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ABSTRACT

Long-Run Effects of Public-Private Research Joint Ventures: The Case of the Danish Innovation Consortia Support Scheme*

Subsidized research joint ventures (RJVs) between public research institutions and industry have become increasingly popular in Europe and the US. We study the long-run effects of such a support scheme that has been maintained by the Danish government since 1995. To cope with identification problems we apply nearest neighbor caliper matching and conditional difference-in-difference estimation methods. Our main findings are that (i) program participation effects are instant for annual patent applications and last for three years, (ii) employment effects materialize first after one year and (iii) there are no statistically significant effects on value added or labor productivity. We further show that these overall results are primarily driven by firms that were patent active prior to joining the RJV and that there are no statistically significant effect for large firms. Both types of firms are disproportionally represented in the support program we study.

JEL Classification: O31, O38

Keywords: public-private partnership, research joint venture, research and development, research subsidies

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1 Introduction

Governments all over the world try to stimulate industrial research expenditures through research subsidies. The economic rationale behind such efforts is that the social value of R&D, mainly due to the existence of research spillovers, lies below its private return — firms are unable to fully appropriate the benefits from their R&D efforts. This external effect leads to under–investment in R&D from a social point of view and thereby justify governmental intervention (D'Aspremont and Jacquemin 1998; Kaiser 2002a; Kamien et al. 1992; Kamien and Zang 2000; Katz 1986; Leahy and Neary 1997; Spence 1984; Suzumura 1992).

Research subsidies come in two main forms, tax breaks and direct subsidies for specific research projects. The economic efficiency of such schedules in terms of their effects on private R&D has been extensively discussed in a special issue on technology policy in *Research Policy* in 2000 with a positive verdict about tax incentives and with somewhat inconclusive results with respect to research subsidies.

Research subsidies are, however, not the only measure governments use to stimulate innovation. In the mid–1980s policy makers in the EU and the US started to permit Research Joint Ventures (RJVs), where RJV members pool their research resources to generate inventions (Kaiser 2002b). Spillovers are internalized within a joint venture so that private and social returns to R&D are equated. Such cooperations where hitherto deemed anti–competitive. After they had been legalized, RJVs have become increasingly popular (Caloghirou et al. 2003a) and constitute the dominant form of research cooperation today (Hagedoorn 2002).

An additional means of research stimulation that has received much attention by policy makers and economists alike is the technology transfer between public sector research institutions and industry. These may take on the form of formal and informal public–private R&D collaborations as well as of university spinoffs and licensing.[1]

While the literature has so far well studied RJVs, research subsidies and public–private R&D cooperation, comparatively little is known about the effects of subsidized public–private research cooperations where the partners involved in the RJV receive subsidies on their R&D expenditures that accrue within the RJV. These "hybrid" forms of RJV and subsidies have spread substantially both in the US (Vonortas 1999, 2000) and in Europe (Caloghirou et al. 2003a) where e.g. the "Cooperation" program within the 7th EU Framework Programme requires participation of both public and private institutions.

[1] Another relevant but more indirect form of research partnership, technology transfer offices, has been established in the wake of the Bayh–Dole act of 1980. A special issue of *Research Policy* had been devoted to economic analyzes of that change in legislature (Grimaldi et al. 2011).

In this paper we study a particular subsidized public/private RJV program, the "Danish Innovation Consortia" (DIC), and its effects on the performance of the participating industrial firms. The DIC program was started in 1995 with the intention to strengthen the technology transfer between public research institutions and industry. It includes private sector entities and public research institutions. Our data trace the period 1990 to 2007. Within that period, the program covered 80 DICs and included 220 unique firms. The total grant volume in 2002, which is the year the last DIC in our data started, was 766 mio. DKK (about 100 mio. Euros).

A unique feature of our data is that they trace a comparably long time period which enables us to investigate the *long–term* effects of subsidized RJV participation on firm performance. In particular, we study both contemporaneous effects as well as the effect of DIC membership with a five years lag. We account for contemporaneous effects since Hall et al. (1986) demonstrate that R&D effects on patent counts appear to be contemporaneous and we consider lags of up to five years since Peterson (1993), using business survey data for the European EUREKA publicly sponsored joint research program, finds that up to five years lapse until EUREKA effects materialize. Related studies by Bayona–Sáez and García–Marco (2010), Benfratello and Sembenelli (2002) as well as Brandstetter and Sakakibara (2002) have considered two year lags. Bayona–Sáez and García–Marco (2010) study the EUREKA program using dynamic panel data technique and find a positive effect of program participation on return on assets that, however, materializes only one year after project completion. Benfratello and Sembenelli (2002) apply differences in means tests to show that participation in the EUREKA program has positive effects on labor productivity and price cost margins while participation in the less market oriented EU framework program FPST is not significantly related to these success parameters. Brandstetter and Sakakibara (2002) study Japanese research consortia and find that program participation has more positive effects if is geared towards more basic research and that they appear first one year after project completion.

We link the instance of program participants to firm–level data, essentially balance sheet information before and after DIC participation, to control for a wide range of variables that affect program participation choice and/or firm performance. We use multi–dimensional measures of firm performance, namely growth in patent stock (i.e. the number of patent applications) as a "direct" measure — the DICs are geared towards "high quality research" — as well as two more indirect and frequently used measures, namely employment growth and growth in value added (which we deflate to account for inflation).

What performance measure to use is a debated issue in the literature (Caloghirou et al. 2003a). Most studies investigate innovative *inputs* to investigate whether public subsidies or RJVs crowd in or crowd out private R&D. With the exception of Wallsten (2000), who provides evidence for crowing out for research subsidies, the existing literature tends to either find insignificant or positive effects for such support schemes (Almus and Czarnitzki 2003; Girma et al. 2007; González and Pazó 2008; Klette et al. 2000; Lach 2002). Branstetter and Sakakibara (1998), Irwin and Klenow (1996) as well as Kaiser (2002b) find positive effects of RJV participation on private R&D spending.

Existing research has also studied the relationship between subsidization and innovative *outputs* (Archibald and Finifter 1999; Benfratello and Sembellini 2002; Branstetter and Sakakibara 1998; George et al. 2002; Huggins 2001; Kogut 1988; Larédo 1998; Klette and Møen 1999), finding positive or statistically insignificant effects. In this paper we consider the number of patent applications per year as a measure of innovative output. We do not, however, observe any measure of innovative input.

Yet another strand of the literature has studied firm *performance* effect of subsidies and research joint ventures. We follow that strand by considering value added and employment growth as our more indirect performance effects of DIC membership. Profit–related variables have previously been considered by Bayona-Sáez and García–Marco (2010), Berg et al. (1982), Hagedoorn and Schakenrad (1994) as well as Siebert (1996). These studies tend to find positive effects of governmental intervention. Employment growth has been analyzed by Wallsten (2000) who does not find any significant effects.[2]

Program participation is non–random and participation choice may well be correlated with firm performance and innovative output. Since firms are either observed as program participants or non–participants, we are faced with an identification problem. Our attempt to getting around that issue is to apply "conditional differences in differences" (cDID) estimation methods (Heckman et al. 1998, 1999) where we first match participating and and non–participating firms with respect to observed firm–specific characteristics before joining a DIC and then run firm performance regression models on the matched data set (hence the term "conditional difference-in-difference"). The idea behind running parametric regression on the matched treatment/control data is to remove any differences in the observable characteristics between both groups. Such an approach has been previously applied for research subsidization programs by Almus and Czarnitzki (2003) as well as by Brandstetter and Sakakibara (1998). The latter do, however, not apply formal matching models but compare what they term "roughly similar" firms instead. Our approach identifies causal effects of treatment conditional

[2]Other author have analyzed productivity (Benfratello and Sembenelli 2002; Sissoko 2011), R&D efficiency (Link 1998a,b) as well as subjective performance measures (Caloghirou et al. 2003b; Caloghirou and Vonortas 2000).

on observables (i.e. the explanatory variables we do control for) and on time–invariant firm–specific effects and time trends. That is to say, we do not identify causal effects if we omit time–variant variables that affect both selection into treatment and our outcome measures, at least to the extent that these are not highly correlated with the variables we do control for.

Our main results are as follows: in the specifications that allow for contemporaneous effects only we find statistically and economically significant effects of DIC participation on annual patent applications: DIC membership increases the number of annual patents by 0.39 percent, a figure that needs to be related to a mean number of annual patent application of 0.59. This finding is primarily driven by firms that applied for a patent prior to joining a DIC. We do not find any statistically significant contemporaneuos effects of program participation on the other outcome variables we consider, employment and value added.

In the specification where we do allow for both contemporaneous and lagged effects we find that DIC participation not only positively affects patent counts contemporaneously but with lags of one, two and three years as well. The total effect of DIC membership on patent counts after three years is 1.62 and measured with high precision. We again find that the positive effects are mainly due to firms that already innovated prior to joining a DIC. By contrast, we do not document any statistically significant effects for the 25 percent largest firms. The latter finding appears to be particularly relevant since program participation is statistically significantly positive affected by firm size which hence indicates that granting agencies may want to shift attention to smaller firms instead. That issue had been raised by Link and Rees (1990) already.[3] By contrast, our finding that the positive effects on patent counts are mainly driven by firms that patented prior to joining a DIC is policy–reassuring since program participation is positively affected by previous patenting activity as well.

For employment growth as our outcome variable we show that program participation is associated with an increase in the number of employees by 0.03 percent after one year, an effect that is statistically weakly significant. The effect is more than twice as large and measured with much more precision for previously patent–active firms. Moreover, the *total* effect of treatment on employment growth for firms patent active prior to treatment is largest after four years where it is 0.16 percent and statistically significant. Sizeable employment effects hence kick in with substantial delay only which in turn implies that studies that consider shorter time horizons may underestimate the true long–run effects of program participation, an issue that may explain why Wallsten (2000) does not find any statistically significant impact of

[3]See Pavitt (1998) for a general critical appraisal of EU R&D policies.

program participation.

Finally, we do not find any statistically significant results of DIC participation on deflated value added. We have additionally considered labor productivity, defined as the natural logarithm of value added relative to total employment and did not find any statistically significant effects either. This is in accordance with Benfratello and Sembellini's (2002) findings for the FPST program but contrast their results for the EUREKA program regarding labor productivity.

The paper is organized as follows: Section 2 characterizes the DICs, Section 3 outlines our empirical approach, Section 4 describes our data, Section 5 details our empirical specification, Section 6 provides descriptive statistics, Section 7 discusses our estimation results and Section 8 concludes.

2 The Danish Innovation Consortia

The Danish Innovation Consortium program was started in 1995 and is administered by the Danish Agency for Science, Technology and Innovation (DASTI) under the Ministry of Science, Technology and Innovation.

An innovation consortium consists of at least two partners that participate throughout the entire project, at least one research institution and one of the Danish "Approved Technological Service Institutes" (ATS) which are research and technology organizations similar to the Fraunhofer Gesellschaft in Germany, France's Alternative Energies and Atomic Energy Commission (CEA) or SINTEF in Norway.[4] Within the DICs the ATS' assume advisory and coordinating roles. DIC projects have life-spans of between two to five years with a mean and median duration of 3.5 years.

Between two and five firms participate in a given DIC. While DASTI does not reimburse firms' expenses associated with project participation, it finances the expenses incurred by the research and the technological service institutions. Total grants sizes for DICs are between 0.9 mio. and 15 mio. DKK (between 100,000 and two mio. Euros).

To be eligible for funding DASTI requires a set of conditions to be met: the joint project should result in the completion of high–quality research relevant to Danish companies, have generic content and require close collaboration between the consor-

[4]The respective URLs of these institutions are http://www.fraunhofer.de/en/index.jsp, http://www.cea.fr/english_portal and http://www.sintef.no/home/. There exists no direct counterpart in the US. There are, however, similar knowledge transfer functions that are delivered by a number of independent engineering research institutes. A short description of the ATS' is available for download from http://www.teknologiportalen.dk/NR/rdonlyres/C63A9F7F-579D-475D-9D35-BFE66CBC0F49/4160/117397_infofolderUK_21x21_low.pdf

tium parties. Yet, it is not supposed to have the character of product development for individual companies, e.g. it is not contract research. Examples of development projects include improving laser beam welding, e–learning, or fermentation technologies.

The DIC program is, like the FPST program and unlike the EUREKA program (Benfratello and Sembellini 2002), more research than market oriented.

Six firms have participated in more than one consortia. It would be interesting to study differential effects of having joined more than one research consortium but the low number of repeated participations prohibits further investigation. Benfratello and Sembellini (2002) study additivity effects of multiple EUREKA and FPST program participation.

Section 6 presents and discusses descriptive statistics of the firms involved in the DICs and our control group of non–DIC member firms.

3 Empirical approach

The main question this paper asks is: what is the causal effect of DIC membership (e.g. treatment received) on those firms that did join a DIC? In other words, what is the difference between firm i that did receive treatment and the same firm i that did not receive treatment. The fundamental problem is that firm i is only observed in one state, with treatment or without treatment. The counterfactual is not observed.

Constructing a valid counterfactual is the key to the estimation of any treatment effect. There is a growing body of literature that is based on the counterfactual framework that was pioneered by Rubin (1974). Blundell and Costa Dias (2005) as well as Caliendo and Kopeinig (2008) provide excellent review articles. A particularly often used workhorse applied in the program evaluation literature is the econometric matching model, and we shall use such an approach in this paper as well.

The panel structure of our data set allows us to combine matching models and regression models. That way we are able to take into account both observable and unobservable factors — at least to the extent that they are time-invariant and linear — that may drive selection into treatment (and that may be correlated with the outcome variables).

In a first step we match control group firms to the firms that have been a member of a DIC. In a second step, we regress our outcome variables on a set of conditioning variables and treatment indicator variables. We observe both treatment and control observations before and after treatment so we effectively combine propensity score matching with difference-in-difference estimation (i.e. cDID estimation) as discussed by Blundell and Costa Dias (2008, Ch. E).

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.

3.1 Propensity score matching

Basics

The basic idea behind matching methods is to remove all differences between participating and non–participating by finding “twins” (in terms of observed characteristics) of the participating firms in the universe of non–participants. We then go a step further by removing all differences that may remain by parametric performance regressions in first differences. It is important to note that we do not need to remove all observable differences in the observable characteristics by matching already since we apply parametric regressions on the match treatment/control data (Heckman et al. 1998). These regressions control for both firm-specific unobserved factors (“fixed effects”) as well as for firm–specific time trends (see Subsection 2.2). We hence attempt to identifying causal effects under the weakest possible assumptions and we even achieve identification in the absence of perfect equality of the characteristics of treatment and control observations. Conditional on our choice of control variables and on treatment and control observations not following different time trends we obtain an unbiased estimator of the program participation effect (Blundell and Costa Dias 2009; Caliendo and Kopeinig 2008).

There are two basic types of matching methods, nearest neighbor matching which minimizes the distances between the observed characteristics of two different observations based on some distance measure and propensity score matching. We refer to the latter method since nearest neighbor matching can quickly lead to problems with the “curse of dimensionality”, i.e., difficulties associated with finding control firms which are highly similar in more than just a few dimensions.

Propensity score matching condensates the distance in observed characteristics between two different firms in one measure, the propensity score.[5] We estimate the propensity score based on a binary probit model estimation for the probability of receiving treatment in a particular year $t + 1$, i.e. we match firms according to their observable characteristics prior to joining a DIC. If we matched firms on their observable characteristics contemporaneously, these characteristics may have been affected by DIC membership already which in turn leads to a violation of the “unconfounded-

[5] Rosenbaum and Rubin (1983) suggest using such “balancing scores”. They show that if potential outcomes are independent of treatment conditional on observed characteristics (which we assume), they are also independent of treatment conditional on a balancing score.

ness" or "conditional independence" assumption underlying matching methods (Blundell and Costa Dias 2009; Caliendo and Kopeinig 2008).

The validity of the conditional independence assumption to a large extent depends on the way the outcome variables in the parametric regression models to follow are defined. In our case, we consider changes in outcomes instead of levels and control for firm-specific growth trends in the regressions and for time–invariant firm–specific effects, which considerably reduces the likelihood of the conditional independence assumption to be invalid.

Our approach to minimize any bias arising from non–random assignment into treatment is to assume that, given a set of observable characteristics — which is affected by treatment — potential outcomes are independent of the assignment to treatment. This assumes that selection is based on observable characteristics only and that all variables that influence treatment assignment and potential outcomes are simultaneously part of our conditioning variables.

While we do control for a large set of relevant variables that are known to affect both firm performance and selection, we cannot formally test if the conditional independence assumption is indeed satisfied. We do, however, formally test whether treatment and control observations no longer differ significantly with respect to observable characteristics after matching. This is known as the "balancing property".

An additional condition for our identification strategy to hold is the "common support" requirement. It rules out that the probability of treatment is perfectly predicted by the set of conditioning variables. It makes sure that individuals with the same observed characteristics have a positive probability of receiving both treatment and non–treatment (Heckman et al. 1999). Anticipating our estimation results we note that all our matched control observations are on the common support.

There exists a multitude of econometric matching models of which Blundell and Costa Dias (2009) as well as Caliendo and Kopeinig (2008) provide reviews. These methods have in common that they trade bias (which is an increasing function of the dissimilarities of treatments and controls) against precision (which is an increasing function of the size of the control group). Our goal is to minimize bias since the results of the matching procedure are not of original interest to us. We hence resort to nearest neighbor caliper matching with a single neighbor and replacement as suggested by Caliendo and Kopeinig (2008).

3.2 Conditional differences–in–differences estimation

With a sample of treatment and observationally highly similar control observations at hand we run multivariate regressions on our three outcome variables patent stock, the number of employees and value added.

Employment and value added

We assume that the data generating process for our outcome variables number of employees (Y_{eit}) and value added ($Y_{\Pi it}$), is log–linear:

$$Y_{kit} = exp(\boldsymbol{X_{kit}\beta_k} + \mu_{ki} + \alpha_{ki}t + \gamma_k D_{it} + \epsilon_{kit}), \tag{1}$$

where $k = e, \Pi$ (employment and value added respectively) and μ_i denotes a time-invariant firm-specific error component, α_i denotes firm-specific time trends, D_{it} denotes a dummy variable for firm i having received treatment at time t and ϵ_{it} denotes an idiosyncratic error term.

We take logs and first difference Equation (1) to remove the time-invariant firm-specific error component, μ_i, and to be able to estimate the parameters corresponding to the firm–specific time trends, α_i, leads to the following estimating equation:

$$\Delta ln(Y_{kit}) = (\boldsymbol{X_{kit}} - \boldsymbol{X_{kit-1}})\boldsymbol{\beta_e} + \alpha_{ki} + \gamma_k(D_{it} - D_{it-1}) + \epsilon_{kit} - \epsilon_{kit-1}. \tag{2}$$

We estimate Equation (2) by linear fixed effects. These fixed effects represent the coefficient of firm–specific time trends, α_i.[6] The coefficient of interest is γ which is to be interpreted as the percentage change in employment and value added caused by DIC membership respectively.

Taking logs of Equation (1) generates missing values for value added as dependent variable since value added can become negative. This is, however, the case for less than one percent of the observations only which is why we ignore this issue.

Patent stock

Patent stock as our third outcome variable does not lend itself to a log–linear specification since the patent stock of most firms is 0. We assume a simple linear functional form and take first differences instead:

$$\Delta Y_{pit} = (\boldsymbol{X_{pit}} - \boldsymbol{X_{it-1}})\boldsymbol{\beta_p} + \alpha_{pi} + \gamma_p(D_{it} - D_{it-1}) + \epsilon_{pit} - \epsilon_{pit-1}. \tag{3}$$

Our dependent variable is the number of patent applications by firm i at time t (the difference in patent stocks between t and $t-1$). It is a count variable and we hence estimate Equation (3) using count data models. The term α_{pi} again represents the coefficients related to firm–specific time trends. The coefficient of interest again is γ which measures the percentage change in the number of patents due to DIC membership.

To estimate that equation, we follow Blundell et al. (1995, 1999, 2002) and take into account both state dependence and time–invariant firm–specific effects. State

[6] Note that this actually is a Difference–in–Difference–in–Difference estimation since the time–invariant error component (after first differencing), α_{ei}, could be removed by taking first differences again.

dependence has proven to be an important feature of innovative activity (Blundell et al. 1995, 1999, 2002; Ejsing et al. 2011; Kaiser et al. 2011; Peters 2009).

The idea behind the Blundell et al. (1995, 1999, 2002) model is to approximate the time–invariant firm–specific effects (e.g. the "correlated effects"), by the number of "pre–sample" patents, e.g. in our case a firms' patent stock prior to 1990, the year when our firm–level data begins (while our patent data go back until 1978). We operationalize that variable following Blundell et al. (1995, 1999) by the natural logarithm of firm i's patent stock in 1990. In case the patent stock is zero, we replace the corresponding natural logarithm by 0. We additionally include a dummy variable for firm i' patent stock in 1990 being positive to account for the nonlinearity we introduce by our substitution. Our correction for the clustering at 0 counts should perform better than "Zero Inflation" (Mullahy 1986) models since it is based on much less restrictive assumptions (Kaiser et al. 2011; Staub and Winkelmann 2009). Our measure of state dependence is lagged patent stock (which is a linear function of lagged patent applications).

4 Data

Information on the DICs was collected by DASTI and was made available for research through the Centre for Economic and Business Research at Copenhagen Business School. These data were merged with balance sheet information supplied by Experian, a global provider of business intelligence. These data have been previously used by i.a. Nielsen et al. (2007). A Pan–european counterpart of the Experian data has been used by Hernán et al. (2003) to study the determinants of EUREKA program participation.

We complement our DIC/KOB data by information on all patent applications to the European Patent Office (EPO) that were filed for between 1978 and 2006 by at least one applicant with Danish residence. We retrieved that data from EPO's "PATSTAT" database.[7] We consider patent applications up to and including 2004 in our analysis, since the database for the years following this date is not complete. It includes 12,873 patent applications in total. The patent data have been previously been used by Ejsing et al. (2011) as well as Kaiser et al. (2011).

Patent applications are used rather than patent grants because the average grant time at the EPO of four to five years for Danish applications (Kaiser and Schneider, 2005) implies that a substantial number of patents applied for during the time period considered for estimation (1990–2004) would be lost if patent grants were used

[7]For information about this data set, refer to http://www.epo.org/patents/patent-information/raw-data/test/product-14-24.html.

instead.[8] The "time stamp" of the patent applications is the "priority date", the date on which the invention was first filed for patent protection at the EPO or any national patent office.

Before matching we remove observations belonging to regions and three digits NACE Rev. I sectors that never received treatment. We also discard firms which do not have employees, have zero value added and for which information on value added or employees is missing.

Value added as our only monetary variable is deflated by the year 2000 consumer price index.

Our combined data set that we use for our propensity score estimations contains 193,037 observations on 27,798 unique firms. The final data set we use for our cDID estimations includes 4,549 observations on 217 unique DIC member firms and 173 unique control group firms. The discrepancy between the unique number of treatment and control group firms is due to us matching with replacement (which increases inefficiency but decreases bias).

5 Empirical specification

5.1 Conditioning variables for propensity score matching

The conditioning variables we take into account for our propensity score matching are standard in the literature: firm size as measured by the natural logarithm of the total number of employees (Fritsch and Lukas 2001; Gonzáles and Pazó 2008, Hernán et al. 2003; Link and Rees 1990; Tether 2002; Wallsten 2000) and its square, firm age (Gonzáles and Pazó 2008) as well as the Hirshman-Herfindahl concentration index to account for competitive pressure (Hernán et al. 2003).[9]

Like Hernán et al. (2003) we control for the speed at which innovation diffuses to competitors by using the survey data of Mansfield (1985) at the two and (where available) three digit NACE sectoral classification level and the self–reported effectiveness of patent protection taken from Levin et al. (1987), again at the two and three digit NACE sectoral classification level. Both variables are measures of R&D spillovers which have been shown to affect RJV formation (Belderbos et al. 2004; Caloghirou et al. 2003a; Cassiman and Veugelers 2002; Hernán et al. 2003; Kaiser 2002b).

Since granting authorities are likely to base access to subsidized research on prior innovation performance we also include the lagged number of patents in our treatment

[8] There exists a reporting lag between the date of application and the date on which the application is published in the EPO database. This implies that not all patents applied for after 2004 are registered in the database at the time of data collection. We do not include such patents in order to avoid biases.

[9] We calculate that index as the sum of squared sales shares at the three digit NACE level.

regression. We operationalize that variable as its natural logarithm and follow Kaiser et al. (2011) and replace the missing values generated by the logarithmization by 0 and additionally include a dummy variable that is coded 1 if the corresponding firms has a positive patent stock (and 0 otherwise). This is the same procedure as for the correlated effects proxy variables discussed in Subsection 3.2. We finally include the usual firm heterogeneity control variables like sector affiliation, regional affiliation, legal form dummies as well as year dummies.

We do control for all variables that may affect RJV formation and/or firm performance and that are available in our data. We do, however, not control for many other variables that may prove to be important and which are reviewed by Caloghouri et al. (2003a). We for example do not control for previous RJV experience (Hernán et al. 2003; Vonortas 1997), absorptive capacity (Cassiman and Veugelers 2002; Kaiser 2002b; Kogut 1991); access to complementary knowledge (Cohen et al. 1997; Caloghouri et al. 2003a; Katsoulacos and Ulph 1988); sectoral R&D (Hernán et al. 2003) and strategic motives to join a RJV (Link 1990; Link and Bauer 1989; Link and Zmund 1984; Martin 1996; Vonortas 1997). These unobserved motives may well affect DIC participation and our outcome measures. Note, however, that these factors are accounted for — at least to the extent that they are time–invariant — in our "second stage" difference–in–difference estimation.

5.2 Conditioning variables for conditional Difference–in–difference estimation

The conditioning variables for our cDID regressions differ between the performance variables we consider. The Hirshman-Herfindahl index of sales concentration, a set of year dummies and treatment dummies are considered in all specifications. Patent effectiveness, time until an innovation is diffused, regional affiliation and sectoral affiliation are time–invariant and drop out from our first difference estimations.

Our specification for patent stock includes the proxy variables for correlated effects — the dummy variable for having applied for a patent prior to 1990 and the log number of patents prior to 1990 — as well as our dummy variable for state dependence as explained above and the natural logarithm of the number of employees. The latter variable is also included in our model for value added.

5.3 Short–run and long–effects

We have so far only considered contemporaneous effects of DIC membership. We do also, however, allow for DIC effects to influence firm performance with lags. Our extended treatment term which accounts for five lags is $\sum_{l=0}^{5} \gamma_l (D_{it-l} - D_{it-l-1})$ (which we substitute for $\gamma(D_{it} - D_{it-1})$ in Equation (2)). The total effect of treatment at time f hence is $\sum_{l=0}^{f} \gamma_l$.

6 Descriptive statistics

We start our empirical analysis by presenting descriptive statistics of our dependent variables in Table 1. The table differentiates between treatment group and control group observations. The table and our estimations discard observations with the one percent smallest and the 1 percent largest values in the outcomes variables employment and value added to avoid our results being driven by outliers. Table 1 only refers to the key variables involved in our estimations. Appendix A displays descriptive statistics of all variables involved in our estimations. It also distinguishes between large and innovative firms before treatment since we estimate treatment effects separately for large and innovative firms. Appendix B displays firm size distributions by sector. Both appendices and Table 1 refer to our data before propensity score matching.

The average firm in our data employs 13.5 workers, possesses a discounted patent stock of 0.008 patents (we discount the stock of patents by 30 percent per year following Blundell et al. 1995), applies for 0.006 patents per year and makes average annual value added of 57,600 DKK per year. It is twelve years old, incorporated with a Herfindahl–index of sales concentration of 0.068, i.e. it operates in unconcentrated markets.

DIC and non–DIC members differ substantially with respect to the number of patents, employment and value added. DIC member firms are substantially larger (576 employees on average compared to 13 employees for non–DIC firms), patent more often (patent stock of 1.29 compared to 0.007) and make higher annual value added (531,906 DKK compared to 57,010 DKK). Looking at these figures alone would suggest that DIC treatment has positive effects on the participating firms. Such an assessment would ignore, however, the fundamental heterogeneity among firms in a DIC and outside a DIC. Our econometric approach is to remove all observable heterogeneity to assess the causal effect of DIC participation.

Turning to our explanatory variables we note that DIC member firms are older, are more likely to be stock listed, are less likely to be privately held companies and operate in much less concentrated markets. There are no significant differences with respect to our spillover variables (which only vary across sectors).

7 Estimation results

7.1 Propensity score estimation

Probit estimation results

Our econometric analysis starts with the estimation of a binary probit model for receiving treatment at $t+1$, i.e., we condition the probability of receiving treatment conditional on variables observed at t.

Table 2 displays our estimation results. It shows that firms with prior patenting activity and larger firms have a higher chance of participating in a DIC than small and non–patenting firms. Indeed, the effect of firms size seems U–shaped, the minimum is, however, reached at a number of employees of 0.18 which is outside the range of our data. There are also statistically jointly significant differences in legal form, sector and regional affiliation as well as in the year dummy variables.

The specification displayed in Table 2 did not balance well on patent stock. To improve our match on patent stock we also include squared patent stock in our final probit specification (not shown in the results table).

Match quality

Given that we do not condition individually on all variables in our set of conditioning variables, but on the propensity score instead, we need to assess if our matching approach is able to balance the distribution of treatment and control individuals, i.e., if the match quality is satisfactory.

We follow a suggestion by Rosenbaum and Rubin (1985) and use "standardized biases", which simply constitute the differences between treatment and control group observations in the means of the conditioning variables before and after matching, weighted by their standard deviations. Appendix C displays standardized biases before and after matching for all explanatory variable we consider. It shows that none of the differences in the observed variables is statistically significant. Indeed, post matching differences are very small and p–values post matching are all well above 0.2.

We would like to stress that we match particularly well on the number of employees and patent stock. The respective p–values for differences in the treatment and the control group are 0.711 and 0.925 (see Appendix C). This is important to note since we shall conduct differential regressions for large firms and firms with patenting activity prior to having received treatment.

As an additional informal matching quality check, Sianesi (2004) suggests re-estimating the propensity score on the matched sample, i.e., only including the treatment individuals and the matched control group observations, and compare the pseudo R^2's before and after matching. There should not exist a significant difference in the distribution of covariates after matching and the pseudo R^2's should therefore be close to 0. In addition, tests for joint significance of the covariates should reject joint significance after matching. The pseudo R^2 after matching is 0.032 and tests for joint significance easily reject the ability of the covariates to explain selection after matching as well (p–value .98). We hence conclude that our control group is satisfactorily matched to our treatment group based on observed firm characteristics.

Such a satisfactory match is not even necessary for an unbiased estimation of treatment effects as we condition on the same set of explanatory variables in the regression models as well. Moreover, our difference–in–difference specification also accounts for time–invariant firm–specific effects as well as for firm–specific time trends.

7.2 Difference–in–difference estimation

With the matched treatment/control group data at hand we now run count data models for patent applications as our outcome variable and linear fixed effects models for employment and value added as outcome variables.

We first discuss our results for the specification that does not allow for lagged effects and then turn to our specification that considers lag lengths of between one and five.

Contemporaneous effects

Table 3 displays our estimation results for the contemporaneous effects of treatment, e.g. we do not consider lagged effects here. We find that DIC members apply for 0.387 percent more patents than non–DIC members, controlled for observable and unobservable differences between firms. There are no statistically significant differences between innovative firms (firms with at least one patent prior to treatment) and non–innovative firms. We do find, however, that the treatment effect is statistically insignificant for large firms, e.g. the 25 percent largest firms in our data. That indicates that DIC membership is most effective for smaller firms firms. This finding may of course be partly driven by the greater heterogeneity among large firms in our data.

That DIC participation has a contemporaneous effect on patent counts may seem surprising since patents are unlikely generated instantaneously despite similar findings in an early study by Hall et al. (1986). We speculate that research that leads to a patent application shortly after a DIC was joined has been "in the pipeline" already, possibly by the public research partner with its stronger focus on publication rather than on patenting. Research cooperation with industrial partners within the DIC may have triggered the eventual patent application for the invention.

For the more "indirect" performance measures employment and value added we do not find any statistically significant contemporaneous treatment effects, neither for innovative firms nor for large firms. We also ran labor productivity regressions that did not indicate any statistically significant effects of program participation. We do not display these results for brevity. These results are consistent with Benfratello and Sembellini for the FPST program but contrast Bayona–Sáez and Garciá–Marco (2010) who find that EUREKA program participation positively affects return on investment one year after the program was joined.

Lagged effects

We expect treatment effects to materialize with some lag after joining a DIC, especially when it comes to our more "indirect" performance measures employment and value added. We therefore additionally consider a specification where we allow for five lags as discussed in Subsection 5.3. Table 4 displays our corresponding estimation results.

We again find statistically and economically significant results of treatment on the number of patent applications. The contemporaneous effect is 0.44 percent and hence somewhat larger than when lagged effects are not considered. The coefficient estimates for the first to third lag are statistically significant as well with point estimates between 0.35 and 0.44. The fourth coefficient is weakly significant and negative which we attribute to noise, in particular when noticing that the coefficient is estimated with little precision and that the fifth lag is statistically insignificant and again positive. Table 5.3 suggests that these positive effects are primarily due to firms that patented prior to joining a DIC — the coefficient estimates are similar to the ones obtained for the gross sample and we attribute their lower significance to the substantial reduction in the number of observations.

We do not find statistically significant results for large firms which reinforces our previous result that DIC membership is more efficient for smaller firms.

While we did not find any statistically significant effects of treatment on contemporaneous employment we do, however, document statistically significant and positive employment effects that materialize one year after having joined a DIC. The related coefficient translates into a percentage change in employment of 0.025 percent. This effect is larger for innovative firms (0.057 percent). For innovative firms we also find statistically significant and positive effects at lag four. We do not establish a statistically significant mapping between large or small firms with respect to employment growth except for the fifth lag which is negative but marginally significant only.

In addition, we do not find statistically significant effects of DIC participation on value added for either all firms, innovative firms or large firms. One exception is a statistically marginally significant point estimate of -0.056 percent at lag five for the overall model.

Long–run total effects

The specifications in Table 4 allow us to calculate the *total* effects of DIC membership after one, two, three etc. years as explained in Subsection 5.3. We display the results of such a calculation in Table 5. It shows that the number of patent applications increases by 1.615 percent in total due to DIC participation three years after a DIC had been joined, an effect that is statistically significant at the four percent marginal significance level. The total effect is with 1.337 percent somewhat smaller for innovative firms prior to treatment. For large firms we do not find any statistically significant total effects.

We do not provide evidence for any statistically significant effects of treatment on employment either, at least if firms are considered overall. This is different for innovative firms where we estimate a statistically weakly significant effect of 0.13 percent after five years. After four years we find an effect of 0.156 percent, statistically significant at the two percent marginal significance level. Large firms again appear not to be affected by treatment in terms of employment.

Neither do we report any statistically significant total effects of DIC participation on value added (or labor productivity).

8 Conclusion

While there exists a substantial body of literature on the effects of either research subsidies, research joint ventures or public/private partnerships on innovative in– and outputs, much less is known about "hybrid" forms of research support schemes that combine all three innovation policy measures such as the Danish Innovation Consortia (DIC) which are in the focus of our study. Until 2002, the program covered 80 DICs and included 274 unique firms. Total grant size was around 766 mio. DKK (about 100 mio. Euros). Each DIC consisted of at least one public research partner and one industrial partner. Government reimbursed the public research partner for the expenses it incurred to conduct the research.

Our data identify 220 of the 274 of the participating industrial partners and we are able to merge this information to balance sheet data for the years 1990 to 2007. We hence have a comparatively long panel data set at our disposal that we use to (i) conduct conditional difference-in- difference analysis to generate estimates of causal effects of treatment and to (ii) consider lagged effects of past DIC participation on firm performance.

To solve the fundamental identification problem inherent in such analyzes we first match treatment firms (DIC members) and control firms (non DIC members) based on their observed characteristics. We then estimate performance equations in first differences, thereby effectively removing any time–invariant firm characteristics. We consider three different types of performance variables, patent counts as a very direct measure of performance as well as employment and value added growth as more indirect measures.

Given the time series dimension of our data we are able to account for lagged DIC participation effects. We allow participation effects to materialize for up to five years after a DIC was joined. The average duration of a DIC is 3.5 years. We also distinguish between "innovative" firms — firm that applied for at least one patent one year *prior* to DIC participation — and "large" firms, firms that belonged to the 25 percent largest firms prior to treatment.

Our main findings are as follows: DIC participation has a statistically and economically significant effect on the number of patent applications per year. The effects appear both instantaneously and with lags of up to three years. The total effect of DIC participation after three years is 0.439 percent (which needs to be compared to an average number of patent applications per year of 0.586). That effect is primarily driven by firms that innovated already before joining a DIC. By contrast, firms that did not innovate before do not seem to be associated with gains from DIC participation. Likewise, we do not find statistically significant effects of DIC participation on patenting for large firms. We also show that previous patenting enhances the probability of DIC participation and demonstrate that firm size and DIC participation are positively related. The latter result calls into question governments' selection of firms into treatment while the former finding reassures it.

We do not find contemporaneous effects of program participation on employment but show that DIC members' employment grows 0.025 percent faster in the year after a DIC was joined compared to non–participating firms. For innovative firms the corresponding effect is more than twice as large. Employment effects are largest for innovative firms four year after a DIC is joined. The total DIC effect for those firms is 0.156 percent. We do not find any statistically significant effects on employment for large firms.

Finally, our estimates do not suggest any statistically significant relationship between DIC participation and value added.

Our results hence indicate that subsidized public-private research partnership has positive effects on direct outcome measures such as patent counts. It also impacts employment growth positively. The latter effect is, however, more pertinent for firms that were innovative already prior to joining a DIC. Governments may hence want to focus support to already innovative firms when it comes to subsidy schemes of the type we study. By contrast, we not find evidence for DIC participation to affect large firms in a statistical or economically significant way. Future work may want to elaborate further on this finding since governmental support often — and indeed in the present case — depends positively on firm size.

Table 1: Descriptive statistics

	All firms		Non–DIC member firms		DIC member firms	
	Mean	**Sd. Dev.**	**Mean**	**Sd. Dev.**	**Mean**	**Sd. Dev.**
Outcome variables						
Patent stock	0.0080	0.5125	0.0066	0.4293	1.2922	8.2788
Annual patent applications	0.0056	0.3499	0.0046	0.3044	0.9078	5.0845
# employees	13.5	130.0	12.8	119.9	576.1	1395.4
Profits (in 1,000)	57.6	12,300.0	57.0	12,300.0	531.9	1,891.6
Explanatory variables						
Patent stock$_{t-1}$	0.0081	0.5975	0.0067	0.4516	1.2812	11.6333
Dummy for patent at $t-1$	0.0074	0.0857	0.0071	0.0842	0.2350	0.4250
Firm age	12.9	15.3	12.9	15.2	35.7	36.7
Stock listed	0.4	0.5	0.4	0.5	0.9	0.3
Private company	0.6	0.5	0.6	0.5	0.1	0.2
Time until leakage	34.9	1.4	34.9	1.4	35.1	3.5
Patent effectiveness	4.5	0.1	4.5	0.1	4.5	0.4
Sales concentration	0.0681	0.1127	0.0679	0.1120	0.3027	0.2859
# obs.	193,037		192,820		217	

Table 1 displays descriptive statistics of our explanatory variables (before matching). Additional descriptive statistics are shown in Appendix A and Appendix B.

Table 2: Probit estimation results — probability of DIC membership

	Coeff.	***p*–val.**
ln(patent stock)$_{t-1}$	0.041	0.017
Dummy for patent at $t-1$	0.848	0.000
Firm age	0.001	0.181
Stock listed	0.157	0.295
Private company	-0.376	0.028
Time until leakage	0.003	0.927
Patent effectiveness	-0.096	0.802
ln(# empl.)	-0.012	0.819
ln(# empl.)^2	0.035	0.000
Sales concentration	0.832	0.001
Tests for joint significance		
Past patenting	78.93	0.000
Legal form	30.39	0.000
Spillovers	0.51	0.776
Employment	282.98	0.000
Sector	52.95	0.000
Region	19.79	0.019
Year	54.31	0.000
Pseudo R^2	0.364	

Table 2 displays Probit estimation results for the probability to be a member of an DIC . The specification also contains a set of legal form dummies, region dummies, and year dummies. It involves 193,037 observations on 27,798 unique firms.

Table 3: cDID estimation results: contemporaneous effects only

	All firms		**Innovative firms**		**Large firms**	
	Coeff.	**p–val.**	**Coeff.**	**p–val.**	**Coeff.**	**p–val.**
Dependent variable: patent stock						
Dummy patent application $t-1$	0.006	0.031	0.007	0.035	0.003	0.353
ln(# empl.)	0.280	0.002	0.255	0.002	0.174	0.214
Sales concentration	-1.805	0.020	-1.873	0.033	-0.162	0.850
Treatment	0.387	0.052	0.383	0.073	0.099	0.716
Tests for joint significance						
Correlated effects	120.38	0.00	25.16	0.00	67.58	0.00
Year dummies	4.61	0.80	5.12	0.74	18.06	0.02
# obs.	3,776		989		1,117	
# firms	390		116		126	
Dependent variable: ln(employment)						
Dummy patent application $t-1$	0.003	0.361	0.003	0.375	0.003	0.478
ln(# empl.)	0.078	0.005	0.087	0.006	0.059	0.153
Sales concentration	0.140	0.011	0.147	0.216	0.400	0.001
Treatment	0.000	0.991	0.023	0.406	-0.019	0.381
Tests for joint significance						
Fixed effects	1.82	0.00	2.30	0.00	2.16	0.00
Patent stock variables	4.05	0.02	3.81	0.02	1.04	0.35
Year dummies	8.25	0.00	3.09	0.00	5.61	0.00
# obs.	4,549		1,218		1,363	
# firms	387		115		125	
Dependent variable: ln(gross profits)						
Dummy patent application $t-1$	0.004	0.934	0.040	0.420	0.000	0.998
ln(# empl.)	0.165	0.000	0.114	0.000	0.097	0.000
Sales concentration	0.002	0.983	0.024	0.900	0.179	0.324
Treatment	0.009	0.693	-0.027	0.530	-0.015	0.648
Tests for joint significance						
Fixed effects	0.98	0.61	1.16	0.13	1.41	0.00
Patent stock variables	0.68	0.51	1.03	0.36	0.42	0.66
Year dummies	4.57	0.00	2.93	0.00	2.58	0.01
# obs.	4,053		1,100		1,229	
# firms	371		110		121	

Table 3 displays conditional difference–in–difference estimation for the effects of treatment on the three outcome variables number of patent applications per year, the natural logarithm of employment and the natural logarithm of gross profit. The estimated treatment effects are to be interpreted as percentage changes in the outcome variable due to treatment. The regressions for employment and gross profits are estimated by linear fixed effects, the patent applications equation is estimated by negative binomial regressions. All estimations are in first differences. We discard the one percent observations with lowest and highest growth in gross profits and employment growth respectively. Our patent count estimations consist of fewer observations since our patent data ends in 2004. The tests for joint significance of the "correlated effects" variables correspond to the correlated effects proxy and zero inflation variables suggested by Blundell et al. (1995). "Innovative firms" are those with a positive patent stock the year before treatment. "Large firms" are firms that belonged to the 25 percent largest ones the year before treatment.

Table 4: cDiD estimation results: lagged effects

	All firms		Innovative firms		Large firms	
	Coeff.	**p–val.**	**Coeff.**	**p–val.**	**Coeff.**	**p–val.**
Dependent variable: patent stock						
Patent stock $t-1$	0.009	0.002	0.009	0.004	0.004	0.307
ln(# empl.)	0.306	0.001	0.275	0.001	0.188	0.168
Sales concentration	-2.080	0.010	-2.063	0.026	0.091	0.922
Treatment$_t$	0.440	0.035	0.420	0.054	0.196	0.520
Treatment$_t - 1$	0.354	0.046	0.268	0.095	0.203	0.322
Treatment$_t - 2$	0.383	0.038	0.259	0.159	-0.080	0.652
Treatment$_t - 3$	0.439	0.037	0.389	0.092	0.385	0.104
Treatment$_t - 4$	-0.779	0.056	-0.657	0.079	-0.227	0.573
Treatment$_t - 5$	0.253	0.406	0.247	0.400	0.328	0.420
Tests for joint significance						
Treatment dummies	10.82	0.09	8.11	0.23	5.93	0.43
Correlated effects	130.33	0.00	31.00	0.00	65.52	0.00
Year dummies	4.16	0.84	4.28	0.83	8.08	0.43
Dependent variable: ln(employment)						
ln(patent stock)$_{t-1}$	0.003	0.354	0.003	0.320	0.004	0.449
Dummy patent stock $t-1>0$	0.074	0.007	0.077	0.016	0.059	0.157
Sales concentration	0.140	0.011	0.156	0.191	0.396	0.001
Treatment$_t$	0.002	0.900	0.033	0.256	-0.032	0.163
Treatment$_t - 1$	0.025	0.067	0.057	0.023	-0.011	0.596
Treatment$_t - 2$	0.002	0.858	0.002	0.947	0.021	0.291
Treatment$_t - 3$	-0.006	0.674	0.005	0.861	-0.003	0.876
Treatment$_t - 4$	0.008	0.616	0.059	0.041	-0.022	0.322
Treatment$_t - 5$	-0.002	0.895	-0.017	0.610	-0.045	0.088
Tests for joint significance						
Treatment dummies	0.68	0.67	1.72	0.11	0.98	0.44
Fixed effects	1.80	0.00	2.26	0.00	2.17	0.00
Patent stock variables	3.60	0.03	2.93	0.05	1.03	0.36
Year dummies	7.73	0.00	2.76	0.01	5.40	0.00
Dependent variable: ln(gross profits)						
ln(patent stock)$_{t-1}$	0.006	0.274	0.008	0.157	0.006	0.404
Dummy patent stock $t-1>0$	0.007	0.880	0.039	0.439	-0.002	0.978
ln(employment)	0.165	0.000	0.112	0.000	0.096	0.000
Sales concentration	-0.006	0.951	0.015	0.938	0.168	0.357
Treatment$_t$	-0.004	0.853	-0.040	0.392	-0.024	0.497
Treatment$_t - 1$	-0.019	0.390	-0.009	0.819	-0.015	0.640
Treatment$_t - 2$	0.009	0.671	0.021	0.627	0.034	0.290
Treatment$_t - 3$	-0.028	0.210	-0.009	0.833	-0.019	0.555
Treatment$_t - 4$	-0.006	0.825	0.025	0.598	-0.004	0.900
Treatment$_t - 5$	-0.056	0.051	-0.074	0.165	-0.036	0.382
Tests for joint significance						
Treatment dummies	0.98	0.44	0.53	0.79	0.45	0.84
Fixed effects	0.98	0.59	1.16	0.14	1.41	0.00
Patent stock variables	0.63	0.53	1.03	0.36	0.41	0.67
Year dummies	4.16	0.00	2.80	0.00	2.46	0.01

Table 4 displays conditional difference–in–difference estimation for the effects of treatment on the three outcome variables number of patent applications per year, the natural logarithm of employment and the natural logarithm of gross profit accounting for five lags of the treatment variable. All estimations are in first differences. The specifications are otherwise identical with those shown in Table 3.

Table 5: Total long–run effects of DIC membership

Lag length	All firms Effect	All firms p–val.	Innovative firms Coeff.	Innovative firms p–val.	Large firms Coeff.	Large firms p–val.
Dependent variable: patent stock						
0	0.440	0.035	0.420	0.054	0.196	0.520
1	0.794	0.024	0.688	0.041	0.399	0.388
2	1.176	0.011	0.947	0.029	0.319	0.524
3	1.615	0.004	1.337	0.015	0.704	0.223
4	0.836	0.204	0.679	0.264	0.477	0.450
5	1.090	0.206	0.926	0.237	0.806	0.378
Dependent variable: ln(employment)						
0	0.002	0.900	0.033	0.256	-0.032	0.163
1	0.027	0.205	0.090	0.025	-0.042	0.184
2	0.029	0.260	0.092	0.063	-0.021	0.585
3	0.023	0.441	0.097	0.095	-0.024	0.589
4	0.031	0.395	0.156	0.025	-0.047	0.388
5	0.028	0.526	0.139	0.101	-0.091	0.168
Dependent variable: ln(gross profits)						
0	-0.004	0.853	-0.040	0.392	-0.024	0.497
1	-0.024	0.497	-0.049	0.450	-0.039	0.438
2	-0.014	0.738	-0.028	0.726	-0.005	0.939
3	-0.042	0.391	-0.037	0.693	-0.024	0.741
4	-0.048	0.419	-0.012	0.914	-0.028	0.741
5	-0.104	0.154	-0.087	0.525	-0.065	0.539

Table 5 "total" long–run effects of DIC participation on our outcome variables. The figures are to be interpreted as percentage change in the outcome variables after 0, 1, 2, 3, 4 and 5 years after a DIC was joined. The calculation of these effects is based on results Table 4. The derivation of the long–run total effects is shown in Subsection 5.3.

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Appendix A: descriptive statistics of all variables involved in the estimations

All firms

	All		Untreated		Treated	
Copenhagen	0.008	0.512	0.007	0.429	1.292	8.279
Number of patents per year	0.006	0.350	0.005	0.304	0.908	5.085
Number of employees	13.452	129.967	12.819	119.886	576.111	1,395.354
Value added	57,599	12,300,000	57,010	12,300,000	531,906	1,891,617
Patent stock at *t-1*	0.008	0.597	0.007	0.452	1.281	11.633
Positive patent stock at *t-1*	0.007	0.086	0.007	0.084	0.235	0.425
Firm age	12.918	15.310	12.892	15.250	35.687	36.690
Stock listed	0.365	0.482	0.365	0.481	0.908	0.290
Private company	0.608	0.488	0.609	0.488	0.055	0.229
Time until leakage	34.915	1.377	34.915	1.372	35.060	3.541
Patent effectiveness	4.549	0.144	4.549	0.144	4.477	0.363
Sales concentration	0.068	0.113	0.068	0.112	0.303	0.286
Manufacturing I	0.090	0.286	0.090	0.286	0.060	0.238
Manufacturing II	0.047	0.212	0.047	0.211	0.290	0.455
Manufacturing III	0.017	0.128	0.017	0.128	0.157	0.364
Electricity, gas and water supply; Construction	0.057	0.232	0.057	0.233	0.046	0.210
Wholesale and retail trade; Repair; Hotels and restaurants	0.115	0.318	0.115	0.318	0.147	0.355
Transport, storage and communication; Financial intermediation	0.386	0.487	0.386	0.487	0.055	0.229
Real estate, renting and business activities	0.232	0.422	0.232	0.422	0.217	0.413
Education; Health and social work	0.057	0.232	0.057	0.232	0.028	0.164
Copenhagen	0.020	0.142	0.020	0.141	0.074	0.262
Frederiksberg	0.363	0.481	0.363	0.481	0.382	0.487
Helsingør	0.088	0.283	0.088	0.283	0.069	0.254
Roskilde	0.069	0.254	0.069	0.254	0.065	0.246
Odense	0.066	0.248	0.066	0.248	0.041	0.200
Kolding	0.053	0.225	0.053	0.225	0.074	0.262
Fredericia	0.097	0.296	0.097	0.296	0.078	0.269
Aarhus	0.172	0.378	0.172	0.378	0.157	0.364
Aalborg	0.071	0.257	0.071	0.257	0.060	0.238
# obs.	193,037		192,820		217	

Manufacturing I: Mining and quarrying of energy producing materials; food products; beverages and tobacco; textiles and textile products; leather and leather products

Manufacturing II: Manufacture of wood and wood products; pulp, paper and paper products; publishing and printing; coke, refined petroleum products and nuclear fuel; chemicals, chemical products and man-made fibres; rubber and plastic products; other non-metallic mineral products; basic metals and fabricated metal products; machinery and equipment n.e.c.

Manufacturing III: Manufacture of electrical and optical equipment; transport equipment; other manufacturing

Innovative firms prior to treatment

	All		Untreated		Treated	
Patent stock at *t*	0.977	5.863	0.812	4.997	5.420	16.528
Number of patents per year	0.648	3.993	0.532	3.537	3.784	10.023
Number of employees	113.905	382.668	90.876	236.667	736.137	1,493.350
Value added	92,903	472,669	66,802	212,909	769,456	2,109,031
Patent stock at *t-1*	1.095	6.860	0.934	5.262	5.451	23.695
Positive patent stock at *t-1*	1.000	0.000	1.000	0.000	1.000	0.000
Firm age	19.441	23.250	18.797	21.990	36.843	42.443
Stock listed	0.735	0.442	0.728	0.445	0.922	0.272
Private company	0.258	0.437	0.266	0.442	0.020	0.140
Time until leakage	35.016	1.930	35.005	1.829	35.314	3.760
Patent effectiveness	4.527	0.182	4.528	0.169	4.479	0.397
Sales concentration	0.176	0.211	0.168	0.204	0.387	0.289
Manufacturing I	0.031	0.173	0.030	0.170	0.059	0.238
Manufacturing II	0.220	0.415	0.216	0.412	0.333	0.476
Manufacturing III	0.088	0.284	0.081	0.272	0.294	0.460
Electricity, gas and water supply; Construction	0.031	0.173	0.032	0.176	0.000	0.000
Wholesale and retail trade; Repair; Hotels and restaurants	0.114	0.318	0.117	0.321	0.039	0.196
Transport, storage and communication; Financial intermediation	0.115	0.320	0.118	0.322	0.059	0.238
Real estate, renting and business activities	0.323	0.468	0.327	0.469	0.216	0.415
Education; Health and social work	0.078	0.268	0.081	0.272	0.000	0.000
Copenhagen	0.010	0.099	0.009	0.093	0.039	0.196
Frederiksberg	0.373	0.484	0.371	0.483	0.431	0.500
Helsingør	0.140	0.347	0.142	0.349	0.098	0.300
Roskilde	0.059	0.237	0.059	0.235	0.078	0.272
Odense	0.067	0.250	0.067	0.251	0.059	0.238
Kolding	0.052	0.222	0.052	0.221	0.059	0.238
Fredericia	0.098	0.297	0.101	0.301	0.020	0.140
Aarhus	0.141	0.349	0.141	0.348	0.157	0.367
Aalborg	0.059	0.237	0.060	0.237	0.059	0.238
# obs.	1,429		1,378		51	

Large firms prior to treatment

	All		Untreated		Treated	
Patent stock at *t*	0.029	1.056	0.229	0.563	1.547	9.073
Number of patents per year	0.021	0.719	0.115	0.461	1.094	5.567
Number of employees	52.404	265.119	1.690	2.055	694.083	1,505.784
Value added	48,395	489,447	7,250	46,465	635,537	2,065,179
Patent stock at *t-1*	0.030	1.232	0.379	0.754	1.507	12.765
Positive patent stock at *t-1*	0.018	0.133	1.000	0.000	0.244	0.431
Firm age	19.426	22.887	11.437	11.894	39.394	37.440
Stock listed	0.696	0.460	0.545	0.498	0.939	0.240
Private company	0.254	0.435	0.451	0.498	0.022	0.148
Time until leakage	34.808	2.280	35.159	1.455	35.017	3.854
Patent effectiveness	4.534	0.221	4.558	0.152	4.461	0.390
Sales concentration	0.093	0.143	0.139	0.183	0.330	0.292
Manufacturing I	0.077	0.266	0.010	0.098	0.072	0.260
Manufacturing II	0.112	0.315	0.133	0.340	0.328	0.471
Manufacturing III	0.033	0.179	0.055	0.229	0.161	0.369
Electricity, gas and water supply; Construction	0.091	0.288	0.019	0.138	0.044	0.207
Wholesale and retail trade;	0.172	0.378	0.094	0.292	0.150	0.358
Repair; Hotels and restaurants	0.259	0.438	0.170	0.376	0.033	0.180
Transport, storage and communication;						
Financial intermediation	0.198	0.399	0.430	0.496	0.200	0.401
Real estate, renting and business activities	0.057	0.233	0.088	0.283	0.011	0.105
Education; Health and social work						
Copenhagen	0.021	0.142	0.021	0.144	0.061	0.240
Frederiksberg	0.360	0.480	0.317	0.466	0.378	0.486
Helsingør	0.076	0.264	0.174	0.379	0.078	0.269
Roskilde	0.066	0.248	0.042	0.201	0.056	0.230
Odense	0.065	0.247	0.054	0.225	0.044	0.207
Kolding	0.057	0.232	0.041	0.197	0.078	0.269
Fredericia	0.110	0.313	0.084	0.278	0.083	0.277
Aarhus	0.169	0.375	0.183	0.387	0.156	0.363
Aalborg	0.076	0.265	0.084	0.278	0.067	0.250
# obs.	45,112		44,932		180	

Appendix B: descriptive statistics of all variables involved in the estimations

	Mean	Std. dev.	10%	25%	50%	75%	90%	# obs.
Manufacturing I	21.0	145.4	1	3	4	9	27	10,389
Manufacturing II	37.5	169.1	2	5	10	26	67	8,246
Manufacturing III	33.9	143.1	1	3	8	21	67	2,886
Electricity, gas and water supply; Construction	24.2	140.5	1	3	6	15	35	9,169
Wholesale and retail trade; Repair; Hotels and restaurants	19.4	84.9	1	2	6	14	35	18,819
Transport, storage and communication; Financial intermediation	17.8	222.5	1	2	3	9	24	37,106
Real estate, renting and business activities	16.8	92.9	1	2	3	9	25	29,480
Education; Health and social work	18.7	71.7	1	3	5	12	34	6,554

Manufacturing I: Mining and quarrying of energy producing materials; food products; beverages and tobacco; textiles and textile products; leather and leather products.

Manufacturing II: Manufacture of wood and wood products; pulp, paper and paper products; publishing and printing; coke, refined petroleum products and nuclear fuel; chemicals, chemical products and man-made fibres; rubber and plastic products; other non-metallic mineral products; basic metals and fabricated metal products; machinery and equipment n.e.c.

Manufacturing III: Manufacture of electrical and optical equipment; transport equipment; other manufacturing.

Appendix C: standardized biases before and after matching

		Mean treated	Mean control	Bias (in perc.)	p–val.
ln(patent stock)$_{t-1}$	Unmatched	-0.406	-0.037	-23.2	0.000
	Matched	-0.406	-0.489	5.3	0.711
ln(patent stock)$^2_{t-1}$	Unmatched	4.741	0.435	24.0	0.000
	Matched	4.741	6.591	-10.3	0.404
Dummy for patent at $t-1$	Unmatched	0.235	0.007	74.4	0.000
	Matched	0.235	0.277	-13.5	0.323
Firm age	Unmatched	35.687	12.892	81.1	0.000
	Matched	35.687	37.124	-5.1	0.688
Stock listed	Unmatched	0.908	0.365	136.7	0.000
	Matched	0.908	0.885	5.8	0.432
Private company	Unmatched	0.055	0.609	-145.2	0.000
	Matched	0.055	0.074	-4.8	0.436
Time until leakage	Unmatched	35.060	34.915	5.4	0.122
	Matched	35.060	35.014	1.7	0.893
Patent effectiveness	Unmatched	4.477	4.549	-25.9	0.000
	Matched	4.477	4.492	-5.4	0.665
ln(# empl.)	Unmatched	4.600	1.581	179.1	0.000
	Matched	4.600	4.619	-1.2	0.925
ln(# empl.)^2	Unmatched	25.578	3.741	153.4	0.000
	Matched	25.578	26.327	-5.3	0.704
Sector dummies					
Sales concentration	Unmatched	0.303	0.068	108.2	0.000
	Matched	0.303	0.289	6.6	0.591
Agriculture, fishing, quarrying	Unmatched	0.060	0.090	-11.5	0.121
	Matched	0.060	0.055	1.8	0.837
Manufacturing	Unmatched	0.290	0.047	68.7	0.000
	Matched	0.290	0.281	2.6	0.832
Electricity, gas and water supply	Unmatched	0.157	0.017	51.3	0.000
	Matched	0.157	0.120	13.5	0.267
Construction	Unmatched	0.046	0.057	-5.1	0.475
	Matched	0.046	0.060	-6.2	0.521
Wholesale & retail trade; hotels, restaurants	Unmatched	0.147	0.115	9.8	0.128
	Matched	0.147	0.171	-6.8	0.513
Transport, post and telecommunications	Unmatched	0.055	0.386	-86.9	0.000
	Matched	0.055	0.060	-1.2	0.837
Public and personal services	Unmatched	0.028	0.057	-14.6	0.062
	Matched	0.028	0.051	-11.5	0.217

		Mean treated	Mean control	Bias (in perc.)	p–val.
Region dummies					
Copenhagen	Unmatched	0.074	0.020	25.3	0.000
	Matched	0.074	0.060	6.6	0.565
Northern Sealand	Unmatched	0.069	0.088	-6.9	0.334
	Matched	0.069	0.060	3.4	0.697
Western Sealand	Unmatched	0.065	0.069	-1.9	0.782
	Matched	0.065	0.097	-12.9	0.218
Funen	Unmatched	0.041	0.066	-10.8	0.149
	Matched	0.041	0.060	-8.2	0.383
Southern Jutland	Unmatched	0.074	0.053	8.4	0.180
	Matched	0.074	0.046	11.3	0.226
Mid Jutland	Unmatched	0.078	0.097	-6.6	0.353
	Matched	0.078	0.092	-4.9	0.607
Aarhus area	Unmatched	0.157	0.172	-4.2	0.543
	Matched	0.157	0.129	7.5	0.412
Northern Jutland	Unmatched	0.060	0.071	-4.5	0.524
	Matched	0.060	0.055	1.9	0.837
Year dummies					
Year 1995	Unmatched	0.078	0.093	-5.2	0.458
	Matched	0.078	0.092	-4.9	0.607
Year 1996	Unmatched	0.106	0.094	4.1	0.530
	Matched	0.106	0.078	9.2	0.321
Year 1997	Unmatched	0.189	0.095	27.2	0.000
	Matched	0.189	0.157	9.3	0.375
Year 1998	Unmatched	0.106	0.100	1.8	0.786
	Matched	0.106	0.074	10.6	0.241
Year 1999	Unmatched	0.253	0.110	37.7	0.000
	Matched	0.253	0.272	-4.9	0.664
Year 2000	Unmatched	0.074	0.126	-17.4	0.021
	Matched	0.074	0.111	-12.3	0.185
Year 2001	Unmatched	0.051	0.140	-30.6	0.000
	Matched	0.051	0.065	-4.8	0.538
Year 2002	Unmatched	0.060	0.150	-29.7	0.000
	Matched	0.060	0.060	0.0	1.000

Appendix C displays descriptive statistics of our explanatory variables before and after matching. The p–values correspond to tests for identity in means of the respective variables.