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Employment policies, hiring practices and firm performance[☆]

Sylvie Blasco [a,b,c,*], Barbara Pertold-Gebicka [d,e]

[a] Gains, University of Le Mans, Avenue O. Messiaen, 72085 Le Mans Cedex 9, France
[b] Crest, 15 Boulevard Gabriel Péri, 92245 Malakoff, France
[c] IZA, Schaumburg-Lippe-Strasse 5-9, 53113 Bonn, Germany
[d] Department of Economics and Business, Aarhus University, Fuglsangs Alle 4, 8210 Aarhus V, Denmark
[e] Institute of Economic Studies, Faculty of Social Sciences, Charles University in Prague, Opletalova 26, 101 00 Prague 1, Czech Republic

HIGHLIGHTS

- We extent the evaluation of active labor market programs to firms' outcomes.
- We apply a panel Difference-in-Difference strategy on a Danish social experiment.
- We find that counseling and monitoring affect hiring practices of small firms.
- But it has no or marginal effects on firms' performance in the short run
- We conclude that these ALMPs increase the matching efficiency but not its quality.

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ABSTRACT

In this paper we investigate how active labor market policy programs affect firms' hiring strategies and, eventually, firms' performance. We focus on counseling and monitoring which may reduce search costs for employers, but which may also have ambiguous effect on the employer–employee matching quality and thus on firms' performance. Using a large scale experiment which was conducted in Denmark in 2005–2006 and induced a greater provision of activation, we find that small firms hiring in the districts where the social experiment was conducted changed their hiring practices in favor of unemployed workers and experienced greater turnover than other firms. Treated firms also experienced no change or a marginal reduction in value added and total factor productivity during the first years after the experiment. These results are consistent with the idea that monitoring creates compulsion effects which counteract the possible improvement in the matching process expected from job search assistance.

1. Introduction

In most European countries active labor market policy programs (ALMPs) were largely developed over the past decade. The rich literature evaluating these programs almost exclusively focuses on outcomes related to individuals participating in the interventions (see the meta-analysis by Kluve, 2010 or Card et al., 2010). Recently, also general equilibrium effects of ALMPs have been evaluated (Crépon et al., 2012; Ferracci et al., 2010; Gautier et al., 2012) with a focus on nonparticipants indirectly affected by these programs. Although equally important, the evidence on how ALMPs may affect firms is very limited (Gautier et al., 2012; Lechner et al., 2012). While there is a growing empirical literature showing that diversity in the workforce demographic composition has an effect on firms' outcomes,[1] there is a lack of empirical evidence on

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* Corresponding author at: Faculté de Droit et des Sciences Economiques, Université du Maine, Avenue O. Messiaen, 72085 Le Mans Cedex 9, France. Tel.: +33 2 43 83 27 97.
E-mail addresses: sylvie.blasco@univ-lemans.fr (S. Blasco), bpertold@econ.au.dk (B. Pertold-Gebicka).

[1] See for instance, Parrotta and Pozzoli (2012) for the influence of cultural, demographic and skill diversity of workforce on firm productivity, Weber and Zulehner (2009), Weber and Zulehner (2010). Smith et al. (2006), Matsa and Miller (2010) and Ahern and Dittmar (2012) for diversity effects along gender, or Grund and Westergaard-Nielsen (2008) and Ilmakunnas and Ilmakunnas (2010) for the effect of the workforce age structure.

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the effect of workforce diversity in terms of ALMP participation history on firms' performance, except for Lechner et al. (2012). In this paper we offer to fill the gap in the literature by providing an empirical analysis of how two important components of ALMPs, namely job search assistance dedicated to the unemployed workers and their monitoring, affect firms' hiring decisions and performance. By investigating firm-level outcomes, we provide the missing input for complete evaluation of costs and benefits of ALMPs.

Labor market institutions have strong potential to affect firms' economic performance by influencing their hiring and firing strategies. For example, employment protection legislation has been both theoretically and empirically documented to increase frictions in the labor market and thus impede hiring (see for instance Autor et al., 2007; Pries and Rogerson, 2005 or Kugler and Saint-Paul, 2004). On the other hand, the effect of active labor market policies, such as counseling and monitoring, is not clear. Acting as an intermediary on the labor market, the public employment service has the potential to affect the employer–employee matching process through several channels.

First, if effective, job search assistance helps the unemployed at finding job offers, and monitoring helps at maintaining certain level of search activity. As a result, unemployed workers send more applications. Labor market tightness is reduced, so firms open more vacancies (Pissarides, 2000) and are more likely to meet and hire unemployed workers. Second, job search assistance may help the beneficiaries at directing their applications to offers which suit their qualifications best. If so, ALMPs generate better employer–employee matches and have a positive effect on firms' performance. On the other hand, compulsory participation in activation programs creates a leisure tax on the participants (Rosholm and Svarer, 2008) so that monitoring and potential sanctions generate a threat effect. This compulsion effect lowers the value of unemployment (Van Ours, 2007) and pushes people back to work without concerns about the match quality. This may counteract the positive effects of counseling on the employer–employee match quality and damage firms' productivity. Thus, the effect of ALMPs on firms' performance becomes an empirical question.

We offer to empirically evaluate the effect of ALMPs on firms' performance in the context of Denmark. Denmark is known for its flexicurity system introduced in 1994, which combines flexible hiring and firing regulations, generous unemployment insurance and social security net, and extensive active labor market policies (Andersen and Svarer, 2007). In this setup, intensification of activation policies has been consistently shown to have a strong positive effect on exit rates from unemployment for participants (see for example Rosholm, 2008). But the existing empirical evidence of both the compulsion effect on participants (Graversen and van Ours, 2008) and the displacement effect on nonparticipants (Gautier et al., 2012), suggests that counseling helps the beneficiaries at the expense of other participants of the labor market, be it the employing firms or the nonparticipating unemployed. This sheds doubt on the hypotheses that firms hire more unemployed workers and have better market performance when the provision of ALMPs increases.

We use a Danish linked employer–employee dataset, which provides a unique panel of firms and workers, combined with accounting data to test the above hypotheses. Because we expect firms' workforce composition to be endogenous to their performance, we use a large-scale social experiment conducted in Denmark in Winter 2005/2006 that provided exogenous increase in the provision of ALMPs for firms hiring in the treated regions. We implement a panel Difference-in-Difference strategy to evaluate whether this increased provision of ALMPs changes hiring strategies of firms in favor of the unemployed, which, in turn, would lead to changes in firms' workforce compositions and affect various measures of firms' performance. We run the analysis separately for small, medium-size and large firms, as they may be affected differently by the experiment.

We find that within a year following the experiment small firms which labor markets were significantly affected by the experiment hire slightly more workers from the pool of unemployed and experience greater workforce turnover. Regarding firms' performance, we find that the experiment had no or only marginal detrimental effects on firms' productivity in the short run. We find no effect for larger firms, which hiring strategies seem not to be affected by the increased provision of ALMPs. These results run along with those found by Lechner et al. (2012) for the German case, and are consistent with the hypothesis that the compulsion effect acts against the counseling effect, so that intensive counseling and monitoring improve the matching process, but not firms' performance.

The rest of the paper is organized as follows. Section 2 reviews the literature and proposes a stylized matching model to underline the theoretical mechanisms. Section 3 presents the empirical question and strategy and Section 4 discusses the results. Lastly, Section 5 concludes.

2. Related literature and theoretical mechanisms

We start by sketching a simple job matching model to show how counseling and monitoring[2] can affect firms' performance through its effect on optimal hiring practices. If counseling and monitoring affect job search strategies of unemployed workers and the employer–employee matching process, these schemes have potential to affect firms' hiring decisions, the composition of their workforce and, as a consequence, their economic performance. This mechanism relates to the one put forward by Autor et al. (2007) who show that stricter employment protection damages firms' performance, because it induces firms to retain unproductive workers. As pointed out by Oyer and Schaefer (2010), a theoretical framework on the firm-level recruitment and hiring strategies may help explaining the persistent firm-level conditional differences observed in profitability. While constructing a complete model of the labor market is beyond the scope of this paper, we provide a sketch of a simple job market model to illustrate the mechanisms analyzed in the empirical analysis.

We consider a representative firm which can freely open vacancies to produce an output. It can hire an unemployed job seeker to fill the position. The probability of being matched to such a job seeker depends on the market tightness $\theta = \frac{U}{V}$, that is on the ratio of the number of active unemployed job seekers U to the number of vacancies V. To capture productivity differences we introduce workers' heterogeneity and assume that the unemployed job seekers population is divided into two types of workers: “high productivity” workers (with productivity ν_H) and “low productivity” workers (with productivity ν_L), where $\nu_H > \nu_L$. The firm does not observe the applicant's specific productivity prior to forming a match, but it knows the distribution of productivities among the applicants. The true quality of the worker is revealed once the match is formed. We consider two policy contexts, one with intensive ALMPs, denoted as $D = 1$, and one with regular ALMPs, denoted as $D = 0$. The policy context affects the matching function $m(\theta,D)$, the hiring costs $C(D)$ and the productivity composition of the active unemployed job seekers, that is the share of high productivity unemployed workers $p(D)$.

The expected value J_e of a job filled with a worker is given by:

$$J_e(D) = p(D)J(\nu_H) + (1-p(D))J(\nu_L),$$

with $J(\nu_H)$ and $J(\nu_L)$ being the values of a firm that hires a worker of productivity ν_H and ν_L respectively. A position filled with a worker of productivity ν_i, with $i = \{L,H\}$, produces ν_i output per unit of time. The worker is paid a wage w. The match is dissolved at a Poisson

[2] We abstract from other types of activation programs, such as training, workfare and subsidized employment, to be consistent with the empirical analysis.

rate δ which is assumed exogenous for simplicity. Hence, the value of a firm matched with a worker of productivity ν_i, $i = \{H,L\}$ is:

$$rJ(\nu_i) = \nu_i - w + \delta(V - J(\nu_i)).$$

The value of a vacant firm is:

$$rV = -C(D) + m(\theta, D)(J_e(D) - V). \tag{1}$$

The firm creates a vacancy as long as the cost of recruiting a worker is greater or equal to the expected gains of recruiting such a worker in the policy context D. The free-entry assumption brings this condition to equality. So we have:

$$\frac{C(D)}{m(\theta, D)} = \frac{(p(D)\nu_H + (1-p(D))\nu_L) - w}{r + \delta}. \tag{2}$$

In presence of frictions and ex ante unobserved heterogeneity of workers, unemployed workers using the formal recruitment channel may be disadvantaged compared to job seekers who are employed or who are using referrals because being unemployed sends a negative signal on ability and productivity (Rees, 1966; Granovetter, 1995; DeVaro, 2008). Empirical evidence indeed shows that a firm tends to prefer contacting and hiring an employed candidate than an otherwise identical unemployed candidate (Eriksson and Lagerström, 2006). The theoretical foundation of this argument is given by Kugler and Saint-Paul (2004) who show that lower productivity workers, who produce a match of lower quality, are more likely to be separated from the firm than more productive worker. As a result, the pool of unemployed candidates is expected to be composed of workers with lower ability. Counseling and monitoring can help reducing this disadvantage imposed on unemployed workers.

First, ALMPs can have a positive quantitative effect reflected in the matching function ($m(\theta,1) > m(\theta,0)$). If counseling reveals job offers and monitoring maintains job search activity at higher level, workers exposed to intensified ALMPs send more applications. As shown in the matching literature (Pissarides, 2000), an increase in the number of job applicants reduces the market tightness. It is easier for a firm to meet applicants, so that the average time needed to fill a vacancy and the expected recruitment costs are reduced. In response, firms open more vacancies and are more likely to hire unemployed workers. Gautier et al. (2012) indeed show that increased counseling and job search assistance slightly increase the stock of vacancies. Moreover, empirical evaluations on individual outcomes show that counseling and monitoring increase the exit rate from unemployment of participants.

Second, ALMPs can have a positive qualitative effect reflected in the distribution of productivity among the applicants faced by a single firm ($p(1) > p(0)$). If counseling helps workers at directing their job applications better, it has a potential to generate better matches. As the result, firms hire more unemployed workers and, at the same time, might be more productive. The growing literature evaluating the effects of ALMPs on participants' post-unemployment outcomes gives mixed results but other studies show that counseling increases the match quality, in the sense that it reduces the separation rate from the new job (van Ours, 2002) or increases reemployment wages (Cebi and Woodbury, 2011). On the other hand, if counseling does not affect where job seekers send their applications, it can adversely affect firms' economic performance (DeVaro, 2005). Moreover, in this case firms are crowded with noisy and irrelevant applications and experience an increase in the reviewing cost ($C(1) > C(0)$), so they do not necessarily open more vacancies or hire more unemployed workers, creating also a negative quantitative effect.

Third, the compulsion effect lowers the value of unemployment and pushes people to send out job applications without the concern for potential match quality. As the result the matching function is improved at the cost of the quality of the unemployed job seekers. This reinforces the positive quantitative effect but runs against the positive qualitative effect ($m(\theta,1) > m(\theta,0)$ and $p(1) < p(0)$). In this case firms hire more unemployed workers but their value is reduced. A number of studies indeed show that monitoring and sanctions increase exit rates from unemployment (Black et al., 2003; Cebi and Woodbury, 2011), but would worsen various post-unemployment outcomes (Van den Berg and Vikström, 2009; Arni et al., in press).

All in all, the final effect of counseling and monitoring is not clear. While we expect that this type of active policy induces firms to hire more unemployed job seekers, it is not clear how it affects firms' economic performance. The net effect of ALMPs on firms' performance depends on whether the quantitative effect is combined with a qualitative effect and on whether the latter is dominated by the compulsion effect. We turn to empirical analysis to answer this question.

3. Empirical analysis

3.1. The evaluation problem

We aim at identifying the effect of increased provision of ALMPs on affected firms' outcomes. Let $Y_{it}(d)$ be the outcome variable of firm i measured in year t in the policy context $D \in 0, 1$, where $D = 1$ denotes an environment with intensified ALMPs. The average treatment effect on firms operating in a labor market with intensive ALMPs is

$$\delta = E(Y_{it}(1)|D = 1) - E(Y_{it}(0)|D = 1).$$

The first term in the above expression is the observed outcome of a firm under intensive ALMPs (a treated firm), while the second term is the unobserved counterfactual, i.e. the outcome of this firm under usual provision of ALMPs. We proxy the latter by the expected outcome of a control firm which never faced intensified ALMPs, $E(Y_{it}(0)|D = 0)$. To correctly identify the treatment effect, control and treated firms should not differ in any characteristics which might influence the outcome variable.

Our main concern is that the hiring decisions of firms are endogenous to their performance. One could expect that firms most likely to hire in labor markets with intensified counseling and monitoring rely more on the unemployment pool to recruit new workers, which in turn might be correlated with their performance, independent on treatment. To give evidence of this selection, we regress the share of new hires coming from the pool of unemployed on various firms' characteristics. The estimated correlations are reported in Table 1. Note that the correlation between firm performance and the share of new hires coming from the pool of unemployed is strong. One log point increase in the value added per employee is connected with 0.5 percentage point increase in hiring from the pool of unemployed, which corresponds to a 10% increase. Also, higher turnover rates are associated with more hiring from the pool of unemployed. Finally, firms hiring more from the pool of unemployed have, on average, slightly older, less educated workforce and fewer females. Thus, we need to use an estimation strategy that will allow us to reduce, if not eliminate, this selection to correctly evaluate the effect of ALMPs on firms' performance.

3.2. The identification strategy

To deal with the endogeneity problem, we take advantage of a large social experiment which was conducted in Denmark in Winter 2005/2006 in the Southern Jutland County.[3] The experiment created

[3] The experiment was also implemented in another county (the Storstrøm County). However, a recent change in the administrative division of Denmark, resulting in a redefinition of location identifiers, does not allow us to clearly identify it in our data. Thus, we drop individuals and firms operating in the new districts that include the former Storstrøm County.

Table 1
Correlations between hiring practice and firm characteristics and performance.

Dependent variable: share of previously unemployed in new hires	Small firms	Medium firms	Large firms
Firm outcomes			
Log of value added per employee	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)
Turnover	0.018***	0.013***	0.008***
	(0.001)	(0.001)	(0.002)
Workforce composition (previous year)			
Median age	0.000***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
% Females	−0.004**	−0.001	0.002
	(0.002)	(0.002)	(0.003)
% University	−0.003	−0.027***	−0.010
	(0.003)	(0.004)	(0.007)
% Vocational	−0.016	−0.046**	−0.053
	(0.013)	(0.022)	(0.057)
% Primary	0.000	−0.010**	0.023***
	(0.003)	(0.005)	(0.009)
% Managers	−0.001	0.004	0.009
	(0.004)	(0.004)	(0.005)
% Blue collar workers	−0.007***	−0.002	0.014***
	(0.002)	(0.002)	(0.003)
Industrial composition			
Industrial	0.014	0.033***	0.018*
	(0.014)	(0.011)	(0.011)
Building and plant	0.011	0.024**	0.008
	(0.013)	(0.011)	(0.011)
Trade, hotel, restaurants	0.008	0.017	0.001
	(0.013)	(0.011)	(0.011)
Transport, post, telecom	−0.001	0.016	0.003
	(0.014)	(0.011)	(0.011)
Finance, business services	0.000	0.018*	−0.005
	(0.013)	(0.011)	(0.011)
Intercept	−0.001	−0.027**	−0.037***
	(0.014)	(0.012)	(0.014)

Note: Standard errors in parentheses. Variable excluded due to collinearity: Share of white collars, share of high school graduates. Other controls included: year and industry dummies. All workforce characteristics are lagged one year. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

an exogenous increase in the provision of ALMPs for a significant share of eligible unemployed workers living in the treated region.

Half of the 2500 workers entering unemployment between November 2005 and February 2006 in Southern Jutland were assigned to intensified ALMPs.[4] Treated individuals had to participate in a two-week Job Search Assistance program from weeks 5/6 to weeks 7/8 of unemployment. From week 7 to week 18, they met frequently with a caseworker to ensure that they were actively searching and to assist them in this job search. After 18 weeks of unemployment, they finally had to participate in an ex-ante unspecified program for a minimum duration of 13 weeks.[5] 30% of treated individuals left unemployment during the first month and 68% during the first 18 weeks, so few treated individuals entered in the 13-week scheme. Thus, the experiment effectively consisted in intensified counseling and monitoring. As shown in Fig. 3 in the Appendix, treated workers faced much higher intensity of activation programs during the second month of unemployment and met a caseworker about 3 times as often as the non-treated during the following months. At the end of this program, treated individuals went back to the regular activation with a meeting with a caseworker once every 13 weeks and with participation in an unspecified program after one year of unemployment.

The existing evaluations of this experiment suggest that the experiment created a shock to the matching process and that firms in the treated regions adjusted their hiring practices and hired more from the pool of unemployed in response: Rosholm (2008) shows that the treatment had remarkable positive effects on the exit rate from unemployment for the treated individuals. Gautier et al. (2012) show that there were negative displacement effects for the untreated unemployed, which, however are weaker than the positive effect on the treated. They also find that the total number of vacancies slightly increased during the experiment. Nevertheless, it is not clear how the experiment influenced the affected firms' economic performance. It has been shown that the increased job search activity of the unemployed during the social experiment can be significantly accounted for by the threat effect (Graversen and van Ours, 2008). This could adversely affect the average quality of job applicants faced by the affected firms and, as a consequence, have a negative effect on their subsequent economic performance.

3.3. Definition of the treatment and control groups

We define the treated and control firms based on the county of residence of their workforce before the experiment has started. This definition of the assignment groups allows us to account for the facts that firms usually hire workers in more than one county and that the majority of their workforce might not come from the county where the firm is registered.[6] As firms were not informed about the experiment, we do not have to worry that firms endogenously choose to hire in an environment with stronger ALMPs. Moreover, cross-region mobility of workers, which could dilute the effects of the experiment and undermine our empirical strategy, is not an important phenomenon in Denmark. On average, during the period of interest, about 11% of the working-age population living in Denmark moved between regions each year. More importantly, cross-region migration from and to the experimental county only concerns 4% of the working-age population each year, on average.

A firm is considered as treated if it was employing on average more than 33% of its workforce from the Southern Jutland County between November 2004 and November 2005.[7] These firms were facing unemployed job applicants who had a high probability of being exposed to intensified ALMPs. This results in about 17,800 treated firms observed yearly.

The basic control firms are those which, on average, between November 2004 and November 2005, were recruiting most of their workforce from a geographical area that was not affected by the experiment. There are two concerns which might influence our estimates. First, firms interested in hiring from the pool of unemployed could be more affected by the experimental intensification in ALMPs. Second, the two treated counties volunteered for hosting the experiment. As is presented in Fig. 1, Southern Jutland had visibly higher unemployment rate prior to the experiment than other Danish counties, even though the coverage of activation policies in this region was relatively large. For these reasons, we are careful in conditioning on a full set of firm characteristics when estimating the effects of the experiment. We also restrict the set of control firms to firms that used to have recruitment areas economically similar to Southern Jutland (as in Gautier et al., 2012). As Fig. 1 shows, East Jutland and Funen experienced a similar evolution of local unemployment rate to South Jutland prior to the experiment. We thus limit our attention to these regions, and we apply fixed effects technique to deal with

[4] Randomization was based on birth dates in the month: those born on the 1st to the 15th were assigned to the treatment group, while those born on the 16th to the 31st were assigned to the control group.

[5] Treated could be allocated to 'private sector temporary employment subsidy jobs', temporary employment within the public sector, classroom training programs and vocational training programs in firms.

[6] Lechner et al. (2012) use a similar strategy to identify firms facing policies implemented by regional job centers.

[7] In Section 4.3, we test the sensitivity of our results to this criterion.

firms' time-constant propensity to hire in the pool of unemployed. Given these restrictions, our final control sample consists of firms hiring less than 33% of their workforce in South Jutland and at least33% of their workforce in East Jutland or Funen. This results in about 21,726 control firms a year.

Table 2 shows the characteristics of the treated and control firms prior to the experiment. The group of comparison firms is in general very similar to the group of treated firms. The only important difference between these two groups can be observed in the share of previously unemployed in new hires and in the qualification structure of the workforce: it appears that the treated firms hired less from unemployment and had less qualified workforce before the experiment.

3.4. The econometric model

To measure how the experimental increase in counseling and monitoring influenced firms' outcomes, we differentiate the change in these outcomes before and after the experiment between firms hiring mainly in the experimental region and firms hiring mainly in control regions. Because we have a panel of firms, we can implement a Difference-in-Difference with firms' fixed effect. The fixed-effect Difference-in-Difference approach is preferred to the cross-section Difference in Difference strategy because it allows for controlling for systematic differences between firms.

In the panel Difference-in-Difference approach, we estimate the following regression with panel methods:

$$Y_{tij} = \delta R_j \times T_t + \lambda T_t + \beta X_{tij} + \alpha_i + u_{tij},$$

where R_j is the regional dummy that equals 1 if the firm used to hire more that 33% of its workforce in the experimental region, and 0 otherwise, T_t is the dummy indicating whether the year of observation is the post-experiment year (2005–2007), X_{tij} is a set of firm's i observed characteristics, and α_i is firm's i fixed effect. The fixed effects and year dummies control, respectively, for systematic heterogeneity between firms and for time trends that can affect, independent on the experiment, the outcome variable. The parameter δ associated with the interaction term identifies the treatment effect under the identifying assumption of common trend: in the absence of the experiment, firms hiring more in the experimental region have the same evolution in the outcome Y as firms relying more on the non-experimental regions.

We graph the evolution of two out of four firm outcomes (share of previously unemployed in new hires and value added per worker) before and after the experiment (Fig. 2).[8] The pre-treatment trends for the treated and control firms are alike, supporting the common trend assumption. Starting from the experiment period, there is some discrepancy in the trends between the two groups.

3.5. The data

In our analysis we use information coming from two Danish datasets which are merged using a unique firm identifier. As the result we have a panel of firms containing economic as well as labor force information.

The first dataset is a rich register-based dataset, the "Integrated Database for Labor Market Research" (IDA) provided by Statistics Denmark. This linked employer–employee data contains firm, workplace and individual identifiers, so it forms a panel of firms linked with a panel of employees. The firm panel provides rich information about the firm, like the industry, size or ownership. The individual panel contains socio-demographic characteristics, like gender, age, nationality, education, or district of residence for all individuals living in Denmark. For those aged 15–64, we additionally observe the labor market situation as well as the characteristics of the job and the identifier of the employing firm and workplace in case of employment as of November 30 each year. We aggregate individual-level data at the firm level to construct a panel of firms with such yearly information as firm employment level, demographic structure of its workforce, the geographic area where firm's workforce comes from, worker turnover characteristics, and the origin of newly hired workers (from another firm, from out of labor force, from unemployment).

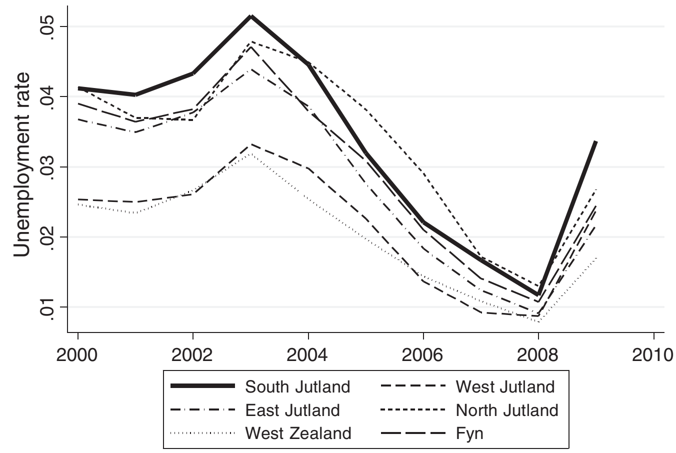

Fig. 1. Evolution of the unemployment rate in the experimental and control counties.

The second dataset we use contains firms' yearly business accounts, that is firms' value added, capital stock, investments, and intermediate inputs. This dataset is collected in the form of a survey by Statistics Denmark. All firms with more than 50 employees and all firms exceeding a specified profit threshold are obliged to fill in the survey. The remaining firms are sampled using stratified sampling strategy.

The choice of the sample used in the empirical analysis is driven by the timing of the experiment and data availability. The accounting dataset is available till the year 2007, which allows us to analyze two years after the experiment (2006 and 2007). To assure comparability, we chose to analyze also two years before the experiment (2004 and 2005). Due to the fact that some variables are defined with one year lag, we limit our attention to firms which exist for at least two consecutive years and we include year 2003 in the initial sample.

3.6. Measurement of firms' outcome variables

We are interested in the impact of the increased provision of ALMPs on workforce-related outcomes and market performance outcomes.

In the workforce-related outcomes we consider hiring composition and employee turnover in a firm. For each firm i, we calculate the turnover TO_{it} for a given year t as the share of employees hired between November $t-1$ and November t in the total stock of employees at the end of November t. This measure is informative about both the growth of the firm and pure turnover. Because we are interested in knowing whether firms hire more unemployed workers due to an increase in the provision of ALMPs, for each year t, we also compute the share of new employees who were unemployed in the previous year. This measure, SU_{it}, is only available for firms which hire new employees between November $t-1$ and November t.

We use two measures of market performance outcomes. First, we focus on firm-level value added to capture the effect of workforce composition on the value created by a firm. Value added is calculated as the difference between firm's total output and the value of inputs, as measured at the end of the calendar year. We measure value added per full-time employee and express it in log values. It is referred to as $VAPC_{it}$. Second, we analyze firm-level total factor productivity (TFP) to test if firms facing with increased provision of ALMPs optimally

[8] We plot time evolution for these outcomes which we observe for at least 3 years before the experiment.

Table 2
Comparison of treated and control firms' characteristics before the experiment.

	Small firms		Medium firms		Large firms	
	Treated	Control	Treated	Control	Treated	Control
Outcomes						
TFP (log)	4.33	4.33	4.27	4.26	4.30	4.32
	(0.66)	(0.66)	(0.53)	(0.54)	(0.48)	(0.49)
VAPC (log)	5.39	5.38	5.47***	5.42	5.63	5.56
	(0.87)	(0.87)	(0.67)	(0.71)	(0.70)	(0.80)
Turnover	0.01***	0.02	0.08	0.09	0.08	0.07
	(0.38)	(0.37)	(0.25)	(0.26)	(0.28)	(0.27)
SU (%)	0.04***	0.05	0.03***	0.04	0.04*	0.04
	(0.14)	(0.15)	(0.06)	(0.07)	(0.03)	(0.05)
Degree of unemployment among new employees coming from unemployment						
<25%	0.39	0.40	0.45	0.43	0.43	0.44
	(0.48)	(0.48)	(0.45)	(0.44)	(0.32)	(0.33)
25–50%	0.29	0.29	0.32*	0.29	0.37	0.33
	(0.44)	(0.44)	(0.41)	(0.40)	(0.32)	(0.30)
50–75%	0.22	0.21	0.16**	0.19	0.15	0.17
	(0.40)	(0.40)	(0.33)	(0.34)	(0.22)	(0.25)
≥75%	0.09	0.10	0.07**	0.09	0.05	0.06
	(0.28)	(0.30)	(0.22)	(0.25)	(0.14)	(0.15)
Workforce composition in the previous year						
Firm size	7.24	6.31	36.47	38.65	295.54	329.75
	(142.18)	(7.68)	(19.65)	(156.38)	(480.63)	(1105.62)
% Managers	0.03***	0.04	0.05***	0.06	0.06***	0.08
	(0.12)	(0.14)	(0.09)	(0.11)	(0.09)	(0.14)
% Skilled workers	0.04***	0.05	0.08**	0.09	0.11	0.10
	(0.13)	(0.14)	(0.13)	(0.14)	(0.11)	(0.11)
% Blue collar workers	0.23	0.23	0.36**	0.35	0.37	0.36
	(0.26)	(0.25)	(0.23)	(0.22)	(0.20)	(0.21)
% Women	0.33	0.32	0.31***	0.34	0.34	0.34
	(0.34)	(0.33)	(0.26)	(0.26)	(0.23)	(0.22)
% Foreigners	0.04***	0.06	0.05	0.05	0.06	0.07
	(0.15)	(0.17)	(0.08)	(0.09)	(0.08)	(0.10)
% University	0.60***	0.62	0.57***	0.59	0.57	0.58
	(0.31)	(0.31)	(0.20)	(0.21)	(0.17)	(0.17)
% Vocational	0.00	0.01	0.00**	0.01	0.01	0.01
	(0.04)	(0.04)	(0.01)	(0.02)	(0.01)	(0.01)
% Secondary	0.29**	0.29	0.31***	0.31	0.31***	0.31
	(0.28)	(0.27)	(0.17)	(0.18)	(0.13)	(0.14)
% Primary	0.06***	0.04	0.07	0.05	0.06	0.04
	(0.13)	(0.10)	(0.07)	(0.06)	(0.05)	(0.04)
Industrial composition						
Agriculture, fishing, extrac.	0.00*	0.00	0.00	0.00	0.01	0.00
	(0.04)	(0.03)	(0.04)	(0.03)	(0.07)	(0.05)
Industrial	0.11	0.11	0.27***	0.24	0.44	0.42
	(0.31)	(0.31)	(0.44)	(0.43)	(0.50)	(0.49)
Energy, water supply	0.20***	0.17	0.17	0.16	0.06	0.07
	(0.40)	(0.38)	(0.37)	(0.36)	(0.23)	(0.26)
Building and plant	0.42	0.42	0.37	0.38	0.26	0.22
	(0.49)	(0.49)	(0.48)	(0.49)	(0.44)	(0.42)
Trade, hotel, restaurants	0.09***	0.07	0.10***	0.07	0.10	0.08
	(0.28)	(0.26)	(0.30)	(0.25)	(0.30)	(0.27)
Finance, business services	0.18***	0.23	0.09***	0.16	0.14**	0.20
	(0.39)	(0.42)	(0.29)	(0.36)	(0.35)	(0.40)
Observations	15,217	18,349	2207	2920	382	457

Test of significance of differences: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

use their technology and resources. Total factor productivity is measured as the residual from firm-level production function estimation. We follow Wooldridge (2009) when estimating the production function. We use labor and capital as the main inputs, assuming that labor can react to short-time productivity shocks and thus use lagged values of labor inputs as instruments. Additionally, we use intermediate inputs, such as material and energy, as a proxy for investment levels (Levinsohn and Petrin, 2003) to partially control for time-varying productivity shocks. A third-degree polynomial of inputs is used to proxy for the unknown shape of the production function (Petrin et al., 2004). TFP is expressed in log values and this measure is denoted by TFP_{it}.

4. Results

In this section we present and comment our estimates of the effects of intensified ALMPs on firms' performance obtained with the panel Difference-in-Differences strategy. The analyzed sample is divided into sub-samples of small firms (with less than 20 employees), medium-sized firms (employing 20 to 100 workers) and large firms (with more than 100 employees) to capture potential heterogeneity in their responses to ALMPs. The set of control variables, X_{ijt}, used when estimating the treatment effects contains industry dummies and workforce socio-demographic characteristics of the firm i (educational and occupational structures, distribution of age of employees

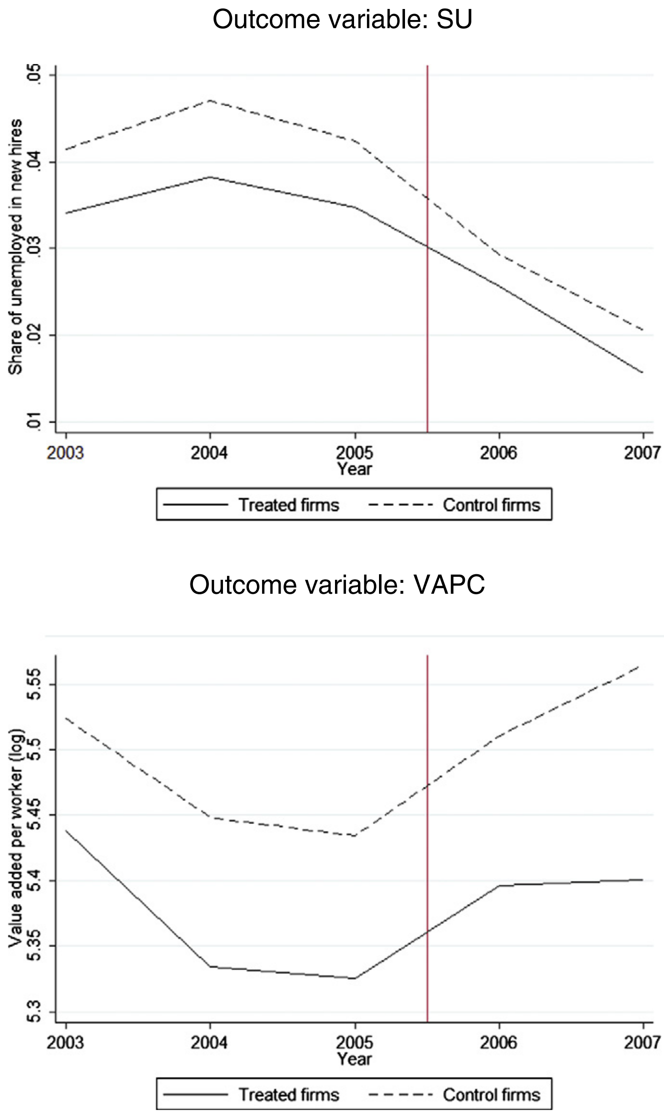

Fig. 2. Evolution of the outcome variables in the experimental and control counties.

and the share of women among employees). The workforce composition is measured in $t - 1$ for both workforce outcomes, while it is measured in the current year t for both economic performance outcomes.

The estimated parameters of main interest are reported in Table 3. The full set of estimated parameters is displayed in Tables 6 and 7 in the Appendix. To verify whether firm self-selection is an issue in the context of the analyzed experiment, we also implemented the cross-section Difference-in-Difference strategy in which we ignore the panel nature of our data. These results are reported in Table 5 in the Appendix. Overall, the comparison between the two sets of results reveals a positive selection: treated firms have some characteristics that make them prefer hiring in the experimental region and at the same time rely more on the pool of unemployed when hiring. Firms which systematically hire the unemployed benefit more from intensive ALMPs than the other firms and thus their hiring performance strongly improves after the experiment. Once we control for this systematic differences in hiring preferences, thanks to the fixed effect approach, the estimated effect of intensified ALMPs on hiring practices is reduced but remains significant. Concerning economic performance, the classic DiD approach shows that increased

Table 3
The impact of ALMPs on firm-level outcomes — panel DiD analysis.

	Small firms	Medium firms	Large firms
Firm-level hiring outcomes			
SU	0.004**	0.003*	0.003
	(0.002)	(0.002)	(0.002)
Workforce turnover	0.015***	−0.001	−0.002
	(0.005)	(0.007)	(0.016)
SU by degree of unemployment (DU)			
SU with DU < 25%	0.003***	0.001	0.002
	(0.001)	(0.001)	(0.002)
SU with DU between 25 and 50%	0.000	−0.001	0.001
	(0.001)	(0.001)	(0.001)
SU with DU between 50 and 75%	0.001	0.002**	0.001
	(0.001)	(0.001)	(0.001)
SU with DU ≥ 75%	0.000	0.001**	0.000
	(0.001)	(0.000)	(0.000)
Firm-level performance outcomes			
TFP	−0.021***	−0.005	−0.012
	(0.006)	(0.010)	(0.022)
Value added per employee	−0.001	0.021**	−0.001
	(0.007)	(0.010)	(0.022)

Note: Parameter estimates for the interaction term between treatment indicator and post-treatment years. Standard errors in parentheses. SU by degree of unemployment is the share of new employees who come from unemployment and spent less than 25%, between 25 and 50%, between 50 and 75% or more than 75% of the year on unemployment. Controls included: year, hiring region, industry dummies and workforce characteristics. Threshold value for the assignation to the treatment group: 33%. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

activation has a significant detrimental effect on the performance of small and medium firms, but this negative effect is largely reduced or is no longer significant once we account for firm's unobserved heterogeneity.

4.1. The impact of increased ALMPs on the hiring strategy

We start by evaluating the effect of intensified counseling and monitoring on hiring strategies, measured by the employee turnover, *TO*, and the share of newly hired who come from unemployed, *SU*, reported in Table 3. First, note that only small and medium-sized firms are significantly affected by the experiment. One possible explanation for this heterogenous effect according to firm size is the following: smaller firms may encounter more difficulties in the hiring process than large firms, for instance because they are less visible to job seekers or because they are less likely to have human resources department. As the result, they are the ones that are most likely to be affected by an improvement in the job matching process generated by counseling.

An increase in ALMPs has a significant positive effect on the share of previously unemployed in new hires among small and medium firms which hire at least one new worker. This is consistent with the existing literature on individual outcomes, which shows that the experiment increased exit rates from unemployment for participants (Rosholm, 2008) and that this positive effect on the treated is not compensated by the negative effect found for the untreated (Gautier et al., 2012). Based on the estimate, we can conclude that, for small firms, intensive counseling and monitoring in the region where they tend to recruit increase the share of new employees coming from unemployment by 0.4 percentage points. Given that, on average, 4% of the new hires in small firms come from unemployment, the increase due to the experiment accounts for 10% of the average share of previously unemployed in the new hires, which is noticeable.

Intensification of ALMPs makes small firms hire relatively more unemployed workers with short unemployment ancestry, while it makes medium size firms recruit relatively more unemployed workers who have spent more than half of the year unemployed. Last, the policy does not affect the composition of new hires in terms of unemployment history for large firms.

Intensified ALMPs have a significant positive effect on workforce turnover for small firms and have no significant effect for bigger firms. This increased turnover among the treated firms may be due to two mechanisms. First, it might be simply driven by increased labor force inflow due to intensified counseling and monitoring reducing matching frictions. Second, it might be driven by the fact that newly hired unemployed are less productive, generate a match of low quality and thus are soon laid off, which would be consistent with the idea that the compulsion effect damages the match quality.

4.2. The impact of increased ALMPs on firms' performance

We now turn to the effect of intensified ALMPs on firms' market performance, measured by the value added per worker, *VAPC*, and the total factor productivity measure, *TFP*, both expressed in log.

Intensified activation has no significant effect on firms' performance for large firms. As large firms do not significantly alter their hiring practices due to the experiment, it was expected that their performance would not be changed as well. The policy has a positive effect on the value added per worker of medium-size firms and a negative effect on TFP for small firms. For smaller firms, a change in the workforce is likely to affect performance since each worker accounts for a significant part of the workforce. However, these effects are negligible. Given that for small firms the mean TFP is about 4, the fact that the experiment reduced the TFP of affected small firms by 0.021 means that increased counseling and monitoring reduced their mean TFP by less than 0.5%. These results are in line with those obtained by Lechner et al. (2012) who find that counseling and job search assistance programs have no effect on firms' performance in Germany.

This mostly non-positive effect of monitoring and counseling on firms may find several explanations. First, the estimated effects are of a short term nature, as the outcome is measured at most one and a half year after the experiment took place.[9] We show that small and medium treated firms tend to hire more unemployed workers. When hiring more unemployed – workers who tend to have difficulties on the labor market – firms' performance in the short run may be reduced due to the adaptation period (e.g. training of the newly hired), but might be improved later. Second, non-positive effect on performance is consistent with the hypothesis that compulsion and quantitative effects act against the qualitative and matching effects: counseling and monitoring push workers of lower quality to employment. Moreover, the compulsion effect may force the unemployed to send job application without concerns for the match quality. As the results firms receive more applications from lower productivity or less appropriate workers, which may have a negative effect on their performance. This could also be the reason why we observe higher employee turnover among some firms.

We are able to test whether the qualitative effect stands behind the above discussed results. To do so, we control for the share of previously unemployed in new hires and for the characteristics of the new employees coming from unemployment to neutralize the effect of the experiment on firms' performance through increased inflows from unemployment. When conditioning on the composition of inflows, the estimated effect of the experiment is driven by the average quality of the newly hired workers. These parameters of main interest are reported in Table 4. The full set of estimated parameters is displayed in the Appendix in Table 8.

[9] The data used in this analysis do not allow us to evaluate later outcomes.

Table 4
The impact of ALMPs on firm-level performance outcomes with control for the share of new employees coming from unemployment — panel DiD analysis.

	Small firms	Medium firms	Large firms
Dependent variable: TFP			
Treatment effect	−0.011	−0.005	−0.012
	(0.007)	(0.010)	(0.022)
Share of new employees coming from unemployment	−0.059***	−0.022	0.114
	(0.015)	(0.045)	(0.153)
Treatment effect	−0.011	−0.005	−0.012
	(0.007)	(0.010)	(0.022)
SU with DU < 25%	−0.020	−0.019	0.082
	(0.024)	(0.062)	(0.162)
SU with DU between 25 and 50%	−0.080***	−0.020	0.089
	(0.028)	(0.076)	(0.289)
SU with DU between 50 and 75%	−0.083***	−0.065	0.314
	(0.029)	(0.108)	(0.381)
SU with DU ≥ 75%	−0.116**	0.043	0.145
	(0.048)	(0.172)	(0.880)
Dependent variable: VAPC			
Treatment effect	0.004	0.020**	−0.001
	(0.008)	(0.010)	(0.022)
Share of new employees coming from unemployment	−0.001	0.034	0.097
	(0.017)	(0.045)	(0.151)
Treatment effect	0.004	0.020**	−0.001
	(0.008)	(0.010)	(0.022)
SU with DU < 25%	0.116***	0.119*	0.240
	(0.027)	(0.066)	(0.174)
SU with DU between 25 and 50%	−0.002	−0.069	0.082
	(0.028)	(0.083)	(0.315)
SU with DU between 50 and 75%	−0.152***	−0.035	0.354
	(0.039)	(0.099)	(0.479)
SU with DU ≥ 75%	−0.178***	0.022	−2.394***
	(0.055)	(0.159)	(0.931)

Note: Parameter estimates for the interaction terms between treatment indicator and post-treatment years. Standard errors in parentheses. DU stands for degree of unemployment within the year. Controls included: year, hiring region, industry dummies and workforce characteristics for the current year. Threshold value for the assignation to the treatment group: 33%. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

When we control for the composition of the newly hired, the negative effect on small firms' productivity disappears. Overall, the type of unemployed workers hired matters for small firms: the greater the share of new employees who come from unemployment and spent more than half the year unemployed, the worse the firm's economic performance. One can conclude that the positive effect of ALMPs on hiring of unemployed workers with a short unemployment ancestry is not large enough to overcome the negative impact of new unemployed workers with low employment attachment. No effect for large firms was found. Thus, we conclude that the estimated negative effect of intensified ALMPs on small firms' performance can be accounted to hiring more unemployed workers. This result is consistent with the mechanism explained in Section 2, according to which the compulsion effect generated by monitoring and sanctions undermines the qualitative effect of job search assistance and counseling. However, the experiment took place during booming years. It is then likely that workers who entered unemployment during the experimental period were of very low productivity. If so, the experimental increase in ALMPs has marginally damaged small firms' performance by pushing these low productivity workers back to work.

The effects of the experiment on value added per worker persist when we control for the previous status of the new employees. This might be because newly hired unemployed are paid lower wages, which compensate for their lower productivity.

4.3. Sensitivity analysis

We perform a sensitivity analysis to test the robustness of our results to the choices made in the definition of the treatment and control groups. First, we consider as treated the firms which hire at least 50% of their workforce in the experimental region. Second, we restrict our comparison group to firms that hire less than 25% of their workforce in the experimental region, excluding from the control group firms that are very close to the threshold value. Both analyses give qualitatively the same results as our baseline specification (Table 9 in the Appendix).

Finally, we perform a placebo test, assuming that the experiment took place in a county where no shock to ALMPs happened. Not passing this test, that is finding a significant parameter associated to the placebo interaction term, would cast doubt on the fact that the estimates discussed in the previous section measure the causal effect of the experiment. Table 10 in the Appendix shows that the test is passed for 9 out of 12 of the outcome variables. The parameter is only significant at 10% for the outcome *SU* of small firms. The tests are less satisfying for the TFP outcome: the placebo treatment has an effect on TFP which is significant at 5% for medium-size firms and at 1% for small firms. This suggests that in fact the increased provision of ALMPs has no effect on treated firms' productivity. Nevertheless, this is still consistent with the hypothesis that compulsion effect acts against the expected positive effect of increased provision of ALMPs.

5. Discussion and conclusion

In this article, we extend the evaluation of active labor market programs to firms' outcomes. The empirical evaluation literature tends to show that ALMPs affect both unemployment durations and reemployment stability of individual participants, indicating that ALMPs help improve the matching process, the match quality and/or the employability of unemployed workers. Although understanding the behavior of the demand side of the market is also crucial for a comprehensive evaluation of the ALMPs, there are only very few studies that investigate how ALMPs affect firms' hiring behavior and, especially, their economic performance.

We present a simple theoretical framework to explain how ALMPs may change the terms of recruitment decision in a context where there is ex ante unobserved heterogeneity of unemployed job seekers. Our theoretical predictions indicate that if the intensity of counseling and monitoring increases, firms hire more workers from the pool of unemployed. The effect on their economic performance is, however, not clear. This is why we turn to the empirical analysis. Using an exogenous variation in the intensity of activation provided by a large-scale social experiment that was implemented in Denmark in 2005/2006, we evaluate the effects of increased job search assistance and monitoring for small, medium, and large firms.

First, we check whether the theoretical prediction of increased hiring from the pool of unemployed under an increased provision of ALMPs can be observed in the data. We show that the large experimental increase in counseling and monitoring leads to an increase in the share of unemployed workers among the newly hired for the affected small and medium-size firms. It also generates higher workforce turnover in small firms. These results are in line with our theoretical predictions as well as with the previous literature, suggesting that ALMPs improve employability of the treated unemployed individuals, but they are not conclusive about the effect of ALMPs on the average match quality. Assuming that job tenure is a measure of match quality, the documented high workforce turnover in small firms could be driven by the compulsion effect lowering the quality of newly formed matches. While we are not able to directly test whether this is the case, our further analysis of the effects of ALMPs on firm economic performance gives further evidence in this direction.

When evaluating the effect of the experimental intensification in ALMPs on firms' economic performance, we find that medium firms experience a marginal increase in the value added per worker due to treatment. Moreover, small firms are marginally negatively affected by increase in counseling and monitoring in their hiring regions, while medium and large firms appear not to be affected by the experiment. These mainly non-positive effects are only partially driven by the fact that the affected firms hire more unemployed job seekers, suggesting that we observe two channels. First, the affected firms hire more unemployed and thus might be forced to spend more resources on training their new workers. Second, the workers hired by the affected firms are, on average, less productive due to the compulsion effect. If so, a policy that emphasizes less on monitoring and sanctions and more on counseling could give more positive effect on firm performance. However we cannot test this hypothesis, as the experiment consists of a package of ALMPs and we cannot disentangle between the two components of the program. Moreover, one cannot conclude since a less strict unemployment insurance system (when the level of sanctions and monitoring is reduced) can have a negative effect on exit rates from unemployment, leading to longer unemployment durations and possible loss in employability. Further evidence is required here.

Altogether, our results imply that the experimental increase in counseling and monitoring of the unemployed induces firms to rely more on the unemployed as the potential hiring pool. As small firms benefited most from the improvement of the matching process, our results tend to advocate in favor of hiring support targeted at small firms. We find that intensification of counseling and monitoring not only increases hiring of unemployed workers, but also increases workforce turnover. Thus, we confirm effectiveness of these policies in pushing more individuals into employment, at least in the short run. Additionally, we show that, on average, firms are not harmed by the analyzed ALMPs, which suggests that the previously estimated benefits of such policies are valid.

The analysis presented in this paper has several limitations. First, the identified effects are responses to one shot experimental increase in the provision of ALMPs in the setup where firms did not know about the intervention. While this allows us to obtain robust estimates of these effects, it is not clear whether our findings can be generalized. Second, due to data availability we estimate only short-term effects. A longer observation period would allow us to test if the non-positive effects observed in the short-term are the signs of the adaptation period of the newly employed and would improve over time once this transition period is over. Finally, the analyzed experiment took place during the economic boom. As the matching literature states, when unemployment is low, firms encounter more difficulties in hiring as it is harder to meet an unemployed worker. As a result, an employment policy, such as counseling and monitoring which improves the matching function will be particularly beneficial for prospective firms. This favorable economic context may explain why the experiment had a noticeable effect on hirings, in particular for small firms which may be less visible to job seekers than larger firms and would therefore benefit from the prospective support provided by caseworkers. Furthermore, the economic context may contribute to the non-positive effect on firms' performance, as the average productivity of unemployed workers during booming years is likely to be lower than during a recession. We would then expect smaller effects on firm-level hiring outcomes and stronger positive effects on firm-level economic performance if the experiment was implemented during a recession instead of a boom. Testing whether our results are specific to the economic context (that is to a pool of unemployed consisting of, on average, very unproductive workers), or are driven by lower employer–employee match quality induced by the compulsion effect, requires to decompose the effect on firm's productivity into a specific firm effect, a specific worker effect and the match effect. The estimation of a model with both firm's and worker's fixed effect is then the natural extension to this research.

Appendix A

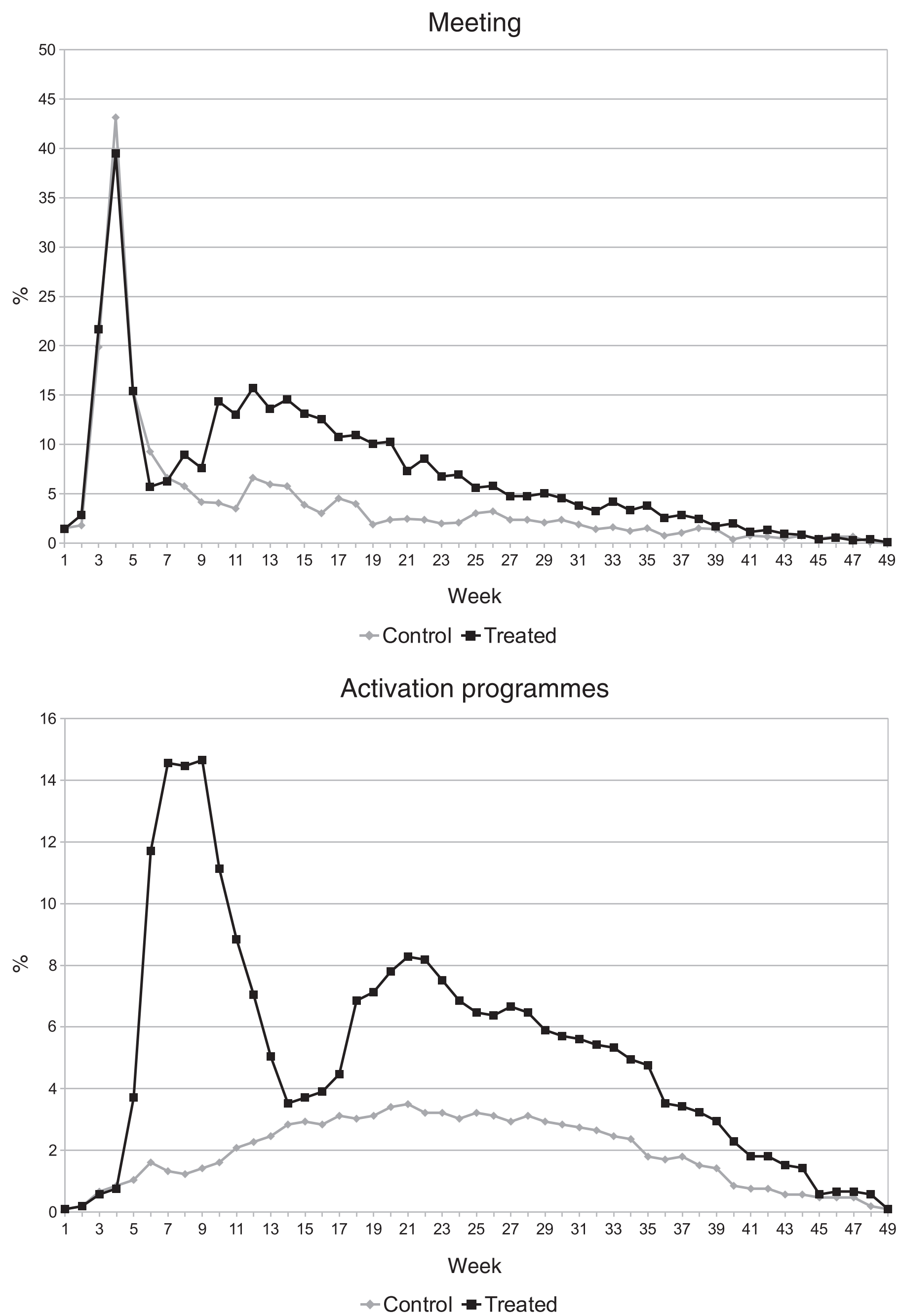

Fig. 3. Evolution of treatment intensity by group in Southern Jutland.

Table 5
The impact of ALMPs on firm-level outcomes — cross-sectional DiD analysis.

	Small firms	Medium firms	Large firms
Firm-level hiring outcomes			
SU	0.008***	0.005***	0.005**
	(0.002)	(0.001)	(0.002)
Workforce turnover	0.104***	0.027***	−0.085***
	(0.004)	(0.008)	(0.021)
Firm-level performance outcomes			
TFP	−0.140***	−0.043***	0.073**
	(0.006)	(0.012)	(0.031)
Value added per employee	−0.160***	−0.037***	0.218***
	(0.007)	(0.013)	(0.045)

**p < 0.05, ***p < 0.01.
Note: Parameter estimates for the interaction term between treatment indicator and post-treatment years. Standard errors in parentheses. Controls included: year, hiring region, industry dummies and workforce characteristics. Threshold value for the assignation to the treatment group: 33%. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.

Table 6
The impact of ALMPs on firm-level hiring outcomes — panel DiD analysis.

	SU			TO		
	Small	Medium	Large	Small	Medium	Large
Treatment effect	0.004**	0.003*	0.003	0.015***	−0.001	−0.002
	(0.002)	(0.002)	(0.002)	(0.005)	(0.007)	(0.016)
Turnover	0.009***	0.006***	−0.000	–	–	–
	(0.001)	(0.002)	(0.002)	–	–	–
Incumbent characteristics (previous year)						
Median age	−0.000*	−0.001***	−0.000	0.012***	0.017***	0.026***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.007)
% Females	−0.005	−0.005	−0.024	−0.131***	−0.035	−0.812*
	(0.006)	(0.008)	(0.017)	(0.020)	(0.092)	(0.468)
% University	−0.015**	0.008	0.066**	0.135***	0.370***	1.949***
	(0.007)	(0.011)	(0.026)	(0.024)	(0.115)	(0.405)
% Vocational	−0.047	0.155**	0.126	0.318***	0.246	2.703
	(0.029)	(0.064)	(0.118)	(0.085)	(0.340)	(1.748)
% Primary	−0.006	0.003	0.053*	0.034	0.029	0.991***
	(0.006)	(0.011)	(0.030)	(0.025)	(0.111)	(0.352)
% Managers	−0.046***	0.013	0.000	0.184***	0.372***	0.873**
	(0.011)	(0.014)	(0.027)	(0.023)	(0.070)	(0.372)
% Blue collars	−0.043***	−0.019***	−0.006	0.179***	0.226***	0.172**
	(0.005)	(0.005)	(0.008)	(0.012)	(0.029)	(0.071)
Year						
2005	−0.007***	−0.005***	−0.008***	−0.095***	−0.079***	−0.066***
	(0.002)	(0.001)	(0.002)	(0.003)	(0.005)	(0.012)
2006	−0.024***	−0.018***	−0.021***	−0.119***	−0.084***	−0.066***
	(0.002)	(0.001)	(0.002)	(0.004)	(0.006)	(0.015)
2007	−0.034***	−0.028***	−0.031***	−0.161***	−0.146***	−0.101***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.007)	(0.016)
Intercept	0.082***	0.066***	0.015	−0.463***	−0.723***	−2.087***
	(0.008)	(0.011)	(0.028)	(0.026)	(0.107)	(0.396)

Note: Standard errors in parentheses. Controls included: year, hiring region, industry dummies and workforce characteristics which are lagged one year. Threshold value for the assignation to the treatment group: 33%. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* p < 0.1.
** p < 0.05.
*** p < 0.01.

Table 7
The impact of ALMPs on firm-level performance outcomes — panel DiD analysis.

	TFP			VAPC		
	Small	Medium	Large	Small	Medium	Large
Treatment effect	−0.021***	−0.005	−0.012	−0.001	0.021**	−0.001
	(0.006)	(0.010)	(0.022)	(0.007)	(0.010)	(0.022)
Incumbent characteristics						
Median age	0.008***	0.005***	0.013**	0.007***	0.007***	0.006
	(0.000)	(0.001)	(0.005)	(0.000)	(0.001)	(0.005)
% Females	−0.031	−0.007	−1.014***	−0.092***	0.028	−1.041*
	(0.021)	(0.067)	(0.324)	(0.022)	(0.071)	(0.555)
% University	0.070***	0.136*	0.038	−0.019	0.093	0.359
	(0.024)	(0.078)	(0.578)	(0.024)	(0.077)	(0.410)
% Vocational	0.318***	0.441	0.898	0.012	0.021	−0.038
	(0.102)	(0.433)	(1.598)	(0.109)	(0.425)	(1.321)
% Primary	0.003	−0.002	−0.404	−0.043*	−0.092	0.034
	(0.024)	(0.073)	(0.782)	(0.023)	(0.073)	(0.393)
% Managers	0.059	0.429***	0.482*	−0.005	0.424***	0.118
	(0.037)	(0.110)	(0.274)	(0.040)	(0.093)	(0.310)
% Blue collars	−0.033**	0.077**	−0.044	0.201***	0.352***	0.376***
	(0.014)	(0.033)	(0.073)	(0.017)	(0.034)	(0.082)
Year						
2005	0.044***	0.038***	0.037***	0.016***	0.007	0.005
	(0.004)	(0.006)	(0.014)	(0.004)	(0.006)	(0.012)
2006	0.063***	0.062***	0.050***	0.046***	0.046***	0.058***
	(0.005)	(0.008)	(0.018)	(0.005)	(0.008)	(0.016)
2007	0.147***	0.129***	0.134***	0.195***	0.117***	0.125***
	(0.005)	(0.009)	(0.021)	(0.006)	(0.009)	(0.019)
Intercept	3.877***	3.881***	4.170***	5.013***	4.969***	5.232***
	(0.026)	(0.079)	(0.601)	(0.026)	(0.081)	(0.387)

Note: Standard errors in parentheses. Controls included: year, hiring region, industry dummies and workforce characteristics for the current year. Threshold value for the assignation to the treatment group: 33%. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 8
The impact of ALMPs on firm-level performance outcomes with control for the share of new employees coming from unemployment — panel DiD analysis.

	TFP			VAPC		
	Small	Medium	Large	Small	Medium	Large
Treatment effect	−0.011	−0.005	−0.012	0.004	0.020**	−0.001
	(0.007)	(0.010)	(0.022)	(0.008)	(0.010)	(0.022)
SU	−0.059***	−0.022	0.114	−0.001	0.034	0.097
	(0.015)	(0.045)	(0.153)	(0.017)	(0.045)	(0.151)
Incumbent characteristics						
Median age	0.007***	0.005***	0.013**	0.004***	0.007***	0.007
	(0.000)	(0.001)	(0.005)	(0.000)	(0.001)	(0.005)
% Females	0.013	−0.008	−1.015***	0.005	0.026	−1.041*
	(0.024)	(0.067)	(0.324)	(0.024)	(0.071)	(0.555)
% University	0.026	0.136*	0.035	−0.066**	0.090	0.358
	(0.028)	(0.079)	(0.579)	(0.026)	(0.077)	(0.410)
% Vocational	0.222*	0.445	0.903	0.009	0.016	−0.031
	(0.121)	(0.433)	(1.596)	(0.115)	(0.425)	(1.322)
% Primary	−0.009	−0.001	−0.405	−0.048*	−0.094	0.033
	(0.027)	(0.074)	(0.782)	(0.025)	(0.073)	(0.393)
% Managers	0.022	0.431***	0.482*	0.075	0.423***	0.119
	(0.052)	(0.110)	(0.274)	(0.053)	(0.092)	(0.310)
% Blue collars	−0.074***	0.078**	−0.048	0.212***	0.351***	0.373***
	(0.017)	(0.033)	(0.074)	(0.018)	(0.034)	(0.082)
Year						
2005	0.048***	0.038***	0.038***	0.017***	0.007	0.006
	(0.004)	(0.006)	(0.014)	(0.005)	(0.006)	(0.012)
2006	0.069***	0.061***	0.053***	0.054***	0.046***	0.060***
	(0.006)	(0.008)	(0.018)	(0.006)	(0.008)	(0.016)
2007	0.160***	0.128***	0.137***	0.197***	0.119***	0.128***
	(0.006)	(0.009)	(0.022)	(0.006)	(0.009)	(0.020)
Intercept	3.887***	3.881***	4.162***	5.006***	4.971***	5.226***
	(0.029)	(0.079)	(0.599)	(0.028)	(0.081)	(0.388)

Note: Standard errors in parentheses. Controls included: year, hiring region, industry dummies and workforce characteristics for the current year. Threshold value for the assignation to the treatment group: 33%. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 9
Sensitivity analysis — panel DiD analysis.

	Small firms	Medium firms	Large firms
Baseline results			
SU	0.004**	0.003*	0.003
	(0.002)	(0.002)	(0.002)
Workforce turnover	0.015***	−0.001	−0.002
	(0.005)	(0.007)	(0.016)
TFP	−0.021***	−0.005	−0.012
	(0.006)	(0.010)	(0.022)
Value added per employee	−0.001	0.021**	−0.001
	(0.007)	(0.010)	(0.022)
Treatment firms defined with 50% threshold			
SU	0.004**	0.003	0.002
	(0.002)	(0.002)	(0.002)
Workforce turnover	0.017***	0.001	0.008
	(0.005)	(0.007)	(0.016)
TFP	−0.023***	−0.013	−0.030
	(0.006)	(0.010)	(0.023)
Value added per worker	−0.007	0.021**	0.003
	(0.007)	(0.010)	(0.024)
Control firms defined with 25% threshold			
SU	0.004*	0.003*	0.003
	(0.002)	(0.002)	(0.002)
Workforce turnover	0.014***	−0.002	−0.003
	(0.005)	(0.007)	(0.017)
TFP	−0.020***	−0.003	−0.008
	(0.006)	(0.010)	(0.022)
Value added per worker	−0.001	0.024**	−0.002
	(0.007)	(0.010)	(0.022)

Note: Parameter estimates for the interaction terms between treatment indicator and post-treatment years. Standard errors in parentheses. Controls included: year, hiring region, industry dummies and workforce characteristics. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 10
Placebo tests — panel DiD analysis.

	Small firms	Medium firms	Large firms
Firm-level hiring outcomes			
SU	0.005*	−0.002	0.005
	(0.003)	(0.003)	(0.004)
Workforce turnover	0.004	−0.002	0.021
	(0.006)	(0.010)	(0.022)
Firm-level performance outcomes			
TFP	−0.026***	−0.034**	−0.007
	(0.009)	(0.014)	(0.031)
Value added per employee	−0.002	−0.008	−0.006
	(0.009)	(0.014)	(0.028)

Note: Parameter estimates for the interaction terms between placebo treatment indicator and post-treatment years. Standard errors in parentheses. Controls included: year, hiring region, industry dummies and workforce characteristics. Threshold value for the assignation to the treatment group: 33%. Small firms: less than 20 employees; medium firms: between 20 and 100 employees; large firms: more than 100 employees.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

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