*Anže BURGER**

EVALUATING THE EFFECTIVENESS OF ANTI-CRISIS STATE AID MEASURES

METHODOLOGICAL APPENDIX TO THE ARTICLE "WHAT CAN BE LEARNT FROM THE EFFECTIVENESS OF SLOVENIA'S ANTI-CRISIS STATE AID MEASURES DURING THE GREAT RECESSION: APPLICATION TO THE COVID-19 DOWNTURN"

Abstract. *This methodological contribution is explaining selected empirical methods useful for evaluating the effectiveness of state aid measures in order to separate the causal effect from the effect due to non-random assignment of the treatment. These methods were employed in the analysis of the Effectiveness of Slovenia's Anti-crisis State Aid Measures During the Great Recession. Methodological note is complementary to of the article entitled "What Can Be Learnt from the Effectiveness of Slovenia's Anti-crisis State Aid Measures During the Great Recession: Application to the Covid-19 Downturn". First, we explain the propensity score matching (PSM) method, followed by difference-in-differences regression (DiD). Finally, we discuss the value of using both methods and include some auxiliary tables and figures.*

Keywords: *effectiveness of anti-crisis measures, propensity score measure, difference-in-differences regression*

Introduction

Different matching estimators are often applied in treatment evaluation to estimate average treatment effects of a program. When selection into the program (state aid allocation in our concrete case) is performed based on the observable characteristics, one has to adjust for the different distributions of the observed characteristics in the treated (subsidized) and the non-treated (non-subsidized) sample when evaluating a socio-economic program. In this way, we are able to separate the causal effect from the effect due to non-random assignment of the treatment. In the next sections we described the propensity score matching and difference-in-differences regression as two estimators with which we estimated the effects of anti-crisis state aid measures handed out to Slovenian firms during the Great Recession.

* *Anže Burger, PhD, Associate Professor, Faculty of Social Sciences, University of Ljubljana, Slovenia.*

Propensity score matching

We study the effect of anti-crisis state aid on recipient firms' employment and revenue using the counterfactual framework pioneered by Rubin (1974). The Rubin causal model is based on the concept of a counterfactual, where each firm has an outcome with and without treatment (state aid in our case). We denote firm *i*'s pair of potential outcomes with Y_{i0} and Y_{i1}, where the former denotes the outcome with treatment and the latter the outcome without treatment. We denote the treatment status of a firm with D_i, where D_i=1 if the firm receives state aid and 0 otherwise. The following exposition assumes that the treatment of firm *i* affects only the outcome of unit *i*, known as stable unit treatment value assumption (SUTVA) in the treatment literature.

To measure the effect of state aid, we are interested in the difference in the outcomes with and without treatment: $Y_{i1}-Y_{i0}$. Since this quantity is a random variable, we focus on the average treatment effect on the treated, which we denote as $ATET = \mathrm{E}(Y_1 - Y_0|D \equiv 1)$. Namely, ATET is the mean effect for firms who actually received state aid. To calculate this statistic, both potential outcomes are required, yet we observe only one for each firm: $Y_i = (1 - D_1)\ Y_{i0} + D_i Y_{i1}$, where Y_i is the observed outcome. This fundamental missing observation problem (Holland, 1986) can be overcome if we can rely on an assumption called conditional mean independence (CMI) (Rosenbaum and Rubin, 1983): a) $\mathrm{E}(Y_0|\boldsymbol{X}, D) = \mathrm{E}(Y_1|\boldsymbol{X})$; and b) $\mathrm{E}(Y_1|\boldsymbol{X}, D) = \mathrm{E}(Y_1|\boldsymbol{X})$. That is, if we can observe enough information (contained in a vector of observed covariates $\boldsymbol{X}$) that determines the treatment status, then (Y_1, Y_0) might be mean independent of D, conditional on $\boldsymbol{X}$. In other words, even though (Y_1, Y_0) and D might be correlated, they are uncorrelated once we partial out $\boldsymbol{X}$.

Following from the CMI assumption, we can write the ATET conditional on the observed covariates as $ATET\,(\boldsymbol{X}) = \mathrm{E}(Y|\boldsymbol{X} = x\,, D = 1) - D = 1)$ that is a function of all observable quantities (Cerulli, 2015 pp. 70–71). Averaging over the support of $\boldsymbol{X}$ yields an estimate of the unconditional ATET. Matching on a set of covariates $\boldsymbol{X}$ is feasible only when $\boldsymbol{X}$ has a very small dimensionality. If the set of covariates is large and many of them take multiple discrete values or they are continuous variables, we can avoid this dimensionality problem by matching units instead according to the propensity score. The latter is defined as the probability of being treated (receiving state aid) conditional on $\boldsymbol{X}$, and is represented by a single scalar dimension, $p(\boldsymbol{X}) = \Pr(D|\ \boldsymbol{X})$. Stratifying units according to $p(\boldsymbol{X})$ produces the same orthogonal condition between the potential outcomes and the treatment that is stratifying on $\boldsymbol{X}$, but with the advantage to rely just on one dimension variable (Unconfoundedness property). In addition, if the propensity score is correctly specified, then we should observe that firms matched according to the p(X) should be indistinguishable in terms of their X (they are balanced; Balancing of confounding variables property). We test empirically whether the balancing property holds to make sure that the correct propensity-score is being used to stratify firms.

We estimate ATET using the one-to-one nearest-neighbour (1-NN) and multiple-nearest-neighbours propensity score matching methods. The basic logic is to select for each subsidised firm one or more control firms with a similar propensity score that have not received state aid. In 1-NN matching, each treated firm is matched with a single control firm whose propensity score is closest. Alternatively, we match each subsidised firm with the three closest control firms (3-NN). As is common in empirical applications, we impose the common support restriction, dropping out all those controls whose p(X) values are either higher or smaller than that of the treated units, and set a calliper to 0.05, so that those treated firms with no matches within the caliper are eliminated (see Caliendo and Kopeinig, 2008). We report robust standard errors as of Abadie and Imbens (2006, 2016), who provided the correct formulas and estimation of the variances for the nearest-neighbour matching when matching is performed on a parametric estimation of the propensity score.

We focus on two outcome variables in the PSM stage of analysis: employment and total revenue. We estimate ATET using a Matching-DiD hybrid method, a combination of the difference-in-differences approach with propensity score matching (Heckman et al., 1998; Smith and Todd 2005). This estimator is similar to the classical DiD, but does not demand the imposition of the linear-in parameters form of the outcome specification. In essence, it can be regarded as a nonparametric DiD, reweighting observations determined by a weighting function contingent on the specific matching strategy adopted (Cerulli, 2015: 198–199). The advantage of applying the hybrid method is that the DiD part controls for the selection on the time-invariant part of the unobservable heterogeneity by differencing out an individual firm's fixed effects. Average treatment effect on the subsidised firms years after the year in which state aid was granted is estimated as follows:

$$\widehat{ATET}_{\tau} = \frac{1}{N} \sum_{i \in \{D\}} \left(\left(Y_{i,t_0+\tau}^{D=1} - Y_{i,t_0-1}^{D=1} \right) - \sum_{j \in C(i)} h(i,j) \left(Y_{j,t_0+\tau}^{D=0} - Y_{j,t_0-1}^{D=0} \right) \right)$$

where N is the number of subsidised firms, $i \in \{D\}$, C is the non-subsidised set of control firms, $Y_{i(j),t_0+\tau}^{D=1(D=0)}$ is the size (total revenue or employment) of subsidised (control) firm i (j) τ years after the state aid year t_0, $h(i,j)$ are the matching weights that depend on the type of matching estimator. *ATET* tells us by how much more (or less) revenue or employment has grown in subsidised firms compared to similar control firms from pre-subsidy year t_{0-1} to τ years after the state aid utilisation year t_0.

Matching procedure begins with an estimation of propensity scores, $\Pr(D_{it}|X_{it})$, with a probit model. We perform the estimation for each anti-crisis state aid category and each year separately to allow for different determinants between measures and years. We include the following regressors as determinants to receive state aid: lagged growth rate of revenue (*Revenue growth*$_{t-1}$) and employment (*Emp. growth*$_{t-1}$), lagged log of employment (*lnEmployment*$_{t-1}$), dummy for zero employees in the previous year (*I(Emp.=0)*$_{t-1}$), lagged log of total revenue (*lnRevenue*$_{t-1}$), dummy for zero revenue in the previous year (*I(Revenue=0)*$_{t-1}$), dummy for exporter status (*Exporter*$_{t-1}$), share of exports in total revenue (*Export share*$_{t-1}$), lagged labour productivity (*VA/Emp.*$_{t-1}$), lagged debt-to-assets ratio (*Debt/Assets*$_{t-1}$), dummy for debt-to-assets ratio exceeding 1 (*I(Debt/Assets>1)*$_{t-1}$), lagged return on assets (*ROA*$_{t-1}$), lagged log of average wage (*lnAvg. Wage*$_{t-1}$), lagged dummy for zero average wage reported (*I(Avg. Wage=0)*$_{t-1}$), lagged dummy for foreign ownership (*For. Ownership*$_{t-1}$), lagged dummy for outward FDI (*Outward FDI*$_{t-1}$), cumulative number and value of different state aid measures utilised from 1998 to the previous year (*Num. Grants CUM*$_{t-1}$ and *Val. Grants CUM*$_{t-1}$, respectively), current age of the firm (*Age*) and a dummy for whether it was established before 1995 (*I(Est. before 1995)*). To make a better fit, we include second-order polynomial terms of the continuous variables in the regression. We also include region, year and 2-digit industry dummies. The probit models are estimated with all firm-year observations of our pool of possible matching partners from 2009 to 2015, whereas for subsidised firms, we only include the observations of the years before and on the year state aid was granted. The estimation results of probit models are presented in the Appendix Table A1, where we report average marginal effects of the main explanatory variables. The probit estimations produced a fairly good model fit (McFadden Pseudo-R^2 between 0.15 and 0.52), demonstrating a sufficient explanatory power of the regressors included. We use these model estimates to predict propensity scores for each observation in our sample.

Next, we perform the matching procedure. To obtain comparable control firms from our pool of potential matching counterparts, we implement nearest neighbour PSM with replacement based on the estimated propensity scores for each state aid category separately. Later, we also combine PSM with elements of exact matching by matching each subsidised firm with a control firm whose propensity score is closest conditional on having the exact same 2-digit NACE code and year of observation. For a robustness check, we report estimates with two different counterfactual groups: single nearest neighbour (1NN) and three closest neighbours (3NN). Allowing for multiple neighbours improves the precision of our estimates, but there is the trade-off with the increased bias. To implicitly check for the unconfoundedness assumption, we also report the matching estimates for the outcome variable growth of revenue (employment) in the year prior to the state aid allocation (DIF_{t0-1}), obtained by replacing $t_0 + \tau$ and $t_0 - 1$ from Equation (1) with $t_0 - 1$ and $t_0 - 2$, respectively. This way we check whether growth of revenue and employment in the subsidised firms differed significantly from control firms already before the aid was allocated. Ideally, growth trajectories just before the state aid allotment would be similar in both groups of firms, revealed through a statistically non-significant pre-subsidy effect DIF_{t0-1}. This is important in order to make sure that the post-subsidy firm growth is not caused by dissimilar pre-subsidy developments.

After the PSM procedure, it is important to evaluate how well the treatment and comparison groups are balanced in the matched samples to ensure that the balancing of confounding variables given the propensity score holds. To check whether the distribution of the covariates to be balanced between the treatment and matched control group, we compare the mean values of the covariates after the matching procedure (Appendix Table A2). Firms in our matched control group generally exhibit similar characteristics to state aid recipients. The only statistically significant differences between the selected control group and subsidised firms are identified with respect to firm ROA for the Employment state aid and R&D state aid categories, with respect to the cumulative number of distinct state aid measures used in the past for the Employment state aid and Training state aid categories and in terms of age for the Employment state aid category. In addition, Kernel densities were plotted to examine the distributions of propensity scores across the matched treatment and comparison groups and were reasonably similar for all five anti-crisis state aid categories examined (Appendix Figure A1). The results suggest that the matching procedure was successful in identifying valid counterfactuals for the subsidised firms in all five groups of anti-crisis state aid.

Difference-in-differences regression

Employing 3-NN PSM, we create the matched sample of subsidised firms and the corresponding control firms. We use this matched sample to estimate the effect of anti-crisis state aids on revenue and employment growth in a difference-in-differences (DiD) regression setting. This allows us to control for relevant factors that influence firm growth, as well as identify the mediating factors of state aid effectiveness. For each subsidised firm and their matched controls, we construct a window around the state aid grant year t_0 and use observations from $t_0–1$ to $t_0+\tau$, where τ = 0, 1, 2, ..., 5. In this way, we apply a time-variant treatment DiD framework with post-treatment effects and recalibrate all acquisition calendar years to technical years t around t_0 denoting the calendar year when the state aid was administered. We do the same translation to technical time for all subsidised firm's control firms. We combine the dynamic specification of a Gibrat law panel data model with the DiD setting and estimate the following specification:

$$Y_{i,t} = \beta_0 + \beta_1 Y_{i,t_0-1} + \beta_2 \boldsymbol{X}_{i,t} + \beta_3 D_i + \sum_{\tau=0}^{5} \gamma_\tau T_\tau + \sum_{\tau=0}^{5} \delta_\tau (D_i * T_\tau) + R_i + I_i + \theta_t + \varepsilon_{i,t}$$

where $Y_{i,t}$ is the size (total revenue or employment) of firm i in year t and Y_{i,t_0-1} is the size of firm i one year before the state aid year t_0. Controlling for constant pre-subsidisation firm size enables us to estimate the post-aid cumulative effect on firm growth from year $t_0–1$ to $t_0+\tau$. This is equivalent to DiD treatment effect from the non-parametric PSM estimation above. As in the standard DiD setting, we include a set of dummies $T\tau$ that indicate the specific post-subsidy period. T_0 designates the period in which the state aid was administered to the subsidised firms and the corresponding counterfactual period in the matched non-subsidised control firms. Likewise, T_5 indicates 5 years after the state aid utilisation year and hence enables us to estimate the long-term cumulative effect on firm growth over a 6-year period from $t_0–1$ to t_0+5. A set of dummies of the utmost importance, $D_i * T_\tau$, designate whether a firm was acquired in the current year ($\tau = 0$), 1 year before ($\tau = 1$) and so on, or 5 years before ($\tau = 5$). To serve as a benchmark period against which post-subsidy periods are compared, we also include observations of the outcome variable 1 year prior to state aid deployment ($t = t_0–1$). The corresponding lagged dependent variable (Y_{i,t_0-1}) refers in this case to the preceding year ($t_0–2$).

Parameters δ_τ therefore identify the cumulative effect of anti-crisis state aid on subsidised firms' employment and revenue growth above that in the pre-subsidy period. In other words, δ_τ shows us by how much more (or less) subsidised firms grew in size compared to similar non-subsidised firms from pre-subsidy year t_0-1 to post-subsidy year $t_0+\tau$. The vector of controls consists of three groups of variables. First, we control for a set of pre-treatment variables to explain the heterogeneity of post-subsidy growth. These include log of labour productivity (*lnVA/EMP*$_{t0-1}$), log of capital-labour ratio (*lnK/EMP*$_{t0-1}$), log of average wage (*lnAvgWage*$_{t0-1}$), share of exports in total revenue (*Export share*$_{t0-1}$), dummy for foreign ownership (*For. ownership*$_{t0-1}$) and dummy for outward foreign direct investments (*Outward FDI*$_{t0-1}$). Second, to control for industry-year-specific demand shifts, we include the 2-digit industry growth rate of value added in the current year (*Industry VA growth*). Third, we include indicators for the state aid instruments and the value of state aid administered. This allows us to identify which type of aid is more effective and what is the elasticity of growth effects on the size of the subsidy. A set of dummy variables indicates the following state aid instruments: grants (*Grant*), interest rate subsidies (*Inter. rate subs.*), fundamental R&D (*Basic R&D*), tax credits (*Tax credit*), social contributions credits (*Soc. contrib. relief*), guarantees (*Guarantee*) and other instruments (*Other instruments*). The value of anti-crisis state aid in t_0 ($lnVALUEcrisis_{t0}$) and possible non-crisis state aid in the same year ($lnVALUEnoncrisis_{t0}$) is included to control for state aid volume, as well as to allow for other types of aid to affect treated and control firms' growth. Finally, we also control for firm age (*Age* and dummy for firms established before 1995) and include region dummies (R_i), industry dummies (I_i) and calendar year dummies (θ_τ) that capture time-varying macroeconomic shocks common to all regions, industries and firms. θ_τ dummies also control for mediating effect of business cycle on the growth of firms. We estimate specification (2) with weighted least squares, using analytic weights provided by the 3-NN PSM procedure. Namely, weights attributed to the selected control firms correspond to the number of controls chosen for each treated firm and number of times a firm was selected as a control unit (we ran matching with replacement).

Conclusion

In this article, we described propensity score matching and difference-in-differences regression as two estimators with which we estimated the effects of anti-crisis state aid measures handed out to Slovenian firms during the Great Recession. The first method, propensity score matching, matches each subsidized firm with one (or more) non-subsidized control firm based on the degree of similarity in the estimated probabilities of receiving state aid. The average effect of the program is estimated by the mean difference in the outcomes of the matched pairs of firms. In the difference-in-differences regression we pooled matched samples of all five anti-crisis state aid categories in order to uncover potential mediating factors that are common to all state aid measures and determine their effectiveness. Namely, we were interested in identifying whether the pre-treatment characteristics of the recipient firms, amount of state aid granted, type of the state aid instrument and industry-specific business cycle moderate the size of the treatment effect.

Appendix: tables and figures

Table A1: PROBABILITY OF OBTAINING STATE AID (AVERAGE MARGINAL EFFECTS), 2009–2015

	Primary	Employment	Resc.&Restr.	Training	R&D
Revenue growth$_{t-1}$	-0.00154**	0.0168***	0.000280	0.00134**	0.00226***
	(0.000604)	(0.00112)	(0.000312)	(0.000619)	(0.000503)
Emp. growth$_{t-1}$	-0.00107	0.00465***	-0.000760*	0.000458	0.00146**
	(0.000709)	(0.000897)	(0.000455)	(0.000586)	(0.000577)
lnEmployment$_{t-1}$	0.00275***	0.0212***	0.000351*	0.00507***	0.00592***
	(0.000366)	(0.000784)	(0.000198)	(0.000350)	(0.000376)
I(Emp.=0)$_{t-1}$	0.00531**	0.00176		0.00512***	0.00594***
	(0.00233)	(0.00231)		(0.00147)	(0.00127)
lnRevenue$_{t-1}$	0.000877***	0.00691***	-1.11e-05	0.00243***	-5.23e-05
	(0.000325)	(0.000638)	(0.000168)	(0.000317)	(0.000323)
I(Revenue=0)$_{t-1}$		0.219***			-0.0690***
		(0.0228)			(0.00702)
Exporter$_{t-1}$	0.00116**	0.00404***	0.000195	0.00222***	0.00516***
	(0.000566)	(0.00136)	(0.000327)	(0.000583)	(0.000652)
Export share$_{t-1}$	0.00228*	-0.00807	0.000385	-0.00131	0.0124***
	(0.00121)	(0.00619)	(0.000357)	(0.00212)	(0.00157)
VA/Emp.$_{t-1}$	1.01e-08	-6.83e-08***	-1.13e-08	-2.34e-08**	1.12e-08
	(1.33e-08)	(1.90e-08)	(1.17e-08)	(9.57e-09)	(9.26e-09)
Debt/Assets$_{t-1}$	0.0114***	5.62e-06	0.00204***	0.00107	0.000265
	(0.00111)	(0.000694)	(0.000514)	(0.000681)	(0.000209)
I(Debt/Assets>1)$_{t-1}$	-0.00639***	-0.0132***	3.51e-05	-0.00585***	-0.00929***
	(0.00170)	(0.00197)	(0.000342)	(0.00127)	(0.00131)
ROA$_{t-1}$	-0.00467	0.00162*	-0.00164	-0.00115	0.000350
	(0.00299)	(0.000927)	(0.00106)	(0.00111)	(0.000607)
lnAvg. Wage$_{t-1}$	0.00126*	-0.0183***	-0.000475	0.00201***	0.00972***
	(0.000754)	(0.00134)	(0.000517)	(0.000692)	(0.000669)
I(Avg. Wage=0)$_{t-1}$		0.272***		0.0416	-0.00981
		(0.0478)		(0.0399)	(0.0304)
For. Ownership$_{t-1}$	-0.00478***	-0.0337***	-0.000607	-0.00577***	-0.00825***
	(0.000825)	(0.00202)	(0.000453)	(0.000838)	(0.000873)
Outward FDI$_{t-1}$	0.00191***	-0.0179***	-0.000357	-0.00260**	0.00567***
	(0.000590)	(0.00323)	(0.000302)	(0.00103)	(0.000922)
Num. Grants CUM$_{t-1}$	9.49e-06	0.00126***	2.45e-06	0.000247***	0.000900***
	(1.45e-05)	(4.40e-05)	(7.41e-06)	(1.92e-05)	(2.60e-05)
Val. Grants CUM$_{t-1}$	-6.58e-11	-9.37e-11	2.00e-10***	7.48e-11	2.94e-09***
	(1.10e-10)	(3.87e-10)	(5.67e-11)	(3.02e-10)	(2.71e-10)
Age	-0.000159	0.000959***	4.48e-06	-9.74e-05	-0.000939***
	(0.000222)	(0.000172)	(0.000105)	(9.31e-05)	(8.92e-05)
I(Est. before 1995)	0.00114	-0.0175***	0.00105*	-0.00225**	0.00207**
	(0.00123)	(0.00208)	(0.000627)	(0.000934)	(0.000997)
Region FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes

	Primary	Employment	Resc.&Restr.	Training	R&D
Observations	116,078	257,906	80,186	250,742	253,701
Pseudo R2	0.4603	0.1501	0.5233	0.2273	0.4093

Notes: For each state aid category, probit results refer to the estimates for the period 2009–2015. Average marginal effects are based on the second-order polynomial regression specification. *** p<0.01, ** p<0.05, * p<0.1

Source: own calculations.

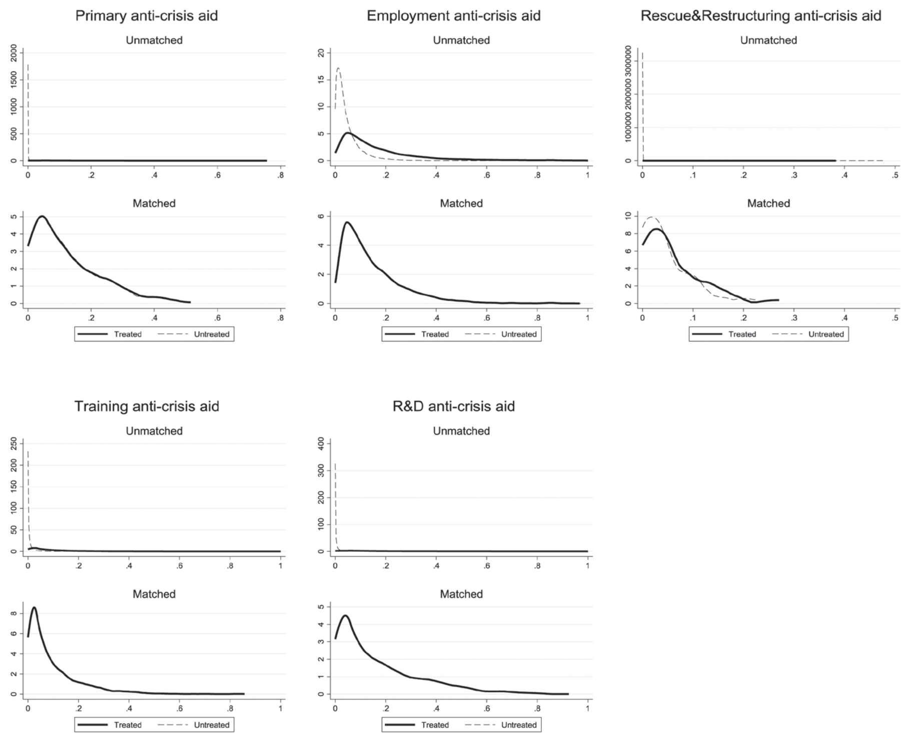
Table A2: T-TESTS FOR COVARIATE BALANCE ACROSS SUBSIDISED AND MATCHED CONTROL GROUPS

		Primary y	Employment nt	Resc.&Restr. tr.	Training	R&D
Propensity score	t	0.29	0.45	0.86	0.27	0.31
	P-value	0.773	0.655	0.391	0.790	0.757
lnEmployment$_{t-1}$	t	-0.410	-1.7	-0.12	-0.04	-1.41
	P-value	0.679	0.089	0.907	0.967	0.159
lnRevenue$_{t-1}$	t	-1.42	-0.99	0.14	-0.41	-0.92
	P-value	0.156	0.322	0.887	0.682	0.359
Exporter$_{t-1}$	t	0.1	-1.16	-0.35	0.03	-1.25
	P-value	0.935	0.246	0.726	0.978	0.213
Export share$_{t-1}$	t	-0.68	-0.35	-0.2	0.01	0.19
	P-value	0.494	0.723	0.843	0.992	0.851
VA/Emp.$_{t-1}$	t	-1.43	0.91	0.37	-1.18	-1.49
	P-value	0.153	0.363	0.713	0.237	0.137
Debt/Assets$_{t-1}$	t	-0.39	-1.6	0.52	-0.89	-1.92
	P-value	0.700	0.109	0.605	0.373	0.054
ROA$_{t-1}$	t	-0.62	2.72***	-0.64	0.57	2.14**
	P-value	0.535	0.007	0.522	0.57	0.033
lnAvg. Wage$_{t-1}$	t	-1.53	0.86	1.02	-0.91	-0.17
	P-value	0.126	0.388	0.312	0.361	0.864
For. Ownership$_{t-1}$	t	-0.44	-0.56	-0.58	-1.55	-1.4
	P-value	0.660	0.578	0.562	0.120	0.162
Outward FDI$_{t-1}$	t	-0.81	-0.94	0.35	0.64	0.95
	P-value	0.418	0.345	0.726	0.522	0.342
Num. Grants CUM$_{t-1}$	t	0.64	8.39***	0.27	3.59***	1.24
	P-value	0.520	0.000	0.789	0.000	0.214
Val. Grants CUM$_{t-1}$	t	0.78	0.15	0.88	1.03	1.31
	P-value	0.434	0.882	0.382	0.303	0.189
Age	t	0.72	2.21**	1.15	0.96	0.34
	P-value	0.471	0.027	0.252	0.335	0.734
I(Est. before 1995)	t	0.82	1.2	1.54	1.13	-0.32
	P-value	0.412	0.230	0.128	0.257	0.746

Notes: The test corresponds to the nearest-neighbour matching with additional constraints that treated and control firms belong to the same 2-digit industry and the same year and that we estimate the propensity score for each state aid category and year separately. T-tests are based on a regression of the variable on a treatment indicator and a test for equality of means in the two samples. *** p<0.01, ** p<0.05, * p<0.1

Source: own calculations.

Figure A1: KERNEL DENSITY ESTIMATES OF THE PROPENSITY SCORE BEFORE AND AFTER MATCHING



Source: own calculations.

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