entitled to do so (e.g. because of not understanding the rules, or in case the hired worker does not disclose the duration of joblessness) and firms that hire a worker whose preceding spell was too short. In most of our specifications, we exclude disappearing firms from the year they disappear, but we also examine effects on firm survival and we are careful to take such effects into account when we interpreting our results.

3.3 Raw sample statistics

Table 1 provides summary statistics for the firms in our sample, but it also contains one of the key findings of this paper. In fact, the most striking feature of the table, in our view, is that with very few exceptions the treated firms (hiring with subsidies) are quite similar to the firms that hire unsubsidized long-term unemployed workers. Moreover, with one exception only (age of the hired worker), selection (on observables) is very similar between the staff-selection and rules-selection regimes.

Panel A of the table shows the industry composition. The treated firms are somewhat more likely to be in the manufacturing industry and wholesale/retail but for other industries, differences are small. Selection on all variables is very similar between the staff-selection and rules-selection regimes. Panel B turns to the employee-composition of the hiring firms. These statistics are again remarkably similar between the treated and controls considering that these are raw data generated by self-selection. The one statistic where there are some differences the share of high educated, which is somewhat lower within the treated firms. The time trends of increasing education and increasing shares of immigrants *between* the two regimes are visible but the within-period selection is very similar for the two regimes.

Panel C shows statistics for the hired workers. The main difference between treated and controls is that the subsidies target workers with much longer unemployment spells on average. This is true by design since we only require the control firms to hire workers who have been unemployed for at least 6 months. But despite this difference, we find rather similar age profiles and shares of immigrants (although higher in the second regime as expected due to low skilled immigration; we will return to this issue). The only notable difference between the treated and comparison firms is that the share of workers below 25 is higher among the treated firms in both regimes. We also see a shift from under-representation among the treated within the oldest group (55-64) to an over-representation of treatment within the same age group. We will explore these differences in several ways.[14] Panel C also shows that education is somewhat lower and the share of males is higher for the treated.

The statistics in Panel C are relative to the subsidized workers and the long-term unemployed workers hired by the treated and comparison firms, respectively. Besides these workers, the two groups of firms may also hire other workers (non-subsidized) during the treatment year. Sample statistics for these workers are presented in Table A2 in the appendix. Here, we find very similar age and education profiles for the treated and comparison firms in both subsidy systems.

Panel D shows the pre-treatment outcomes of the hiring firms. Here we see somewhat larger differences, but as we will show in the results section below, they all essentially reflect the same underlying variable, namely that treated firms tend to be smaller than the comparison ones. Note that we focus on firms with fewer than 30 employees, which explains why the average firm size is rather small. Figure 1 shows the average number of workers in the treated and comparison firms within five years since the start of the subsidy, in the staff-selection system. Year zero is the year the subsidy starts or, for the comparison firms, the year they hire a long-term unemployed worker without a subsidy. From the figure we see that although the comparison firms are on average somewhat larger than the treated firms, the trends for the two groups are very similar. For both treated and comparison firms, the average number of workers remains roughly constant before the subsidy. Since we sample firms hiring at least one worker in year zero, we observe a jump in firm size in year zero for both groups. After this, firm size decreases over time, consistently with regression towards the mean. Figure 2 shows similar patterns for the rules-selection system.

3.4 Matched samples

We believe that the statistics presented above (in particular, the size trends) are reassuring in terms of the basic approach of comparing treated and comparison firms to assess the impact of the subsidies. However, to ensure that we purge our comparison from any additional differences in observables, we use a matching algorithm. We select one comparison observation for each treated observation using nearest-neighbor

[14]In one robustness analysis, we match on all worker characteristics, and in another robustness analysis we exclude the oldest and the youngest workers. In both cases without any change in results.

propensity-score matching. Our matching vector includes the following variables (described in Table 1): industry dummies (8 categories), firm size, wage sum, number of separations as well as firm-level employee composition as captured by the variables in Table 1, Panel B. We perform the matching procedure separately for each calendar year (thus, also by subsidy scheme), and aggregate the data into two matched samples, one for the staff-selection system and one for the rules-selection system.

Figure 3 illustrates the matched treated and comparison firms in the staff-selection system. 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.[15] More importantly, 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. We obtain similar results for the rules-selection system (Figure 4).

Differences between treated and matched controls in number of employees, wage sum and separations within a 5-year pre-match period are shown in Table A.1. To assess the usefulness of the matching protocol, we also check for pre-treatment differences in firm-performance measures that we do *not* match on. To this end, Table A.1 reports balancing tests for average profits, log value added and investments, as well as these three outcomes measured per worker, in the pre-hiring period up to five years before the subsidy. We also report statistics on the fraction of the firms that existed in the 5-year period before the treatment. Even if we do not match on these variables, we find very small differences between the treated and the comparison firms. This holds both for the staff-selection system (columns 1-3) and the rules-selection system (columns 4-6). The fact that we find similar pre-treatment trends also for these variables suggests that our matching protocol does produce control firms with a very similar history as the treated firms, also in terms of unobserved dimensions. In the robustness section, we provide estimates when matching on the characteristics of the hired worker, and when matching on a broader set of firm outcomes in levels and trends. As expected from the balancing tests described here, results are robust.

3.5 Empirical model

Our analysis relies on comparing treated and comparison firms' outcomes using the matched samples. However, since we observe each cross-sectional unit over time, we

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

can further strengthen the analysis by applying panel data methods to control for any group-specific differences not accounted for in the matching step. Thus we can adjust for all observed and unobserved fixed characteristics by estimating the following baseline model for firm i in year t:

$$y_{it} = \lambda_t + \beta D_i + \gamma(D_i \cdot T_{it}) + \varepsilon_{it}, \tag{1}$$

where λ_t is a year dummy, D_i is an indicator variable for firms in the treated samples and T_{it} is an indicator variable taking the value 1 after the start of the subsidy in this set of firms. Thus, D_i captures any remaining time-constant pre-existing differences between matched treated and comparison firms. In our robustness analyses, we also use firm fixed effects. The interaction $D_i \cdot T_{it}$ reflects any difference between the two groups after the start of the subsidy. We allow this difference to vary by time since the start of the subsidy. Model (1) is estimated separately for each subsidy system. Standard errors are clustered at the firm level.[16]

4 Results

4.1 The staff-selection system

We first focus on the staff-selection system, during which all subsidies need to be approved by caseworkers. Figure 5 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 noted, there are virtually no differences between treated and comparison firms in the pre-subsidy period. In the subsidy year, 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. At the same time, the total number workers is almost unaffected. This happens because the comparison firms also hire at least one worker in year zero. After this, we see a gradually increasing positive difference between the average firm size of the treated and comparison firms. Five years after the start of the subsidy, the difference is around 0.5 workers. Since the average

[16]Note that this procedure does not into account that there is sampling variation in the matching step. Addressing this issue properly involves a large computational burden. We therefore provide estimates for given matched samples and have validated the most important results by performing genuine conditional difference-in-differences, using nearest neighbour Mahalanobismetric matching and the Abadie and Imbens (2006) estimator of the standard errors. This resulted in very similar standard errors.

firm size in our sample is just below ten workers, the magnitude of this difference is far from trivial.

Figure 5 also reveals to what extent the observed differences between treated and comparison firms are due to the number of subsidized workers and/or due to the number of non-subsidized workers. Individuals hired with a subsidy are counted as subsidized workers throughout the remainder of their job spell.[17] Unsurprisingly, over time the number of subsidized workers decreases since some of them leave the firm, reflecting standard firm turnover in the labor market. Five years after the subsidy start, roughly 50% of the workers remain in the firm (around 0.5 workers). This number is almost identical to the difference in the total number of workers between treated and comparison firms. We conclude that the subsidies in the staff-selection system create *net* employment, and that the subsidized workers who remain in the firm do not replace other workers.

In Panel A of Table 2, we analyze the impact on various outcomes using the regression model presented in equation (1). In Column 1, we first examine if the effects on firm size are driven by differential firm survival, but we see no impact on the probability to remain in business. The table also reports estimates for several other firm performance outcomes. Column 2 repeats the results for firm size already highlighted in Figure 5. As expected, we obtain a similar pattern and the differences between treated and comparison firms both 1-2 and 3-5 years after the start of the subsidy are statistically significant. In Column 3, we study effects on the yearly wage sum. Although estimates are less precise, we obtain a similar pattern as for the number of workers.

A reasonable concern at this stage is that the increased number of workers could have a negative impact on productivity. We therefore turn to the impact on firm performance measures. Column 4 reveals significant positive effects on profits. This may partly be a mechanical effect due to the subsidy. In Column 5, we show that the size effect is also visible in terms of production (log value added), which is reassuring. But, more importantly, we also want to assess the impact on *productivity per worker*. To this end, Column 6 studies the impact on log value added per worker. The results in fact suggest that productivity increases by 3 percent as a result of the subsidy. Thus, the faster size growth in the treated firms does not come at the cost of decreased per-worker

[17] The number of subsidized workers includes everyone hired using a subsidy, including both currently subsidized workers and workers who remain in the firm after the subsidy has expired. Very few firms in our sample use the subsidies more than once.

productivity, but rather the reverse. This is perhaps even more surprising considering that the treated firms hire workers with twice as long elapsed unemployment duration as the control firms (see Table 1).

4.2 The rules-selection system

In the rules-selection system, caseworkers' involvement in the match creation greatly diminishes. First, we show in Figure 6 the difference in the total number of workers and the number of subsidized workers between treated and comparison firms. As for the staff-selection system, the number of subsidized workers increase by roughly one unit in the subsidy year and subsequently declines to about 0.5 workers five years after the subsidy. In contrast to the staff-selection results, we find no differences in size between treated and comparison firms during the follow-up period.

Results in table format are presented in Panel B of Table 2. Interestingly, we find a significant positive effect on firm survival that we do not see for the staff-selection system. Two years after the subsidy, the treated firms are 2 percentage points more likely to remain in business than the control firms. Since we find no evidence in this direction during the staff-selection period, the results appear to suggest that the caseworkers may have reduced the exposure to wage subsidies of firms that are on the verge of collapsing. It also suggests that the rules-selection subsidies have a positive effect on employment through reduced firm closures whereas the staff-selection subsidies had a positive employment effect through the performance of the survivors.

In Column 2, we repeat the analysis for number of employees, finding very small (insignificant) estimates both 1-2 and 3-5 years after the treatment. This pattern holds for all the other outcome variables shown in the table (Column 3 wage sum, Column 4 profits, Column 5 production and Column 6 productivity).

4.3 Comparison between the two subsidy schemes

We now turn to a more explicit comparison between the two systems. We use the matched samples and show separate estimates for each year before and after the long-term unemployed hire. Figure 7 shows the estimates for the total number of workers for each system (with 95% confidence intervals). As already stressed, for both systems there are no significant pre-treatment trends. Moreover, the figure confirms the striking differences between the two systems. During the staff-selection system, the subsidies lead to increased employment, while during the rules-selection system there is no effect on the total number of workers. This pattern holds despite the fact that the subsidized workers tend to stay in the firm to the same degree in the two systems. In both Figures 5 and 6, we showed that around half of the subsidized workers remain in the firm five years after the start of the subsidy. Thus, the differences in total number of workers across systems is due to the number of non-subsidized workers.

To highlight that this result is unlikely to be due to random variation, Figure 8 shows estimates for each pair of two contiguous calendar years (using number of employees as the outcome). As expected, we observe some non-trivial variability in the estimates but the long-run staff-selection estimates remain distinctly positive (red dots), whereas the estimate is at zero for the rules-selection hirings (blue triangle).

4.4 Robustness and alternative interpretations

Table 3 presents results from several robustness analyses with our baseline results for the number of workers in Column 1. Column 2 reports estimates when we add firm fixed effects to the baseline specification, instead of fixed effects for the two groups (treated and comparison firms). For neither of the two systems does this change our conclusions. There are positive effects for the staff-selection system but not for the rules-selection system. In Column 3, we include characteristics of the hired worker when we match treated and comparison firms (we use the characteristics shown in Panel C of Table 1, except for unemployment duration). When we in these ways adjust for differences in workers characteristics we still obtain very similar results as in our main analyses. Next, Column 4 adjusts for unemployed workers' time in unemployment before the start of the job in the matching step. That is, treated firms hiring a subsidized worker after 7 months are compared to comparison firms hiring a long-term unemployed worker after 7 months of unemployment, and so on. This adjusts for any additional differences between the subsidized workers hired by the treated firms and the long-term unemployed workers hired by the comparison firms. Again, this leads to similar results as on our baseline analysis. All this suggests that the composition of workers does not drive our results.

In Columns 5–7, we match on larger sets of firm outcomes. Column 5 shows estimates when we add profits and value added, as well as all firm outcomes two years before the subsidy. In Column 6, we also match on pre-treatment investments one and two years before the subsidy, and Column 7 reports estimates adjusting for the pre-treatment hiring rate one and two years before the subsidy. Here, the hiring rate is defined as the number of workers hired in the treatment year.[18] This way, we adjust for a large set of pre-treatment levels and trends in key dimensions. These robustness estimates are all similar to our baseline specification.

In a final robustness analysis, we exclude the oldest workers (above 54) and the youngest workers (below 25), because the sample statistics showed differences between treated and comparison firms in the fraction of young and old workers. Again, the results are very similar to our baseline results.

In order to shed more light on the allocation process, we split the sample into small firms (fewer than 10 workers in year zero) and medium-sized firms (10-30 workers). Interestingly, the results presented in Columns 9 and 10 of 3 reveal somewhat larger effects for the small firms; 3-5 years after the subsidy the effect is 0.63 workers for the small firms and 0.45 workers for the medium-sized firms. Notably, in relative terms, the difference is even more pronounced as small firms by definition have fewer employees to start with.

We now turn to exploring additional alternative interpretations of the differences between the two systems. In our analyses, we compare periods with partly different business cycle conditions and the unemployment rate at the time of the subsidized hiring may affect the impact of wage subsidy. To examine whether this affects our findings, Columns 1 and 2 of Table 4 report estimates for firms hiring under different business cycle conditions defined by high (above the median) vs. low (below the median) national-level unemployment rates during our sampling period. The results for the two systems are similar to those in our main results.

Another interesting aspect is the Great Recession which lead to increased unemployment rates in Sweden from the first quarter of 2009. Even though the impact of the Great Recession on the Swedish labor markets was, in fact, not particularly great (unemployment rate was 6.2% in 2007 and 8.6% in 2010), it may still affect our results since the effects in the medium-run for rules-selection system (firms sampled in 2007–08) are identified during the recession. To explore this, we split the sample by the unemployment rate four years after the treatment year. The idea is to compare the effects of subsidized jobs in firms in the two systems that face the same type of business conditions in the medium-run. The estimates from the two different samples reported in Columns 3 and 4 of Table 4 show that this does not change the interpretation of

[18]We have also explored specifications where we adjust for for the share of hired workers and the number of ineligible workers (number of non-subsidized workers). Again, this leads to similar results.

our results. The only notable difference is that for the staff-selection regime the effect for high unemployment periods are insignificant but the point estimate is very close to that for the low unemployment period.[19] This provides suggestive evidence that the Great Recession cannot explain our findings. Also, note that we compare firms that hire workers with and without subsidies (equally affected by seniority rules), and that our outcomes are at firm (not worker) level.

Next, we explore the impact of the rising share of immigrants amongst the unemployed. Since there are more immigrants in the unemployed pool during the more recent rules-selection system, our findings may be sensitive to differences in effects between immigrants and natives. To test for this, we split the samples into firms hiring natives and immigrants.[20] The results are presented in Columns 5 and 6 of Table 4. The patterns appear robust, in particular if we focus on the impact on natives. As expected, the estimates become very imprecise for immigrants, in particular during rules-selection when the sample gets very small.

One possible interpretation of our main findings is that caseworkers are able to target firms that are or expect to grow faster in the future, despite identical pre-treatment trends. If so, the subsidies are allocated to firms that would have outperformed the comparison firms regardless of the subsidy. As already documented in both Table A.1 and Figures 3 and 4, the treated and comparison firms have very similar pre-treatment trends (both before and after matching the data), including in dimensions that we do not match on (most importantly profits and production). But this does not completely rule out the possibility of differences in forward-looking expectations. Hence, instead of solely focusing on pre-treatment trajectories, for a much more direct test we use data on investments. The idea is that investments capture expectations about future outcomes and forward-looking attitudes. We therefore study how the yearly net investments in machinery and land differ between the treated and comparison firms in the subsidy year and the year before the subsidy.

The estimates, provided in Table 5, reveal no significant differences in the staff-selection system. This result suggests that the fact that treated firms outperform comparison firms is not explained by caseworkers targeting firms with better forward-looking expectations, as captured by investments at least. In the rules-selection system

[19] We have also divided the sample by the unemployment rate three and five years after the treatment leading to the same conclusions.

[20] For the few cases of multiple hirings, we use the modal immigrant status type of the hires, giving priority to migrants in case of ties.

the results are very different, however. The evidence suggests *lower* investments among the treated firms in the subsidy year. Comparing across the two regimes, the results thus suggest that caseworkers are able to select away firms with lower-than-average future expectations and investment rates. These businesses are instead more likely to use the subsidies during the rules-selection setting.

5 Summary and conclusions

In this paper we study how targeted wage subsidies schemes are related to firm performance. We find that subsidies can have a very positive sustained effect on a range of firm production and productivity measures, including firm size, wage sum, profits, value added and per-worker productivity. This is robustly true in the setting (before 2007) when caseworkers needed to approve all subsidies.

However, the patterns are less robust after 2007 when caseworkers no longer were involved in the allocation process. Instead, results turn much smaller and, with two exceptions, statistically insignificant for subsidies falling under the rules-selection regime. In this period, the impact on firm survival is positive. In addition, treated firms have lower-than-average investments. A possible interpretation of these changed patterns is that caseworkers during the staff-selection regime prevented firms with poor expectations from receiving subsidies, a process which may have reduced the impact on the firm-survival margin if this process kept marginal firms from seeking treatment as a last resort. We try to test for alternative explanations, including those related to the business cycle (although the "Great recession" was quite mild in Sweden) and find no support for the alternative explanations, but we acknowledge that we cannot fully rule out that other factors contributed to the change in responses.

Overall, however, we do believe that our results should be interpreted as suggesting that our Swedish targeted wage subsidies in fact have not allocated subsidies to poor performing firms, at least during the period when caseworkers acted as gatekeepers. The starkest result of our paper is the relatively strong post-match performance of the treated firms during this period. But it should also be noted that surviving firms who hire through subsidies, even during the period *without* caseworker approval, appear to perform at least as well as other firms that hire unemployed workers.

Our paper adds to the growing, but still relatively scarce, literature on how ALMPs affect firm-level performance, employer-employee sorting, and the interplay between the two. Thus providing evidence in line with the recent call by Card et al. (2018) for more research on how public policies affect the allocation of workers across firms. The policy relevance of the results is apparent. The results suggest that i) concerns that targeted wage subsidies allocate resources to bad firms may be unwarranted and that ii) policy-makers who are worried about displacement effects may want to consider ensuring caseworkers' approval of targeted wage subsidies since our results were unanimously positive during the period with caseworker approval.

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Tables and Figures

Table 1 – Sample statistics for treated and comparison firms in the two regimes

	Staff selection		Rules selection	
	Treated firms	Control firms	Treated firms	Control firms
Group size	8,679	25,322	3,411	4,798
Panel A: Industries				
Agriculture	0.04	0.03	0.02	0.03
Manufacturing	0.19	0.13	0.20	0.12
Construction	0.11	0.10	0.11	0.10
Wholesale and retail trade; repair	0.28	0.24	0.26	0.22
Accommodation and food service	0.07	0.09	0.10	0.13
Transport and storage	0.06	0.09	0.06	0.08
Real estate activities	0.16	0.20	0.16	0.19
Education	0.02	0.02	0.01	0.04
Human health and social work	0.02	0.03	0.02	0.03
Panel B: Pre-treatment average workers' characteristics				
Married	0.38	0.38	0.38	0.37
Male	0.68	0.62	0.68	0.62
Immigration to Sweden	0.12	0.16	0.21	0.24
Education: Compulsory	0.25	0.24	0.22	0.22
Education: Secondary	0.58	0.55	0.57	0.53
Education: Upper	0.17	0.22	0.21	0.26
Age: 24 or less	0.19	0.18	0.18	0.19
Age: 25–34	0.28	0.28	0.23	0.26
Age: 35–44	0.24	0.23	0.25	0.24
Age: 45–54	0.18	0.18	0.18	0.17
Age: 55–64	0.10	0.11	0.13	0.12
Age: 65 or more	0.01	0.01	0.02	0.02
Panel C: Hired workers' characteristics				
Age: 24 or less	0.26	0.11	0.17	0.11
Age: 25–34	0.32	0.33	0.25	0.31
Age: 35–44	0.21	0.26	0.23	0.26
Age: 45–54	0.14	0.18	0.18	0.19
Age: 55–64	0.07	0.12	0.18	0.13
Age: 65 or more	0.00	0.00	0.00	0.01
Immigration to Sweden	0.21	0.22	0.32	0.30
Married	0.29	0.35	0.36	0.35
Male	0.69	0.59	0.66	0.58
Education: Compulsory	0.20	0.20	0.21	0.20
Education: Secondary	0.66	0.59	0.59	0.55
Education: Upper	0.15	0.20	0.21	0.25
Average unemployment (days)	660.88	410.98	638.13	371.02
Panel D: Pre-treatment firm outcomes				
No. of workers	9.70	11.09	10.08	11.66
Wage sum per worker	109.11	107.19	124.38	119.93
Hirings rate	0.28	0.30	0.30	0.31
Separations rate	0.23	0.26	0.22	0.25
Value added per worker	385.35	410.55	426.90	439.16
Operating profit per worker	74.96	76.78	91.56	79.71
Total investments	228.71	206.45	163.53	175.60
Investments per worker	52.69	44.53	37.39	41.21

Notes: Sample statistics for treated and comparison firms before matching. Panel A: share of firms hiring in each industry; Panels B, D: pre-hiring averaged workers' characteristics; Panel C: hired workers' demographics and residual time in unemployment before exiting to job. Panel D: all monetary values are inflation-adjusted (base year: 2000), and all outcomes normalized by firm size. Wage sum is the yearly sum of wages paid by the firm. Value added is total revenues minus costs of intermediate goods. Operating profit is the difference between operating revenues and expenses, minus depreciation and amortization.

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

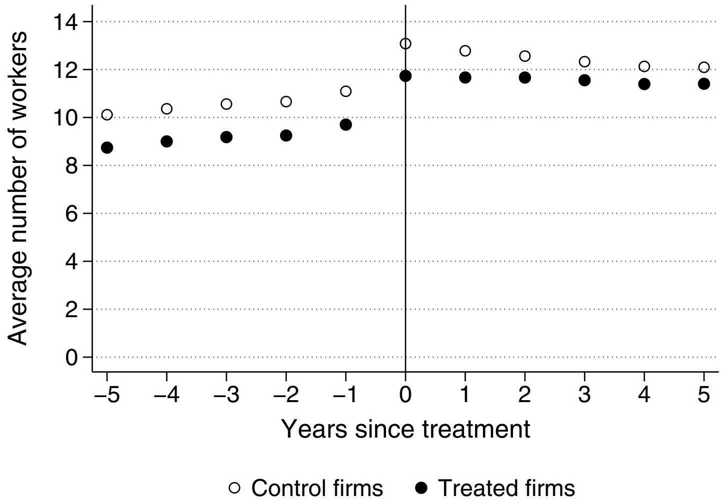

Figure 2 – Number of workers for treated and comparison firms, before matching (rules-selection system)

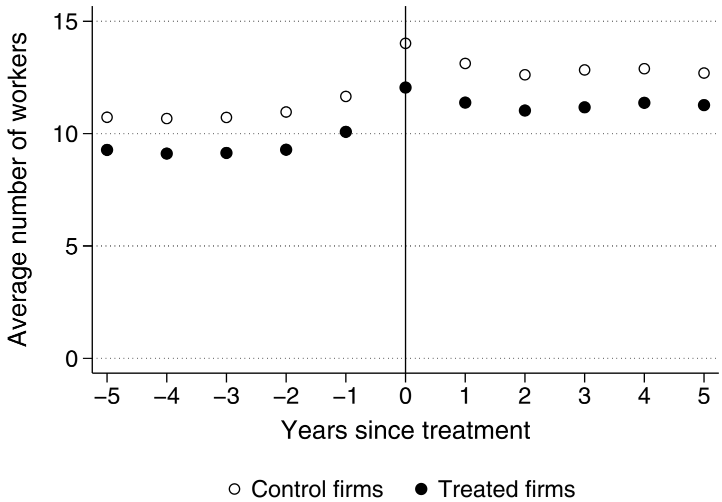

Figure 3 – Number of workers for treated and comparison firms, after matching (staff-selection system)

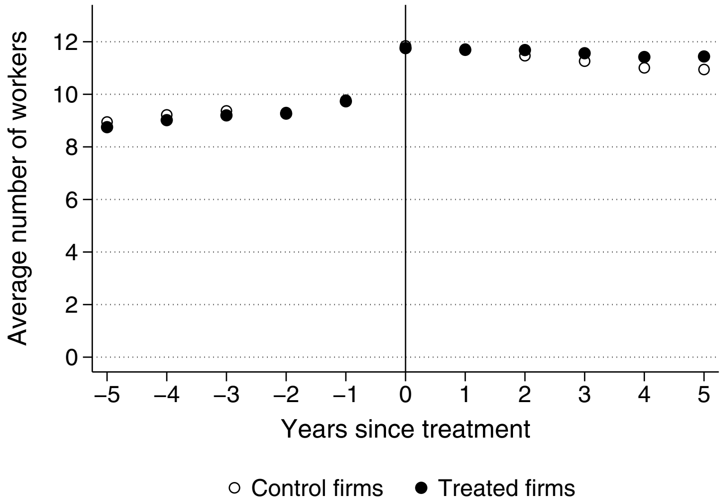

Figure 4 – Number of workers for treated and comparison firms, after matching (rules-selection system)

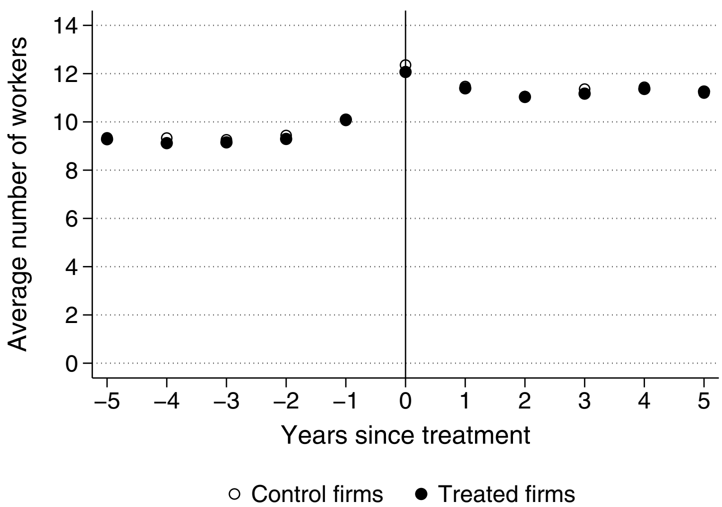

Figure 5 – Difference treated and comparison firms, staff-selection system

Figure 6 – Difference treated and comparison firms, rules-selection system

Table 2 – Estimates for firm–level outcomes by years since treatment

	Firm survival (1)	No. of workers (2)	Wage sum (3)	Profits (4)	Value added (5)	Value added per worker (6)
Panel A: Staff selection						
Year of treatment	–	0.03	26^{*}	35	0.06^{***}	0.01
		(0.11)	(14)	(33)	(0.01)	(0.01)
1–2 years after treatment	-0.0001	0.21^{*}	20	64^{*}	0.09^{***}	0.03^{***}
	(0.0026)	(0.12)	(20)	(34)	(0.02)	(0.01)
3–5 years after treatment	0.0039	0.52^{***}	55^{*}	116^{***}	0.09^{***}	0.03^{***}
	(0.0034)	(0.15)	(29)	(42)	(0.02)	(0.01)
Average	0.7721	11.48	1731	482	7.56	6.02
No. of observations	86,020	157,758	157,758	121,376	127,104	119,580
Panel B: Rules selection						
Year of treatment	–	-0.20	28	-7	-0.01	-0.02
		(0.19)	(30)	(62)	(0.03)	(0.02)
1–2 years after treatment	0.0200^{***}	0.05	13	66	0.03	-0.00
	(0.0039)	(0.21)	(41)	(65)	(0.03)	(0.02)
3–5 years after treatment	0.0433^{***}	0.02	7	-60	0.03	-0.01
	(0.0053)	(0.26)	(63)	(76)	(0.04)	(0.02)
Average	0.7826	11.26	2081	584	7.68	6.13
No. of observations	33,970	62,807	62,807	52,139	50,741	47,195

Notes: Estimates using the matched samples. Each model includes calender time fixed effects and indicators for treatment status. Average outcomes computed 3–5 years after treatment. Number of observations corresponds to the observed firm history years for Columns 2–5 and to the post–treatment period years for Column 1. Wage sum (in 1000 SEK) is the sum of all wages paid by the firm during the calendar year. Total value added (in log 1000 SEK) is total revenues minus intermediate consumption of goods and services. Profits (in 1000 SEK) are the difference between operating revenues and operating expenses, minus depreciation and amortization. Value added per worker is the logarithm of total value added divided by firm size. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Figure 7 – Estimates for total number of workers, comparison of the two systems

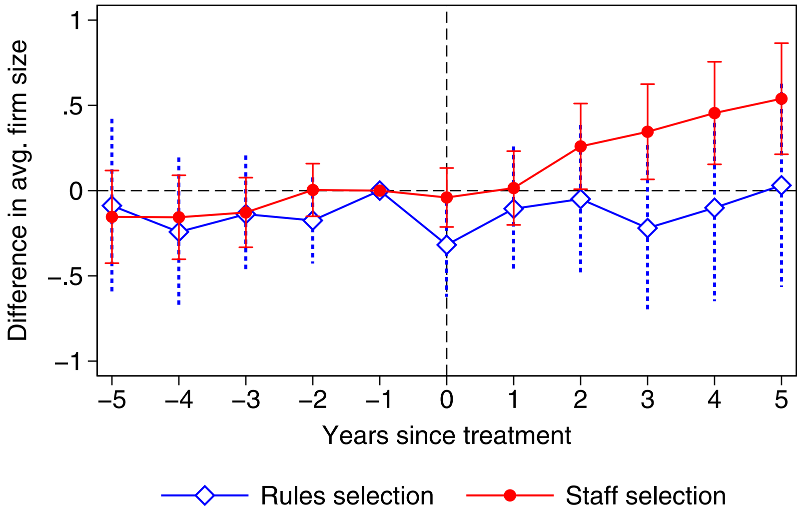

Figure 8 – Estimates for number of workers by calendar year

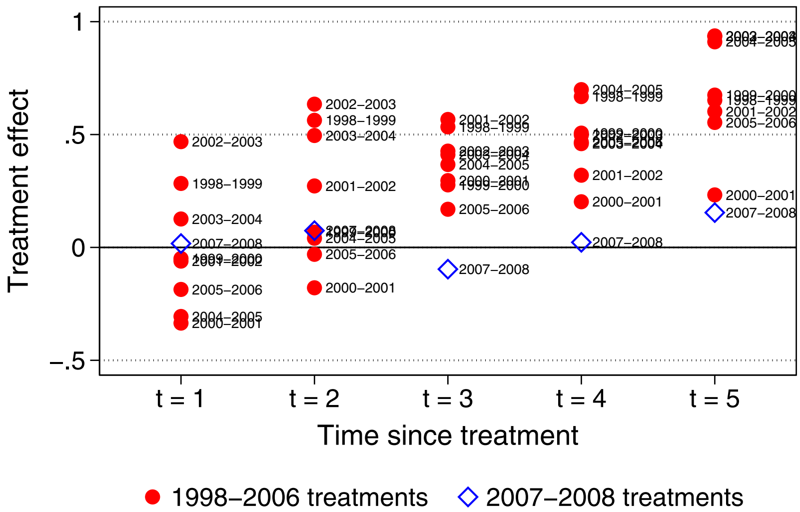

Table 3 – Estimates for number of workers, different specifications

	Baseline (1)	Firm FE (2)	Match workers characteristics (3)	Match time unemployed (4)	Additional controls (5)	Match investments (6)	Match hirings (7)	Age 25–54 (8)	Small firms (9)	Medium firms (10)
Panel A: Staff selection										
Year of treatment	0.03	0.08	0.00	0.12	0.03	0.01	-0.03	0.02	0.06	0.10
	(0.11)	(0.10)	(0.12)	(0.12)	(0.11)	(0.11)	(0.11)	(0.14)	(0.11)	(0.21)
1–2 years after treatment	0.21*	0.21*	0.33**	0.30**	0.17	0.25**	0.19	0.24	0.25**	0.19
	(0.12)	(0.12)	(0.13)	(0.14)	(0.12)	(0.13)	(0.13)	(0.15)	(0.12)	(0.23)
3–5 years after treatment	0.52***	0.52***	0.57***	0.45***	0.56***	0.50***	0.47***	0.44**	0.63***	0.45*
	(0.15)	(0.14)	(0.15)	(0.17)	(0.15)	(0.15)	(0.15)	(0.19)	(0.14)	(0.27)
Average	11.48	11.48	11.48	11.76	11.48	11.48	11.48	11.53	7.05	17.92
No. of observations	157,758	157,758	157,372	112,291	157,926	158,162	158,139	101,797	91,455	64,934
Panel B: Rules selection										
Year of treatment	-0.20	-0.07	-0.15	-0.01	-0.16	-0.23	0.03	-0.18	-0.20	-0.30
	(0.19)	(0.18)	(0.19)	(0.21)	(0.19)	(0.20)	(0.19)	(0.22)	(0.19)	(0.33)
1–2 years after treatment	0.05	0.20	-0.02	-0.12	-0.12	-0.18	0.20	-0.02	-0.23	0.41
	(0.21)	(0.19)	(0.21)	(0.25)	(0.20)	(0.20)	(0.20)	(0.24)	(0.21)	(0.37)
3–5 years after treatment	0.02	0.21	-0.03	-0.08	-0.09	-0.14	0.17	-0.22	-0.14	0.23
	(0.26)	(0.24)	(0.28)	(0.32)	(0.27)	(0.27)	(0.26)	(0.34)	(0.27)	(0.45)
Average	11.26	11.26	11.27	11.54	11.26	11.26	11.26	11.16	6.45	17.63
No. of observations	62,807	62,807	62,952	42,041	63,261	62,657	62,841	39,324	35,410	27,271

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): baseline specification augmented with firm fixed effects; Column (3): individual–level demographics of the hired workers added to the baseline PS specification; Column (4): results when sampling all PES unemployment spells longer than 30 days – with exit to either unsubsidized or subsidized job – and then matching treated and controls hirings based on residual time registered at the PES as unemployed (discretized through 36 monthly- and 4 biannual-dummies); Column (5): baseline PS specification augmented with (i) profits, log value added and per–worker productivity measured the pre–treatment year and with (ii) the change in these quantities as well as in firm size and wage sum between -2 and -1.; Column (6): baseline PS specification augmented with log net investments per worker in -1; Column (7): baseline PS specification augmented with hirings level from $t = -1$ to $t = 0$; Column (8): restrict to firms hiring 25 to 54-year old unemployed; Column (9): matched sample of firms having less than 10 employees the pre–treatment year; Column (10): matched sample of firms having 10 to 30 employees the pre–treatment year. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Table 4 – Firm size regressions by unemployment rate and immigrant status

	Unemployment rate hiring year		Unemployment rate 4 years since hire		Immigrant status	
	Low	High	Low	High	Native	Immigrant
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Staff selection						
Year of treatment	0.14	0.02	0.04	0.03	0.09	-0.26
	(0.24)	(0.12)	(0.12)	(0.30)	(0.12)	(0.25)
1–2 years after treatment	0.31	0.19	0.20	0.30	0.40***	0.03
	(0.27)	(0.14)	(0.13)	(0.37)	(0.14)	(0.29)
3–5 years after treatment	0.51	0.52***	0.52***	0.55	0.59***	0.34
	(0.34)	(0.16)	(0.16)	(0.41)	(0.17)	(0.36)
Panel B: Rules selection						
Year of treatment	-0.25	0.07	-0.10	-0.21	0.03	0.12
	(0.21)	(0.45)	(0.51)	(0.20)	(0.21)	(0.37)
1–2 years after treatment	0.04	0.23	-0.24	0.03	-0.00	0.27
	(0.23)	(0.51)	(0.60)	(0.21)	(0.25)	(0.42)
3–5 years after treatment	0.05	0.02	-0.36	0.05	0.27	0.23
	(0.29)	(0.60)	(0.96)	(0.25)	(0.33)	(0.53)

Notes: Columns (1) and (2) show results for firm size regressions by partitioning firms as hiring when the monthly unemployment rate is above or below the 1998–2008 median national level, respectively. In columns (3) and (4) firms are partitioned according to the yearly unemployment rate 4 years since treatment as compared to the 1996–2012 median national level. Columns (5) and (6) report the coefficients for firms hiring native or immigrant long–term unemployed. All regressions include year fixed effects and use the matched sample. Standard errors clustered at firm level in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Table 5 – Effects on firms investments; matched sample

	Net investments per worker	
	logs	level
Panel A: Staff selection		
Pre–treatment year	0.02	-2.39
	(0.04)	(5.82)
Year of treatment	0.06	12.78
	(0.04)	(11.17)
Panel B: Rules selection		
Pre–treatment year	-0.10	-15.63
	(0.07)	(9.65)
Year of treatment	-0.18**	-17.12*
	(0.07)	(9.01)

Notes: Firm investments regressions using the matched sample. The outcomes are defined considering the yearly amount invested in machinery and land 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

Table A.1 – 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: outcomes matched in $t-1$						
No. of workers						
$t-5$	8.75	8.95	−0.20	9.28	9.34	−0.06
$t-4$	9.02	9.22	−0.20	9.12	9.33	−0.21
$t-3$	9.20	9.37	−0.17	9.15	9.26	−0.11
$t-2$	9.27	9.30	−0.04	9.29	9.44	−0.14
$t-1$	9.73	9.78	−0.04	10.09	10.06	0.03
Wage sum per worker						
$t-5$	104.44	104.11	0.33	122.43	124.80	−2.36
$t-4$	106.13	106.81	−0.68	123.36	124.25	−0.89
$t-3$	110.03	108.01	2.02	125.00	125.75	−0.75
$t-2$	111.53	110.31	1.22	126.00	126.46	−0.46
$t-1$	109.59	107.82	1.76	124.62	122.14	2.48
Separations						
$t-5$	0.24	0.26	−0.02	0.26	0.28	−0.02
$t-4$	0.25	0.25	0.00	0.26	0.27	−0.01
$t-3$	0.24	0.24	−0.01	0.26	0.24	0.01
$t-2$	0.24	0.26	−0.02*	0.23	0.26	−0.02*
$t-1$	0.23	0.23	−0.01	0.22	0.23	−0.01
Panel B: outcomes not matched						
Profits (Th. SEK)						
$t-5$	368.85	401.72	−32.86	322.62	344.51	−21.90
$t-4$	302.34	382.75	−80.41	323.72	314.14	9.58
$t-3$	281.52	371.97	−90.46	344.66	369.42	−24.75
$t-2$	299.03	337.15	−38.12	403.10	355.92	47.18
$t-1$	332.20	386.26	−54.07	465.85	437.44	28.42
Profits per worker						
$t-5$	96.59	100.73	−4.14	79.22	95.40	−16.18
$t-4$	80.81	88.55	−7.74	75.07	76.25	−1.18
$t-3$	70.67	89.00	−18.33	84.36	87.27	−2.91
$t-2$	74.96	77.32	−2.35	88.93	77.96	10.98
$t-1$	74.83	83.94	−9.11	91.56	84.11	7.45
Log value added						

Continue to next page

Pre–treatment average outcomes matched in $t-1$ (Panel A) or not matched (Panel B), where t is the time when the firm hires. All firm–level outcomes computed using the matched samples. Monetary values are inflation-adjusted (base year: 2000). Separations are normalized by firm size. Wage sum and total profits measured in 1000 SEK, total firm value added in log 1000 SEK. Columns (3) and (6) report the differences in the averages for treated and control firms (hiring with or without a subsidy, respectively) in the two regimes. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Table A.1 – continued from previous page

	Staff selection			Rules selection		
	Treated (1)	Control (2)	Difference (3)	Treated (4)	Control (5)	Difference (6)
$t-5$	7.11	7.14	−0.03	7.19	7.12	0.06
$t-4$	7.08	7.14	−0.06	7.15	7.13	0.01
$t-3$	7.11	7.12	−0.01	7.14	7.16	−0.02
$t-2$	7.12	7.12	0.00	7.18	7.18	0.00
$t-1$	7.14	7.12	0.02*	7.28	7.23	0.05*
Value added per worker						
$t-5$	389.97	408.84	−18.87	445.58	427.75	17.83
$t-4$	378.48	402.19	−23.70	420.14	428.43	−8.29
$t-3$	375.53	401.39	−25.86	416.45	427.23	−10.78
$t-2$	371.87	384.97	−13.10	420.86	423.11	−2.25
$t-1$	385.25	409.74	−24.48	426.90	442.15	−15.25
Tot. investments						
$t-5$	206.43	257.03	−50.60	211.38	208.33	3.05
$t-4$	204.51	206.39	−1.88	137.22	126.79	10.43
$t-3$	208.25	200.21	8.05	118.22	147.59	−29.37
$t-2$	191.41	172.72	18.69*	145.67	113.83	31.84*
$t-1$	228.83	222.96	5.87	163.58	178.20	−14.62
Tot. investments per worker						
$t-5$	44.21	60.28	−16.07	49.07	48.58	0.49
$t-4$	47.21	39.56	7.65	38.22	17.83	20.39
$t-3$	54.85	49.02	5.83	32.61	33.94	−1.33
$t-2$	46.55	42.30	4.25	34.22	33.00	1.22
$t-1$	52.92	52.74	0.18*	37.39	46.82	−9.43*
Firm survival						
$t-5$	0.64	0.65	−0.01	0.65	0.65	−0.00
$t-4$	0.71	0.72	−0.01	0.72	0.72	−0.00
$t-3$	0.79	0.79	−0.00	0.80	0.79	0.01
$t-2$	0.88	0.87	0.01**	0.90	0.89	0.01

Pre–treatment average outcomes matched in $t-1$ (Panel A) or not matched (Panel B), where t is the time when the firm hires. All firm–level outcomes computed using the matched samples. Monetary values are inflation-adjusted (base year: 2000). Separations are normalized by firm size. Wage sum and total profits measured in 1000 SEK, total firm value added in log 1000 SEK. Columns (3) and (6) report the differences in the averages for treated and control firms (hiring with or without a subsidy, respectively) in the two regimes. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Table A.2 – Hired workers' characteristics before matching

	Treated firms		Control firms	
	Subsidized hires	All hires	Unsubsidized hires	All hires
	(1)	(2)	(3)	(4)
Panel A: Staff selection				
Age: 24 or less	0.26	0.31	0.11	0.27
Age: 25–34	0.32	0.29	0.33	0.30
Age: 35–44	0.21	0.20	0.26	0.22
Age: 45–54	0.14	0.13	0.18	0.14
Age: 55–64	0.07	0.06	0.12	0.08
Age: 65 or more	0.00	0.00	0.00	0.01
Immigration to Sweden	0.21	0.17	0.22	0.19
Married	0.29	0.27	0.35	0.29
Male	0.69	0.67	0.59	0.59
Education: Compulsory	0.20	0.24	0.20	0.24
Education: Secondary	0.66	0.59	0.59	0.55
Education: Upper	0.15	0.17	0.20	0.22
Firm hirings	1.06	4.80	1.05	5.56
Panel B: Rules selection				
Age: 24 or less	0.17	0.30	0.11	0.29
Age: 25–34	0.25	0.24	0.31	0.27
Age: 35–44	0.23	0.20	0.26	0.21
Age: 45–54	0.18	0.14	0.19	0.14
Age: 55–64	0.18	0.10	0.13	0.08
Age: 65 or more	0.00	0.01	0.01	0.01
Immigration to Sweden	0.32	0.25	0.30	0.26
Married	0.36	0.29	0.35	0.29
Male	0.66	0.64	0.58	0.59
Education: Compulsory	0.21	0.25	0.20	0.24
Education: Secondary	0.59	0.54	0.55	0.52
Education: Upper	0.21	0.21	0.25	0.25
Firm hirings	1.05	4.97	1.04	6.15

Notes: Characteristics of workers hired by the treated and control firms before matching. Columns (1) and (3) report the characteristics of the long-term unemployed workers hired in the first quarter with or without a subsidy, respectively. Columns (2) and (4) show the characteristics of all workers hired the same year in which the long-term unemployed were hired with or without a subsidy.