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Short communication

Worker-level and firm-level effects of a wage subsidy program for highly educated labor: Evidence from Denmark☆

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ABSTRACT

We study the effects of a Danish wage subsidy program for highly educated workers on the performance of the persons and firms participating in the program. Using data on the population of program participants, both workers and firms, we find that the program had positive effects on employment and annual earnings during program participation while there are no positive effects for the years after program expiration. At the employer-level, we find statistically significant effects on the number of highly educated employees for both the period of program participation and the subsequent time period. For the total number of employees we only find positive effects during program participation while there are no statistically significant effects for value added, net income, return on assets, wages per employee and labor productivity.

1. Introduction

Policy makers at the local, regional and national level have started a fair number of initiatives to combat increasingly high unemployment rates of high skilled labor. In May 2012, the Council of the European Union recommended "to adopt measures (...) aimed at increasing the employability of graduates leaving the education and training system".[1] A formal evaluation of such programs is, however, still lacking.

This paper studies the Danish "innovation assistant" (hereafter "IA") labor market scheme, a "targeted wage subsidy program" (Katz, 1996) for persons with a post-secondary (bachelor) or tertiary-level (master) education. The scheme served the dual purpose of getting more academics into employment and at transferring academic knowledge to SMEs since they have historically been reluctant to hire high qualified labor in Denmark and elsewhere, possibly due to information asymmetries on both sides. The IA program was launched by the Danish Agency for Science, Technology and Innovation (DASTI) in 2005 when the unemployment rate for high skilled workers was 3.7% and considered high given an average unemployment rate of 4.8% and the cost of educating academics.[2]

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[1] Source: http://www.parlament.gv.at/PAKT/EU/XXIV/EU/08/02/EU_80203/imfname_10027589.pdf. Appendix A provides an overview of initiatives that aim at bringing more academics into work.

[2] The program is more fully described at URL http://ufm.dk/en/research-and-innovation/funding-programmes-for-research-and-innovation/find-danish-funding-programmes/programmes-managed-by-innovation-fund-denmark/innovation-assistant/innovation-assistant.

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Our evaluation of the Danish IA program studies the effect of the subsidy on both persons and firms. We ask, (i) how do persons who participate in the program perform with regard to employment and income and (ii) how do participating firms perform in terms of employment, productivity and other success criteria. Existing studies on the effects of wage subsidies almost exclusively deal with programs geared at "the disadvantaged" (Katz, 1996), i.e., mostly low skilled workers.[3] In addition, comparatively little is known about the effects of training or wage subsidy programs on the performance of the firms involved and the long-run effects on wages and employment that generally tend to appear to be more positive than the short-run impacts (Card et al., 2010), possibly since they change the recruitment patterns of hiring firms in the longer run (Katz, 1996).

The general economic intuition behind wage subsidies is as follows: the subsidy is directly paid to the employer (and subsequently passed on to the IA), it hence shifts the labor demand rather than the labor supply curve to the right (Bell et al., 1999; Katz, 1996; Perloff and Wachter, 1979; Mofitt, 2002). Wages and employment will, however, increase by less than the value of the subsidy since employers will compete for the subsidized worker which in turn induces a higher labor supply as pointed out by Bell et al. (1999). The total effect of wages subsidies depends on the elasticity of labor supply – the greater it is, the smaller is the effect on wages and employment (Bell et al., 1999; Katz, 1996).

Eligible for the IA program were privately owned firms with at least two and at most 100 employees whereof no more than two may have been academics. In addition, firms needed to exist for at least one year and must make more than DKK 1 million (Euro 130,000) annually in revenues. SMEs needed to stipulate a specific development project that the IA was supposed to carry out. Firms that successfully applied for funding through the IA program received a wage subsidy of up to half of the IAs salary, with a maximum of DKK 12,500 (Euro 1700) per month – about half the average monthly wage in our data – for a period between six and twelve months. We do unfortunately not know much about how IA projects were initiated. Anecdotal evidence that we have collected does indicate, however, that it was mostly the potential IA who contacted the SME and suggested an employment relationship under the IA program. While we do not know anything about the mechanisms that match potential IAs with potential hosts, we do know that essentially all applications for wage subsidies were eventually granted. In order to cope with potential self-selection problems of firms and persons into the program, we apply "conditional differences in differences" (cDID) estimation methods (Heckman et al., 1999).

At the person-level, we find that the IA program had positive annual earnings – our measure of wages – and employment effects in the year of program participation as it should be by construction. There are no additional positive effects for participating persons in the subsequent years. For the first year after program expiration we even find a statistically significant negative effect which reflects the insignificance of the employment effect in the same year. At the employer-level, we find positive effects on the number of highly skilled employees for both the period of program participation and the subsequent time period.

2. Data

Our data stem from three main sources, (i) DASTI which contains information on individual IA-projects, an identifier for both the participating IA and the corresponding SME and the starting date of the project, (ii) Experian A/S, a credit rating agency whose financial reports have been used in prior research by Kaiser and Kuhn (2012) and which contains 1.7 mio. records on firms over the relevant time period and (iii) Statistics Denmark which provides population data on both persons and firms in Denmark that are linked to one another. Our final data set contains information on 364 IAs and 316 recruiting firms which we observe over a period of 6.7 years on average. Our set of control group observations essentially consists of the population of persons and firms in Denmark. We provide more details on our data set in Appendix B.

3. Empirical approach

For both the analysis of person-level and firm-level effects we first match treatment and potential control group observations on their observed characteristics in the year before entering the IA program, $t-1$. We subsequently run multivariate regressions on the matched treatment/control data.

3.1. Propensity score matching

To match treatment and control observations, we follow Kaiser and Kuhn (2012) by applying nearest neighbor caliper matching with a single neighbor and replacement. We match on the propensity scores which simply constitute the predicted probabilities of program participation which we calculate from binary logit regressions which control for a wide range of variables which affect both treatment choice and performance and that we measure at time $t-1$, the year before treatment. We stack these variables into matrix $\boldsymbol{X_{it-1}}$. The conditional probability of receiving treatment is $P[D_{it}|\boldsymbol{X_{it-1}}] = \boldsymbol{X_{it-1}}\beta + \epsilon_{it}$, where ϵ_{it} denotes a logistically distributed error term.[4]

3.2. Person-level analysis

We match our treatment group IAs to a total untreated population of 1,018,245 persons. Appendix C displays descriptive statistics of the variables involved in our estimations for both treated and control persons *before* matching and shows that program participants and control group persons differ substantially from one another with respect to basically all variables, an observation that is corroborated by our person-level program selection logit estimates displayed in Appendix D.

To start our person-level analysis we first remove any potential control group observations with characteristics not observed in the set of treated persons. Our set of conditioning variables of the propensity score matching model $\boldsymbol{X_{it-1}}$ includes (i) demographic information like age, gender and marital status, (ii) information on the persons' highest level of formal education which includes a total of 15 different categories, (iii) a dummy variable that specifies whether or not the person is currently enrolled in an education program, (iv) the average high school grade and sets of dummy variables for the persons' high school majors, (v) a person's occupational status like employment, unemployment or parental leave,[5]

[3] Reviews of the extant literature are provided by Card et al. (2010), Dar and Tzannatos (1999), Heckman et al. (1999), Kluve (2010) as well as Martin and Grubb (2001).

[4] We note at this stage already that all our matched control observations are on the "common support", i.e., persons and firms with the same observed characteristics have a positive probability of receiving both treatment and non-treatment (Heckman et al., 1999). All estimations are performed using Stata 11.0. We use the "psmatch2" module by Leuven and Sianesi (2003) implemented in Stata to perform our propensity score matching estimations.

[5] Note that we do *not* compare persons in treatment with persons who are unemployed as most of the empirical evaluation literature does. We compare persons with the same occupational status, e.g., unemployed to unemployed, wage-employed to wage-employed, etc.

(vi) wage income, (vii) years of labor market experience and (viii) geographical location of residence.

We use the propensity score generated by our logit model to match our treatment to our control group observations, matching exactly in terms of education, gender and occupational status. After matching, the once strong differences in observed characteristics vanish. Since we condition on the propensity score alone instead of the individual conditioning variables, we need to assess if our selected set of control group observations indeed match well with our set of treatment observations. To this end, we use Rosenbaum and Rubin's (1985) "standardized biases", the mean differences between treatment and control group observations before and after matching, weighted by their standard deviations. None of these differences are statistically significant. As an additional match quality check that was suggested by Sianesi (2004), we run a logit model for treatment on the matched data and find a pseudo R^2 of 0.032 and cannot reject that the covariates are statistically highly insignificant – as they should if treatment and control observations are appropriately matched. These tests hence indicate that our control group is well matched to our treatment group based on observed person characteristics.

3.3. Firm-level analysis

Our firm-level analysis proceeds in the same steps as our person-level one. We first select a pool of potential controls in the Experian data by removing firms operating in industries without any participating firm, with ownership types not found among the treated firms and companies larger than 150 employees since they are not eligible for program participation. Our final set of potential control group SMEs includes information on 296,000 firms.

We partition our set of participating firms by year and industry. Within each group, we identify matched control group observations on the basis of the propensity score. Our set of conditioning variables $\boldsymbol{X_{it-1}}$ includes sets of (i) industry dummies, (ii) the number employees and their formal education, (iii) net income, (iv) return on assets, (v) labor costs per employee, (vi) labor productivity, (vii) total assets, (viii) equity share, (ix) short-term debt and (x) time dummies.

Appendix E displays descriptive statistics for the variables we include in our analysis. As for the person-level analysis, we find major and statistically significant differences between treatment and control group observations before matching that are again reflected by our logit regression results displayed in Appendix F. As shown in Appendix E, the formerly substantial and economically as well as statistically significant differences between firms before matching vanish after we have matched them on their propensity scores and they are no longer statistically significant. In addition, the pseudo R^2 of a logit regression on the matched data is 0.04 with the corresponding test for joint significance being statistically highly insignificant.

4. Results

4.1. Person-specific results

To evaluate the effect of program participation, we run two year-by-year "performance" regressions to allow the treatment effects to vary over time. In our first regression, a logit model, we consider whether or not a person is in wage-employment WE_{it} conditional on treatment D_{it} and a similar set of explanatory variable that we used for propensity score matching, $\boldsymbol{X_{it-1}}: P[WE_{it}|D_{it}, \boldsymbol{X_{it-1}}] = \alpha D_{it} + \boldsymbol{X_{it-1}}\delta + \xi_{it}$, where ξ_{it} denotes a logistically distributed error term. We estimate this equation by separate year-by-year regressions.[6]

Table 1
Year-by-year ATT logit estimation results for IA-treatment person being employed in $t=0$ to $to=4$ on matched data.

Treatment effect in	Coeff.	Std. err.	# Obs.
$t=0$	2.365***	0.350	568
$t=1$	0.271	0.293	486
$t=2$	0.577	0.354	383
$t=3$	−0.423	0.408	286
$t=4$	0.282	0.716	199

Notes: the logit model is run on the matched data and includes the following additional sets of control variables that were set to their pre-treatment $(t-1)$ values: age, gender, annual wage, years of experience, marital status, schooling, occupation, immigration and region. It also contains a set of time dummies. The asterisk *** denotes statistical significance at the 1% level.

Table 2
Fixed effects regression results for absolute change in annual income.

Treatment effect for time period	Coeff.	Std. err.	# Obs.	R^2
$t=0$	50,153.12***	11,487.56	4805	0.0422
$t=1$	−30,948.98***	12,572.4		
$t=2$	−10,544.71	14,138.51		
$t=3$	−21,014.23	16,168.94		
$t=4$	−18,001.97	23,859.21		

Notes: the fixed effects model is run on the matched data. The asterisk *** and * denote statistical significance at the 1% and 10% significance level.

In our second regression, we study the change of person i's income I_{it} before and after treatment using fixed effects: $I_{it} - I_{it-1} = \gamma D_{it} + \mu_k + \eta_{it}$, where μ_i denotes a person-specific effect and η_{it} is the usual error term. Persons enter the program at $t=0$. All control variables that we used to estimate the propensity score drop out from our fixed effects regression since they are time-invariant (all measured at time $t-1$).

Table 1 displays our cDiD estimation results for the probability of being wage-employed at a time period subsequent to having entered treatment. We only show the treatment effect dummy variable for brevity. The table shows that there are only statistically significant effects for the year in which a person enters the IA program ($t=0$) while there are no such effects for any of the four later time periods we consider.

The results of our annual earnings increase regressions, shown in Table 2, are even more gloomy: we do of course also find one-off effects for the year of program participation which basically implies that the participating firms pass on the wage subsidy to the IAs as they should. We do, however, also find a statistically significant and negative effect the year after program participation. The first year effect is 50,153 DKK while the second effect is −30,948 DKK. This is in line with our previous finding of one-off effects of the program on employment. Adding up the coefficient estimates shows that there no longer is a statistically and economically significant difference between treatment and control observations two years after program participation. The treatment effect on annual earnings hence is below the value of the subsidy as theory suggests (Bell et al., 1999).

4.2. Firm-specific results

In order to assess the programs effect of firm performance, we estimate dynamic fixed-effects regressions for our seven firm

[6] If we estimate it using fixed effects, we loose 30% of our observations – all persons who do not change their employment status. Our fixed effects estimation results are, however, both quantitatively and qualitatively very similar to the year-by-year ones.

Table 3
Fixed effects regression results for alternative outcome variables on matched data in $t=0$ to $t=4$.

	Coeff.	Std. err.	# Obs.	# Firms	R^2
# Highly educated employees					
$t=0$	0.458***	0.12	2609	535	0.03
$t=1$	0.318**	0.14			
$t=2$	0.01	0.17			
$t=3$	−0.14	0.21			
$t=4$	−0.22	0.26			
# of employees					
$t=0$	0.596**	0.30	2611	533	0.08
$t=1$	0.00	0.34			
$t=2$	0.33	0.40			
$t=3$	−0.45	0.60			
$t=4$	−0.69	0.65			
Value added (in 1000 DKK)					
$t=0$	219.3	217.2	2611	533	0.04
$t=1$	374.1	239.6			
$t=2$	165.2	324.2			
$t=3$	124.0	448.2			
$t=4$	−563.1	580.3			
Net income (in 1000 DKK)					
$t=0$	−48.5	95.2	2553	542	0.03
$t=1$	136.5	111.4			
$t=2$	133.3	122.1			
$t=3$	205.5	218.1			
$t=4$	−103.5	189.2			
Return on assets					
$t=0$	−0.03	0.02	2669	544	0.02
$t=1$	−0.04	0.03			
$t=2$	0.00	0.03			
$t=3$	−0.04	0.04			
$t=4$	−0.04	0.06			
Wage per employee (in 1000 DKK)					
$t=0$	8.28	11.26			
$t=1$	5.25	10.27	1494	346	0.01
$t=2$	−21.34	13.79			
$t=3$	16.06	17.18			
$t=4$	−19.32	29.92			
Labor productivity (in 1000 DKK)					
$t=0$	−27.92	93.59	1693	323	0.02
$t=1$	57.28	107.90			
$t=2$	−137.30	91.84			
$t=3$	−39.23	134.00			
$t=4$	−159.70	215.30			

Notes: all models are estimated using fixed effects on the matched data. They also include a set of time dummies and a constant term. The asterisk *** and ** denotes statistical significance at the 1% and 5% significance level.

performance indicators number of employees, number of highly educated employees, value added, net income, return on assets, annual earnings per employee and labor productivity on our matched data. Our estimation equation is:

$$Y_{kt} - Y_{kt-1} = \sum_{j=0}^{4} \left(\alpha_j D(t_i = j) + \beta_j (D(\text{treat} = 1) * D(t_i = j))\right) + \boldsymbol{T_t}\boldsymbol{\rho} + u_k + \eta_{kt},$$

where Y denotes the respective performance measure of firm k at time t, vector $\boldsymbol{T_t}$ denotes a set of (calendar) time dummies, and the Ds denote dummy-operators that are coded one if the respective condition in parentheses holds. The specification accounts for fixed effects u_k which is why all time-invariant variables such as our set of conditioning variables, all measured in the year before treatment, used in the previous steps drop out. The α coefficients denote time fixed effects and the β coefficients denote the year-specific treatments effects and η_{kt} is the usual error term.

Table 3 displays our cDiD fixed effects estimation results. The coefficients displayed correspond to the difference IA program participation makes for our outcome variables. We again and unsurprisingly find a positive one-off effect of IA participation of 0.458 additional highly educated workers due to program participation which implies that the treatment effects are again below the value of the subsidy. This may, however, just be the consequence of the program lasting between six months and one year which implies that we may not fully grasp employment effects for contracts that lasted for less than one year. We also find a positive high skilled employment effect of 0.318 workers for the subsequent year. Finally, we also document a positive effect on total employment of 0.596 additional workers. This might reflect that firms did not only hire additional highly skilled workers but on top of them also recruited complementary workers with a lower formal education.

We do neither find any evidence for the program having any effect on any of these variables in subsequent years nor do we find any statistically significant effects for the other outcome variables we consider.

4.3. Robustness checks

We finally run two robustness checks. We first test if the "common trends" assumption holds, i.e., if persons and firms who participated in the program had developed like control-group persons and firms in the absence of treatment. We do that by additionally including interactions of time and treatment dummies three years prior to treatment. These additional terms are both separately and jointly statistically grossly insignificant both in our person-level and firm-level regressions. This shows that there is no evidence for the common trend assumption not to hold. We hence drop the additional pre-trend terms from our main analysis.

Secondly, we run all models except for the binary outcome of employment/unemployment using OLS instead of fixed effects to check if our mostly insignificant findings are driven by a lack of variation in our data which would lead to a weak identification in fixed effects. For the total number of employees and value added we find statistically significantly positive effects on the second and third lag, respectively, where the fixed effects results were statistically insignificant. For the annual income per worker we find a statistically significantly negative effect on the second lag if we use OLS instead of fixed effects. Our point estimates are, however, quantitatively not much different from one another. There are no changes from insignificant to significant effects for the other outcome variables annual income, number of highly educated labor, net income, labor productivity and return on assets. Our plain OLS results hence let appear the IA program in a slightly less gloomy light.

5. Conclusions

This paper sought to contribute to the recent debate on active labor market programs for high skilled labor by analyzing the consequences of a wage subsidy program for workers with higher secondary and tertiary education that was launched in Denmark in 2005, the "Innovation assistant" (IA) scheme. We used data on the population of persons and firms enrolled in the program to study the effects of this policy measure on both persons' labor market outcomes and firms' performance using conditional difference-in-differences analysis where we control for a wide range of observed characteristics.

At the person-level we find that the program only had effects on employment and annual earnings during program participation. At the firm level, while the program of course increased both the number of workers and the number of high-skilled workers during program participation, these effects were short-lived and wore out after program expiration.

The program did not only aim at bringing unemployed academics into work, it was additional geared at bringing academic knowledge into SMEs. We additionally did consider a direct innovation-related variable, patent application counts using the data explored in Kaiser et al. (2015). There were only two firms with at least one patent during the period under investigation so we needed to drop this analysis.

The IA program has been replaced by the "InnoBooster" (http://innovationsfonden.dk/da/investeringstype/innoboster) in February 2015. The new scheme also pays wage subsidies to firms hiring academics but that has much stricter requirements for program eligibility. The application must now be more detailed in pinning down what exactly the new innovative activities of the firm will be, and, most importantly, that the project is commercially relevant to the SME. An evaluation of this more precisely defined program is left for further research. Another area for further research is to study possible positive productivity spillover effects from the highly skilled wage subsidized workers to workers with a lower formal qualification. Finally, wage subsidies affect "incremental hirings" (Perloff and Wachter, 1979, p. 174) which implies that IA program participation should lead to additional employment for growing firms in particular, an issue that we leave for future research as well.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.respol.2016.05.008.

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